PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

SIR JOHN Y. W. MACALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE THIRTEENTH

SESSION 1919-20

PARTS I & II

GENERAL REPORTS

SECTIONS:—

ANÆSTHETICS BALNEOLOGY AND CLIMATOLOGY
STUDY OF DISEASE IN CHILDREN CLINICAL DERMATOLOGY
ELECTRO-THERAPEUTICS EPIDEMIOLOGY AND STATE MEDICINE
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The Royal Society of Medicine.

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Egyptian Bilharziasis: Its Recent Pathology, Symptomatology and Treatment.[1]

By N. HAMILTON FAIRLEY, M.D.Melb., O.B.E., Lieutenant-Colonel(Temp.) A.A.M.C.

Senior Physician, formerly Pathologist, Fourteenth Australian General Hospital.

BILHARZIA hæmatobia and *Bilharzia Mansoni* are the two species of schistosomes producing bilharziasis in Egypt. The importance of the high endemic index of these diseases in the Nile valley has been emphasized by Ferguson [1], who has had unique opportunities of estimating at autopsy the ravages caused by these diseases. He reckons that at least 61 per cent. of the fellaheen are infested with one or other or both of these parasites. The death-rate, too, is high. Madden [2] asserts that 10 per cent. of the fellaheen infested die as a direct result of the disease and its complications.

In 1915-16 the controversy between Looss [3] and Sambon regarding the duality of species of these two parasites was finally ended by the brilliant work of Leiper, Thomson and Cockin, who formed the special mission dispatched by the the War Office to Egypt to investigate this subject.

It has now been proved that the terminal spined ovum of *Bilharzia hæmatobia*, on reaching water, hatches out a ciliated miracidium which has forty-eight hours in which to reach its *intermediary* host—certain species of fresh water snail of the genus *Bullinus* sp. *contortus* or *Dybowski*. The miracidium invades the soft parts of the snail and reaches the liver or digestive gland: here it produces sporocysts which

[1] At a meeting of the Society, held October 31, 1919.

bud daughter sporocysts that produce bifid-tailed non-eyed cercariæ containing no muscular pharynx. These cercariæ are periodically discharged into water, and in turn have forty-eight hours in which to find their *definitive* host-man. As a general rule they invade man by piercing the skin while bathing: but it is probable that occasionally infection occurs through the pharynx and œsophagus. The cercariæ ultimately reach the liver, where they attain sexual maturity, mate, and later, after migrating against the portal blood stream, deposit terminal spined ova in the venules of certain viscera, especially the bladder.

A similar life cycle holds for *Bilharzia Mansoni*, which however has a different morphology, a different *intermediary* host, *Planorbis Boissyi* and a lateral, not a terminal, spined egg. This species deposits its ova especially in the colon and rectum.

The Early Clinical Appearance of Bilharziasis in Man.

Elsewhere [4] I have suggested that from the clinical viewpoint the manifestations of bilharziasis amongst our troops in Egypt could frequently be divided into two distinct stages:—

First: A toxæmic stage occurring four to ten weeks after infestation.

Secondly: A much later stage of *localized* bilharziasis characterized in the case of *Bilharzia hæmatobia* by vesical, and of *Bilharzia Mansoni* by ntestinal symptoms.

(I) *Toxæmic Symptoms of Bilharziasis.*

The earliest symptoms associated with bilharziasis were first accurately described for the Japanese species of infection—*Bilharzia japonica*. Flu [5], in Surinam, first called attention to the appearance of febrile symptoms resembling Katayama disease in patients with lateral spined ova in the fæces. Archibald [6] in 1914 described febrile manifestations in *Bilharzia Mansoni* unassociated with eosinophilia, and suggested it was due to a secondary infection.

In 1916 Lawton [7] reported an outbreak of bilharziasis amongst Australian troops stationed at Tel-el-Kebir, on the canal zone in Egypt. The following symptoms were noted with great frequency: pyrexia, bronchitis, urticaria, diarrhœa, abdominal pain, enlargement and tenderness of the liver and spleen. A high eosinophilia was frequently noted, and the fæces were found to contain the lateral spined ova of

Bilharzia Mansoni. The incubation period varied, often being from four to eight weeks, and the onset was often insidious. The duration of fever was from ten days to eight weeks. Loss of weight and emaciation were characteristic features.

During the past three years I had the opportunity of observing seventy-five cases of bilharzial infestation amongst Australian troops. Lawton's original observations were abundantly confirmed and amplified. The toxæmic symptoms, however, were not found to be dependent on infestation with *Bilharzia Mansoni* alone. Such symptoms were met with most frequently in double infestations, but were also, for the first time, recorded in pure *hæmatobia* infestations from Serapeum, a section of the canal zone where only *Bullinus* species abound. An analysis of the clinical histories of these seventy cases showed the following groupings:—

(1) Twenty-three cases presented symptoms of urticaria and prolonged pyrexia.

(2) Ten cases occurred with urticaria and pyrexia of less than ten days' duration.

(3) Five cases occurred with urticaria alone.

Thus thirty-eight out of these seventy-five cases, or 50·8 per cent. of the series, manifested early toxæmic symptoms. As these features have already been recorded in detail [8] they will not be further considered except to allude to the peculiar characteristics of the urticarial eruption. Sometimes it was of a most intense type causing much irritation and a general though transient œdema of the face, and often of the penis and scrotum as well. Urticarial wheals were not infrequently noted on the trunk and limbs. The eruption was sudden in onset, and of variable duration, lasting on and off from one to ten days.

The disappearance of toxæmic symptoms was followed by a *latent period* which lasted from six weeks to three years, before obvious localized symptoms of bilharziasis developed. Ova are passed in the dejecta either continuously or from time to time throughout this latent period. Such cases constitute carriers, and are obviously especially dangerous in disseminating the disease. They are very commonly met with in *Bilharzia Mansoni* infestations.

(II) *The Stage of Localized Bilharziasis.*

Localized symptoms can be classified according to the site of the lesions and the species of the parasite infesting the patient.

Bilharzia hæmatobia Infestation.—In my experience the onset of localized *Bilharzia hæmatobia* infestation is most commonly indicated by a burning and scalding urethral pain, less frequently by perineal discomfort or penile pain. This is followed later by a *terminal* hæmaturia, the most characteristic feature of vesical bilharziasis. Hæmaturia may occur with every act of micturition, or at daily, weekly, or even monthly intervals. The blood is usually bright red in colour; rarely it may be dark and occasionally long, worm-like clots are noted. Suprapubic pain is by far the commonest type of pain met with: it is present especially when the bladder is distended, and is generally described as a dull ache which is aggravated by any increase in intra-abdominal tension such as is produced by tightening the belt, or by manual pressure during abdominal palpation. It is relieved by emptying the distended viscus. Backache and loin pain are not infrequently present. Frequency of micturition is usually an early and transient symptom in these early cases, and I would suggest that persistency of this symptom should arouse suspicion of a secondary bacterial cystitis. Even in pure *hæmatobia* infestations it is quite common to elicit a history of passage of blood and mucus *per rectum*, and terminal spined ova are recovered from the fæces. Rectal examination not infrequently reveals localized tenderness above the prostatic gland between the *vesiculæ seminales*. In my experience enlargement and tenderness of the prostate is distinctly rare, though Madden [9] regards it as an early sign of some diagnostic importance.

Cystoscopic examination may show venous congestion and injection of vessels, and very frequently the presence of whitish or yellowish submucous tubercles or nodules elevated above the level of the mucosa. Such pseudo-tubercles may be widespread over the base, fundus and lateral vesical walls, or may be limited to one anatomical section of the viscus. In size they vary from 1 mm. to 5 mm. in diameter, and are the earliest lesions of the disease. Their pathology will be discussed in more detail later. Ulceration is not a frequent lesion in early bilharziasis, but in two cases cystoscoped by Captain R. T. Silverton, A.A.M.C., ulceration of the trigone was present.

Involvement of the ureteric orifice is not infrequent even in these early cases, and herein lies one of the gravest dangers of *Bilharzia hæmatobia* infestation. The orifice of the ureter may be golf hole in type, patulous at rest, yawning and circular during the emission of blood-stained urine. Catheterization of the ureter and collargol injection may reveal an early hydronephrosis even in the absence of any clinical

features suggestive of such a condition. This tendency to involvement of the lower portion of the ureter early in urinary bilharziasis has in the past been insufficiently appreciated. It is dependent on the common nature of the vascular supply of this portion of the ureter and of the bladder. The *urine* does not always yield macroscopic evidence of mucus and blood, and small amounts of albumin may or may not be detected.

In conducting the examination for ova the last 10 c.c. of urine passed should be selected, and the centrifugalized deposit examined with low power magnification. Pus cells constantly accompany the presence of ova and a variable number of red blood corpuscles are present. The vicarious manner in which ova appear in the excreta needs emphasis. For this reason a single negative examination of the dejecta does not exclude the disease.

The *blood picture* is characteristic of helminthic infections in general. There is a moderate leucocytosis, the average, in thirty-five cases of my series, being 10,030 per cubic millimetre; the lowest was 4,700 per cubic millimetre, the highest 22,000 per cubic millimetre. The differential count showed an average eosinophilia of 13·5 per cent. The tendency is for the eosinophilia to be most marked in the early months of the disease, 50 per cent. (Lawton), and to decrease during the subsequent two years to 11 per cent. or thereabouts. There is generally a slight secondary anæmia. In late cases with septic complications Day [10] has shown an increased leucocytosis, accompanied by a relative and absolute increase in the polymorphic neutrophils and a corresponding decrease in the eosinophilic elements.

As mentioned above, cystitis, involvement of the ureteric orifices and hydronephrosis of low grade type may be met with during the first three years of infestation. In addition the following complications may be seen amongst the Egyptians: septic cystitis, pyonephrosis or ascending pyelonephritis, interstitial nephritis, urinary calculus, vesical carcinoma or sarcoma, prostatitis and infiltration of the vesiculæ seminales, periurethral fistulæ, and bilharziasis of the penis. The comparatively high death-rate amongst these unfortunate people is occasioned by three complications—sepsis, renal insufficiency, the result of back pressure or infection, and carcinoma supervening on the chronic irritation produced by ova in the bladder walls. The most common form of cancer in Egypt has been shown by Ferguson [11] to be vesical carcinoma, and it is here interesting to note that in the vesical bilharziasis of monkeys most extraordinary activity, proliferation and downgrowth of epithelium

may be found in the papillomata produced by the toxic products of ova and worms *within three months* of infestation.

The Localized Symptoms in Bilharzia Mansoni Infestation.—The most remarkable feature of the localized stage of *Bilharzia Mansoni* disease is the latency, insidiousness and symptomless character of many of the cases. For this reason I am convinced there is no intestinal infection which is more likely to remain undiagnosed over long periods of time. Thus in my series of thirty-eight cases eleven failed to give any history whatsoever of intestinal symptoms, though lateral-spined ova were readily demonstrated in their fæces. In the remaining cases the patients themselves had not attached much significance to their intestinal symptoms, which were characterized from time to time by the appearance of blood or mucus in the stools, occasional attacks of diarrhœa or rectal uneasiness. A few cases gave a history of intermittent dysenteric attacks with tenesmus, which sometimes lasted two or three weeks and were then accompanied by emaciation. A history of colicky abdominal pain was infrequent. Dyspeptic symptoms were not noted, nor was the liver enlarged to physical examination. Microscopic examination of the mucous coating of the stool revealed the presence of lateral-spined ova in these cases, and in the case of four patients they were demonstrated in the urine as well. Tenderness along the course of the colon was occasionally present, but the local thickenings and tumour-like polypoid masses which occur in Egyptians were never met with. Hepatic periportal cirrhosis could never be clinically demonstrated. Digital examination of the rectum revealed no features of note, but sigmoidoscopy in one case showed small, multiple, diffusely scattered tubercles somewhat analogous to the vesical lesions described. Besides noting early changes Madden [12] has shown the frequency with which sigmoidoscopy reveals the presence in chronic Egyptian bilharziasis of a papillomatosis of the rectal and colonic mucosa.

The Complement-deviation Reaction in Bilharziasis.

The frequency with which urticaria and fever were appearing in the early stages of *Bilharzia Mansoni* and *Bilharzia hæmatobia* infestations led me to presume that worms or ova must be producing this effect by the production of some circulating toxic substance. Similar manifestations—presumably toxic—are known to occur in other helminthic infections such as Katayama disease and trichiniasis, while we are quite familiar in Australia with the profuse urticarial eruption that follows the intraperitoneal rupture of hydatid cysts (*Tænia echinococcus*).

For the purpose of testing this *toxæmic theory* I employed a modification of the complement-deviation method first described by Bordet and Gengou [13]. In the first place an extract was made from bilharzia worms obtained at autopsy from an Egyptian. Preliminary experimentation showed it possessed true antigenetic properties, but owing to its difficult manufacture the method was discarded. A saline extract [14] of the livers of infected snails of the species *Planorbis Boissyi* was then prepared. This antigen yielded satisfactory results, but was later discarded in favour of an alcoholic saline soluble extract, which, while showing no tendency to produce pseudopositive reactions, had the additional advantage of being more stable. By utilizing this antigen and employing a quantitative complement-deviation technique the following results were obtained in 322 consecutive examinations of fresh human serum :—

(1) In a group of thirty-six cases in which bilharziasis was under two years' duration thirty-two, or 88·8 per cent., yielded positive reactions.

(2) In a group of ninety-seven more chronic cases seventy-two, or 74·2 per cent., yielded positive reactions.

(3) In forty-four cases of syphilis yielding positive Wassermann reactions, negative results were always registered.

(In one case a positive reaction was obtained, but here there was an anti-complementary tendency shown in the serum control tube.)

(4) In 150 other cases, which included protozoal, metazoal and bacterial infections, negative results were uniformly obtained. In only one case was a positive result registered where ova were not found in the dejecta, but as only one examination was made and as intestinal symptoms were present bilharzia could not be excluded. Probably the case was one of latent infection.

It is interesting to note that, as a general rule in bilharzia infections, the higher the eosinophilia the greater the amount of complement fixed. There are, however, exceptions, and a proportion of cases without eosinophilia yield positive reactions. It is important to emphasize that this serological phenomenon is of the nature of a group reaction, and does not enable one to differentiate the species of the parasite—i.e., whether *Bilharzia hæmatobia* or *Bilharzia Mansoni*.

The serological response was next investigated in experimentally infected monkeys.

First, it was found that monkeys could be killed by hyperinfection before there was time for ova to be deposited in their tissues. Toxic

stigmata were present. Microscopic examination showed cloudy swellings of the parenchyma of organs and peripheral infiltration of the lobules of the liver with round cells. The cellulo-humoral response was minimal as a general rule. The animals died either before there was time for an immunity response to develop, or, if they survived longer, the intensity of the toxæmia often caused a complete depression of the mechanism of immunization. Thus the complement deviation reaction was generally negative; there was, as a rule, absence of eosinophilia, and not infrequently an actual leucopænia.

Secondly, entirely different findings were observed in monkeys spontaneously recovering from the disease. Thus in ten monkeys recovering from the initial toxic effects of bilharzial infection a remarkable cellulo-humoral response was present in every case. The *leucocytosis* during the sixth to the twelfth week varied from 13,000 to 34,000 per cubic millimetre. The *eosinophilia* varied from 10 per cent. to 63·2 per cent., and the average over this period equalled 28 per cent. In every case a strongly *positive complement-deviation* reaction developed. The bone marrow showed a marked increase in eosinophil myelocytes, and also a cellular proliferation and increase in the normoblastic elements.

Such findings proved that the toxæmic theory of bilharziasis was in accord with experimentally established facts, and strongly suggested that the immune body produced was framed to antagonize bilharzia toxic products.

The Early Pathological Changes in Infected Monkeys.

As no human cases were fatal the pathological basis of the toxæmic symptoms could not be observed in man. A detailed study of the organs of monkeys infected with *Bilharzia Mansoni* and *Bilharzia hæmatobia* from the sixth to the twelfth week was therefore made. The conclusion reached was that *there was no essential difference in the type of the lesion produced by either species of schistosome.* In both cases there were produced widely-disseminated round whitish nodules which, microscopically, were found to be composed of centrally placed ova surrounded by multinucleated giant cells, eosinophil polymorphs and small mononucleated cells. Central degeneration may occur in such foci. The distribution of these nodules varied with the type of infection and was dependent on the anatomical habitat of the two species of worms.

In *Bilharzia Mansoni* infestation the chief features were as follows: On laparotomy a congested liver was demonstrated with disseminated whitish tubercles (1 mm. in diameter) on the surface and scattered throughout the substance. The spleen was enlarged and the colon showed pericolitis with adhesions, subperitoneal nodules, congestion of the mucosa with submucous infiltrations composed mainly of eosinophil and mononucleated cells. The adult worms of this species (*Bilharzia Mansoni*) were found in the superior and inferior mesenteric veins and in the portal radicles of the liver. Macroscopic lesions of the bladder and lung were quite rare, though ova were found to be widely disseminated by utilizing Ferguson's method of digesting the tissues with a 3 per cent. solution of caustic soda, centrifugalizing and examining microscopically the sediment obtained.

In *Bilharzia hæmatobia* infestations similar microscopic lesions were produced in the liver and intestine, but in addition the pelvic organs, such as the *bladder* and *uterus*, showed vesical papillomata and greyish nodules scattered throughout the latter. In the lungs there were similar accumulations resembling closely those of a miliary tuberculosis. The adult worms in *Bilharzia hæmatobia*, although plentiful in the mesenteric and portal veins, predominated in the pelvic plexuses, as in the vesical and the uterine. They also migrated via the inferior vena cava to the pulmonary capillaries, where they could be demonstrated *in situ*.

Thus we find that the study of these lesions affords the explanation of the hepatic splenic and pulmonary signs and symptoms seen in the early toxæmic stage of bilharziasis in man. Similarly the early nodule or bilharzial tubercle seen in the bladder and rectum in early human cases is identical with this most characteristic lesion of experimental bilharziasis.

The Mode of Deposition of Ova in the Tissues.

By examining fresh pieces of infected bowel pressed between glass slides it was possible to observe microscopically the deposition of ova in the vessels, the various stages in the egress of ova through the vessel wall and the manner in which the spine assisted in this process. Female worms were also observed in the mesenteric veins of anæsthetized monkeys separated from their male partners. The actual process is as follows:—

When the time for oviposition arrives the paired worms travel

against the direction of the blood stream to the furthermost possible point. Hence the female leaves the male partner and being of a much narrower calibre, works her way into veins of small diameter, distending them in the process and only stopping when the mechanical resistance is too great for further progress. Here she deposits an ovum always in a characteristic fashion, the spine being directed posteriorly. The reason for this is that the ovum lies in a similar position in the uterus of the female itself. The female then slightly withdraws. *Pari passu* the distended vessel wall contracts down on the ovum. Again the female deposits an egg and withdraws, and again the vessel wall contracts down on the foreign body. In *Bilharzia hæmatobia* infestation many eggs are deposited in this fashion, so that microscopically the venule, with its contained ova, presents the appearance of a string of sausages. Probably local vascular spasm is a big factor in the production of this picture. When the female finally withdraws from the branched venule the blood current, which has in the meantime been held up, assumes its normal course, driving the ova before it. The spines which point in the direction of the blood stream, and are in close apposition with the walls of the venule, naturally pierce the latter structure and are then driven into the perivascular tissue.

Hereafter the progress of the ovum through the tissues is independent of the spine and is dependent on an active inflammatory process induced by a toxic substance secreted by the contained miracidium. This toxic substance exerts a positive chemiotactic influence on eosinophil leucocytes, which enable the ovum to ulcerate through the tissues in a very characteristic fashion. Ova might be described as reaching the lumen of the hollow viscera by the pointing and discharge of these small eosinophilic abscesses.

It must be remembered, however, that many ova never reach the exterior, some being destroyed by cellular and presumably humoral agencies also, while others are localized *in situ* by fibroblastic tissue reaction, later undergoing calcification. Indeed my observations lead to the conviction that the vast majority of ova gain the lumina of the hollow viscera within a few weeks of being deposited in the tissues, and that after this period the chances of the contained miracidia perishing in the tissues of the host are very great.

The Treatment of Bilharziasis.

The fact that *Bilharzia hæmatobia* and *Bilharzia Mansoni* inhabit the vascular system only and are incapable of living elsewhere in the

tissues of the host, strongly indicates that the intravenous route for the administration of any drug will yield the best results.

Several different observers have appreciated this fact and a number of drugs, including perchloride of mercury, salvarsan and eusol, have been given intravenously in bilharziasis. The only known remedy of importance, however, is tartar emetic. This drug was apparently first used in the treatment of bilharziasis by McDonagh, but Christopherson [15] quite independently proved its value, and to him we are indebted for the demonstration on a large scale of the beneficial results following its administration. Since his original publication a number of competent workers, including Newlove, Innes [16], Low [17], and Newham, have reported favourably on tartar emetic treatment. Christopherson claims that tartar emetic not only kills the parasites *Bilharzia Mansoni* and *Bilharzia hæmatobia, in situ,* but later kills the miracidia in the ova deposited in the tissues of man.

The indices of cure adopted by these different observers have been as follows: First, improvement in the general health of the patient and disappearance of vesical symptoms. Secondly, the disappearance of ova in the excreta or the passage of ova containing only dead miracidia. Observations on the eosinophilia have also been made and observers appear agreed that during treatment a rise in the eosinophilia ensues.

In order to obtain additional data as to the value of the treatment a critical investigation was made in the following three cases in May, June and July of this year, the treatment being controlled not only by the above mentioned observations, but in addition by investigations of the cystoscopical appearances of the bladder, and of the complement-deviation reactions before and after treatment.

Treatment employed.—The most severe case received a total dosage of 50 gr. of tartar emetic, the other two cases received 30 gr. each. The initial dosage employed was ½ gr. dissolved in 10 c.c. of saline solution (0·85 per cent.), and thereafter the dose was increased by amounts of ½ gr. until the physiological limit of tolerance was reached. This point was estimated by the amount of the drug, which produced paroxysmal coughing and vomiting immediately after injection; it was decided to give slightly smaller doses than this. The amount of each single injection varied from 2 gr. to 3 gr. in different cases, and was administered every second day, at a period of at least two hours after the last meal.

Effect on the General Condition of the Patient.—During treatment all the patients lost considerable weight (7 lb. to 14 lb.); they did not

feel well and were easily tired. One case had an occasional evening temperature. In all cases, however, after the cessation of treatment, the general condition rapidly improved and normal weight was established. No *local* effect follows the intravenous injection unless the fluid escapes into the perivenous tissues, when a severe local inflammatory reaction may supervene. In one case only was such a result observed. Local induration, swelling and tenderness were present, but this condition subsided in four days.

Case A.—The patient gave a history of bathing at Serapeum in the fresh-water canal (Egypt) in May, 1916. Two months later he developed a severe urticaria lasting for one week. Ten months later, in March, 1917, he noticed urethral irritation followed subsequently by terminal hæmaturia. Subsequent investigation showed terminal spinal ova of *Bilharzia hæmotobia*, in the urine and fæces. He had an eosinophilia of 28 per cent. The complement-deviation test was strongly positive (+ + +). Cystoscopy showed numerous yellowish submucous vesical nodules of 1 to 3 mm. diameter. Several ulcers with ragged edges and sloughing bases were situated on the trigone. The effect of treatment is indicated in the following table :—

Date	Tartar emetic	Eosinophilia	Ova in urine	Remarks
1919		Percentage		
April 9 ...	½ grain	29·0	+ for ova	
,, 11 ...	1 ,,	33·5	+ ,,	
,, 13 ...	1½ grains	35·6	+ ,,	
,, 15 ...	2 ,,	26·0	+ ,,	
,, 17 ...	2 ,,	34·6	+ ,,	
,, 19 ...	*Nil.*	23·0	+ ,,	Attack of renal colic, therefore treatment was suspended
,, 21 ...	*Nil.*	31·25	+ ,,	
,, 24 ...	2 grains	33·0	— ,,	
,, 27 ...	2 ,,	34·75	— ,,	
,, 29 ...	2 ,,	41·2	— ,,	
May 1 ...	2 ,,	32·75	— ,,	
,, 3 ...	2 ,,	42·5	— ,,	
,, 5 ...	2 ,,	36·0	— ,,	
,, 7 ...	2½ ,,	33·0	— ,,	
,, 9 ...	2½ ,,	37·4	— ,,	
,, 11 ...	3 ,,	45·25	— ,,	
,, 13 ...	3 ,,	53·8	— ,,	
,, 15 ...	3 ,,	47·5	— ,,	
,, 19 ...	2½ ,,	52·0	— ,,	
,, 21 ...	2½ ,,	42·75	— ,,	
Total ...	38 grains			

Treatment was now suspended. The complement-deviation reaction was still definitely positive (+ +) though less strong than before treatment. The cystoscopic examination showed healing of ulcers in the trigone. The

eosinophilia varied during the next fourteen days between 35 per cent. and 43·3 per cent. These results were not considered satisfactory and treatment was resumed :—

Date	Tartar emetic	Eosinophilia	Urine	Remarks
1919				
June 12 ...	2 grains	27·6 per cent.	Negative for ova	
,, 14 ...	2½ ,,	Not examined	,, ,,	
,, 16 ...	2½ ,,	Not examined	,, ,,	
,, 18 ...	2½ ,,	45·3 per cent.	,, ,,	
,, 20 ...	2½ ,,	Not examined	,, ,,	
	50 grains Total dosage			

On June 25, 1919, the urine still contained no ova, and the complement-fixation reaction was positive (+), the eosinophilia equalled 23 per cent. Cystoscopical examination showed that the ulcers on the trigone were now healed, but that the yellowish submucous nodules were still present. Vesical symptoms which had early disappeared never returned.

Case B.—This patient gave a history of having contracted infestation by watering horses at Serapeum in March, 1916. He first noted symptoms on March 9, 1919, *three years later*. Urethral irritation then occurred and was followed by terminal hæmaturia. He also complained of pain in the left loin

Date	Tartar emetic	Eosinophilia	Ova	Remarks
1919				
May 9 ...	½ grain	Not examined	+	
,, 11 ...	1 ,,	19·3 per cent.	+	
,, 13 ...	2 grains	Not examined	+	
,, 15 ...	2 ,,	22·0 per cent.	+	
,, 19 ...	2½ ,,	Not examined	–	
,, 21 ...	2 ,,	19·0 per cent.	–	
,, 26 ...	2½ ,,	18·5 ,,	–	
,, 28 ...	2½ ,,	Not examined	–	
,, 30 ...	2½ ,,	18·0 per cent.	–	
June 1 ...	2½ ,,	Not examined	–	
,, 3 ...	2½ ,,	14·3 per cent.	–	
,, 5 ...	2½ ,,	Not examined	–	Patient developed right epididymitis and orchitis
,, 7 ...	3 ,,	,, ,,	–	
,, 8 ..	2 ,,	17·3 per cent.	–	
Total ...	30 grains			

radiating to the left testicle. A slight purulent urethral discharge was present but gonococci were never demonstrated. Urine contained terminal spined ova of *Bilharzia hæmatobia*. The eosinophilia equalled 18 per cent. The complement-deviation reaction was definitely positive (++). Cystoscopic

examination showed the lateral walls and roof of the bladder to be studded with yellowish submucous nodules. The orifice of the ureter (left) was of the golf hole type. The orifice was patulous at rest, yawning and circular during the emission of blood-stained urine. Catheterization of the left ureter showed that the pelvis of the kidney would easily contain 80 c.c. of collargol.

Sixteen days later the patient had much improved, and there had never been a relapse of vesical symptoms. The urine was still negative, the eosinophilia was 13 per cent., and the complement-fixation reaction registered positive only. The cystoscopic picture showed an apparent diminution in size of the submucous nodules, but the left ureteric orifice was still golf hole in type.

Case C.—Source of infestation doubtful. Patient had been stationed in Egypt for twenty-one months. Terminal hæmaturia had been noted for the past five weeks, since April 1, 1919. Urine showed terminal spined ova, pus and red blood corpuscles. The eosinophilia was 7 per cent. Cystoscopic examination shows a small scanty scattering of yellowish submucous nodules in the submucosa. The complement-fixation reaction was positive (+ ±).

Date	Tartar emetic	Eosinophilia	Urine	Remarks
1919				
May 11 ...	½ grain	7·4 per cent.	+ ova	
,, 13 ...	1 ,,	Not examined	+ ,,	
,, 15 ...	1½ grains	7 per cent.	+ ,,	
,, 19 ...	1½ ,,	*Nil*	+ ,,	
,, 21 ...	2 ,,	8 per cent.	+ ,,	
,, 26 ...	2½ ,,	8 ,,	—	
,, 28 ...	2 ,,	*Nil*	—	
,, 30 ...	2½ ,,	9 per cent.	—	
June 3 ...	2½ ,,	9·6 ,,	—	
,, 5 ...	2½ ,,	*Nil*	—	
,, 7 ...	3 ,,	*Nil*	—	
,, 8 ...	2 ,,	5·3 per cent.	—	
,, 10 ...	2½ ,,	*Nil*	—	
,, 12 ...	2½ ,,	*Nil*	—	
,, 14 ...	2 ,,	9·75 per cent.	—	
Total ...	30 grains			

Eleven days later the eosinophilia was 2·6 per cent. and the complement-deviation reaction was reduced to a positive (±). The cystoscopic picture was very similar to the one already described. Vesical symptoms had disappeared early and never returned.

Summary of the Results Obtained.

Examination of the Urine.—In all three cases the vesical symptoms disappeared early in the treatment and did not return. Ova disappeared

from the urine in all cases after 7 gr. to 8 gr. of tartar emetic had been administered, and did not reappear. Pus cells persisted for a longer period but finally disappeared in two out of the three cases.

Cystoscopy.—Captain R. H. Silverton, A.A.M.C., carried out cystoscopical examinations in all three cases. In two cases definite ulceration of the trigone disappeared during treatment. The characteristic yellowish-white nodules which in all three cases were scattered over the vesical mucosa were present after full treatment in every case; these lesions appeared to have slightly decreased in size. The final observations were made in one case eleven days, in another sixteen days, and in the last forty-two days after thirty grains of tartar emetic had been administered intravenously.

The Complement-deviation Reaction. — Quite apart from its diagnostic value I had suggested on the analogy of the Wassermann reaction in syphilis, that this serological test might afford an index to the therapeutic action of any drug on the bilharzia worms within the venous system of man. The following observations support that contention: In two cases, fourteen days after 30 gr. of tartar emetic had been given intravenously, the complement-deviation reaction had changed from a positive + + to a positive +, and from a positive + ± to a positive ± (a reduction of three minimum hæmolytic doses of complement in both cases). In the other case 38 gr. reduced a positive + + + reaction to a positive + +, and a further 12 gr. reduced it to a positive + (a reduction of six minimum hæmolytic doses of complement). In this last case I had obtained a positive + + + reaction six times during the preceding fourteen months, so that such a result afforded remarkably strong evidence in favour of the lethal action of the drug. Owing to the hospital embarking for Australia at this date, later observations could not be made, but it appears certain that had a little more time elapsed after cessation of treatment the complement-deviation reactions would have become completely negative. Furthermore, in bilharziasis once the serological reaction has been made negative it should remain always negative, since the life cycle of these schistosomes cannot be completed within the definitive host. On the other hand, in a disease like syphilis, where the treponema has power of reproduction, relapsing serological reactions are only too commonly observed.

Eosinophilia.—The effect of intravenous tartar emetic on the eosinophilia was interesting but more difficult to interpret. Apparently, the tendency was for the eosinophilic curve to rise during the treat-

ment and to fall after intravenous therapy was discontinued. It is necessary, however, to remember that ova as well as the worms themselves exert a positive chemiotaxis on eosinophils. Considering the number of ova imprisoned in the walls of the hollow viscera, it is hardly likely that any treatment could rapidly modify the eosinophilia.

Significance of these Observations.

(1) The disappearance of vesical symptoms and the improvement in general health following treatment proves the beneficial effect of tartar emetic in the treatment of vesical bilharziasis.

(2) The disappearance from the urine of ova containing living miracidia indicates that the female worms were either sterilized or killed by the drug. The modification of the complement fixation reaction in all the three cases investigated affords the surest evidence of the lethal action of tartar emetic on the worms themselves, and this undoubtedly was the main action of the drug in the three cases under investigation.

(3) The evidence that the drug killed the miracidia in the tissues rests on a less sure basis, but in my opinion this is a factor of little importance. There is much evidence to show that unless ova rapidly pass through the walls of the viscera cellulo-humoral and local tissue responses are quite capable of sterilizing or of actually destroying them. If this is true, then once the worms are killed and no fresh ova deposited, the definitive host would in any case in a very few weeks cease to pass ova containing living miracidia.

At any time vesical lesions are largely due to ova deposited in the walls of this viscus. The eosinophilia met with in bilharziasis is also partly dependent on ova deposited in the tissues. It would appear that a greater modification of both the cystoscopic picture and eosinophilia would have ensued if all the ova in the body of the host had been killed off.

In view of these findings I would submit that the evidence collected in these three cases entirely supports the view that tartar emetic exerts its beneficial effect in bilharziasis by killing off the adult schistosomes in the venous system.

Finally it is important to recognize that despite the large amount of information obtained regarding bilharziasis in recent years there yet remains much to be worked out.

In the life cycle of *Bilharzia hæmatobia* the difficulty in demonstrating infected snails of the genus *Bullinus* sp. *contortus* and *Dybowski* was recognized by Manson-Bahr and myself, and indeed we are inclined to the view that another molluscan intermediary exists for this schistosome.

Regarding the exact manner in which the emetic acts in bilharziasis, the estimation of the end point in treatment, and the most suitable compound of antimony to be utilized, I would emphasize that these problems can readily be solved only by further researches along the lines indicated above on a series of experimentally infected monkeys.

The production of a stable antigen is another problem requiring solution if the complement-deviation reaction is to be employed in countries other than Egypt, but I have little doubt that further investigation will enable this to be done.

The administration of antimony compounds intravenously in other helminthic infections is indicated, and at a subsequent date I hope to report on the results obtained in infection with *Tænia echinococcus* in man, controlling the results by precipitin and complement-fixation reactions.

Acknowledgements.—I would like to thank Captain R. T. Silverton, A.A.M.C., for the reports on the cystoscopical appearances in these cases, and Dr. Manson-Bahr for his very generous assistance during the researches on bilharziasis, of which this paper is an epitome.

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The Royal Society of Medicine.

President—Sir Humphry Rolleston, K.C.B., M.D.

The Position of the Medical Profession with Regard to a Scheme for National Physical Education.[1]

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I am sure no one has ever stood on this platform to address a meeting of the Royal Society of Medicine with more feelings of pride at the honour granted to him than I do at this moment. I have waited anxiously for this hour, as the subject of my paper, "The Position of the Medical Profession with Regard to a Scheme for National Physical Education," is one of burning importance to-day. I will endeavour to point out to you that if ever a subject needed the support of the medical world this—the very foundation of the Science of Preventive Medicine—ranks, in my opinion, first and foremost.

The study of physical education is not a new toy or a sudden spasm on my part, but it has been my hobby, my speciality since 1906, when I was first appointed on the staff of the Royal Naval School of Physical Training at Portsmouth. At that time I realized at once the subject was essentially one which was worthy of the full attention of the medical profession. Since 1906 the Admiralty has given me many opportunities of studying the subject theoretically and practically outside the Navy as well as in it, under varied conditions and in many countries, which include a year's course at the Central Gymnastic Institute at Stockholm. Of the several billets I have held in this connexion I feel the most interesting and important has been that to which I was appointed during the four and half years of war.

[1] At a meeting of the Society, held January 21, 1920.

I had the good fortune to serve in the flagships of Sir John Jellicoe and Sir David Beatty during the four and half years of war, when I combined with my duties of junior medical officer in the ship the position of adviser in physical training matters to the Commander-in-Chief. Hence I was kept in constant and close touch with the sports and recreations of that enormous Fleet, learning such important lessons as only this War could teach.

Although my study of this subject has not been restricted to purely Naval conditions, yet I feel I should approach it (in a paper of this nature) from a Naval point of view, making observations from time to time bearing on the national issues so closely bound up with it.

The development of physical training in the Navy started in 1902, when the Admiralty decided it was necessary to have a central headquarters school of physical training in the Navy, for the purpose of training officers and petty officers to be instructors. After full discussion and inquiry the Swedish system was chosen as being the most suitable for practical application to a large number of men and boys, and for the training of an all-round type of physique. In 1903 the services of a Mr. Broman, who was a third year graduate of the R.G.C.I. of Stockholm, were obtained. He gave his services for six weeks to introduce to the Navy the Swedish system of gymnastics. From that date until the commencement of the War, the staff officers of the headquarters school of physical training endeavoured to acquire more knowledge and to train more officers and men as instructors, in the application of the Swedish system specially adapted to our particular Service requirements.

At the commencement of hostilities in 1914 the physical training school at Portsmouth was closed, so the supply of instructors was cut off.

During the War many valuable lessons have been learned by everybody, and especially has this been the case with regard to our methods of physical training.

"Up North," in the Grand Fleet, the spectre we had to dread seriously was the possible onset of a depression due to the incessant nervous strain and ghastly monotony. There we had our men-of-war crowded to their utmost limit with virile human beings—buoyed up with the daily hope that with to-morrow would come "THE DAY." That to-morrow gradually became weeks and months and the months extended to over four years, with the ever-haunting thought that another winter was coming on. When it is realized that the mere

sight of a tree filled thousands of men with that thrill of intense joy, which we have so lately seen radiating from the faces of our children as they saw the Christmas trees a few weeks ago; when even to get mud on their boots once a month was a thing to be proud of to those whose blood called for natural exercise; and when, further, you picture yourself living in a home in which you can never get away by yourself for one moment's peace, but around you all day and night is a heaving mass of struggling humanity, huddled together in an incessant noise, on electrically lit mess-decks, with the very air you breathed pumped down to you artificially — then perhaps you can appreciate that sort of restless monotony which almost grips you by the throat. That this monotony cannot be said to have affected the morale of the sailor to any extent was due in great part to the ever watchful care and forethought of those in charge of units and squadrons in the Grand Fleet. The lessons learnt were that recreation and more recreation of a simple and attractive nature for *all* on board ship was wanted. It was always possible to keep the 10 per cent. who excelled at games, amused and energetic. They would roller-skate, box, dance or do anything to keep themselves fit and cheery, when weather and other conditions made landing on shore impossible. But it was the 90 per cent. who were not endowed with this natural ability to excel at games on whom we had to concentrate, in order to kindle a small spark of energy to shake off the dull and listless mood into which they were unconsciously drifting. Recreational committees were formed and schemes worked out to assist those ships less fortunately equipped and situated. Physical training began to be looked upon in a much bigger light, the study of amusements of a healthy and stimulating nature for the whole community was embodied in it; the need for that psychological element of personal touch between officer and man was never more apparent than at such a time as this. So, when the physical training school was revived at Portsmouth early last year, 1919, the subject had also to be reviewed afresh.

This element of recreation had also been realized as an integral part of the scheme of training in our Armies at home and abroad. That exceptionally fine system of treating the trench-weary soldier had been inaugurated by Colonel R. Campbell, D.S.O., at his headquarters of P. and B. training at St. Pol. Consequently it had to be realized that physical education embodied a larger field of study and research now than was ever believed in pre-war days.

I will now try to deal with the result of war experience with regard to the reconstruction of our ideas as to present-day requirements. I particularly wish to place before you this chart of physical education which is hung on the wall. It is naturally incomplete, as a science of this nature is bound to be ever expanding and enriching itself the more it is developed, but I feel sure you will agree with me that this chart opens our eyes to the magnitude of the subject, at the same time as placing it before us in a concise and digestible form. Further I am personally of the opinion that it is admirably suited to form the basis of a national scheme of physical education, and will endeavour to prove this as I briefly describe the chart to you. At present we are using it as the groundwork of our scheme for training the officers now under instruction at the Royal Naval School of Physical and Recreational Training (*see* Chart attached).

We divide physical education into three main headings:—

(1) Activity developed by physical training.
(2) Energy developed by recreational training.
(3) Character developed by morale training.

And though for the purposes of description I shall run quickly over the branches radiating in sequence from each main heading, yet it must be understood that every portion of the chart is interdependent: the development of character being an object of the recreational training just as the recreational training is, after all, an essential part of physical training.

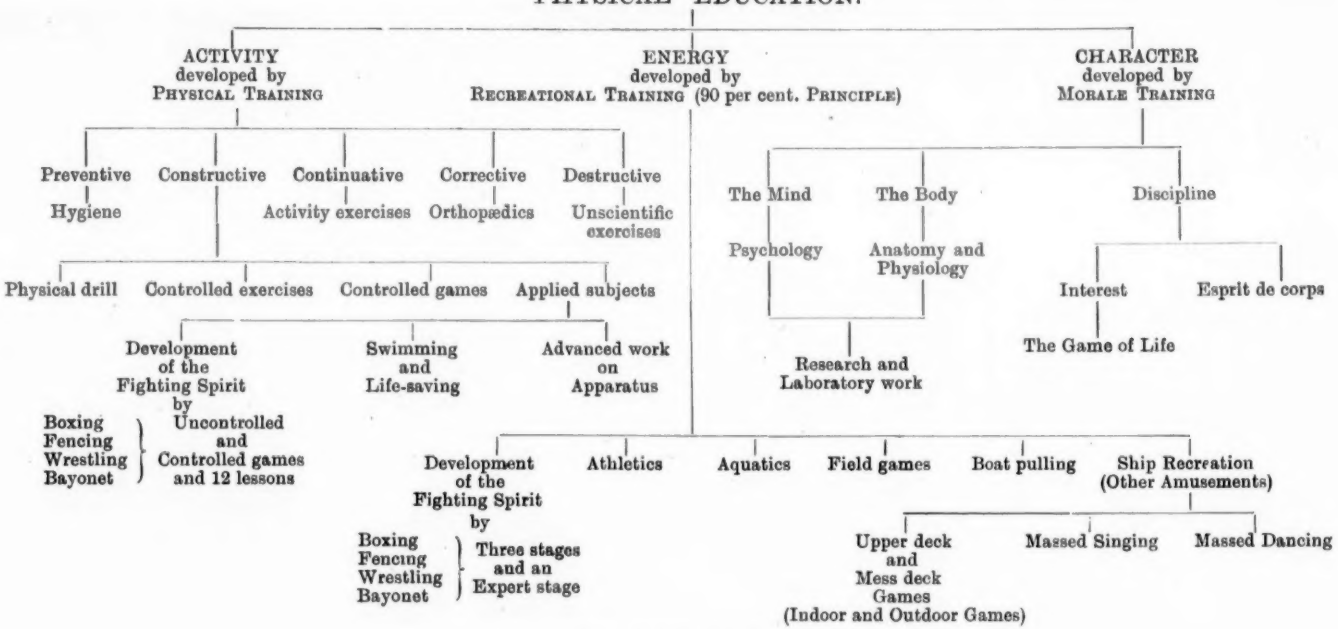
Activity is health—the two are inseparable. We all of us realize that as long as a child is active he is in good health; we become anxious at once if his activity wanes or is replaced by listlessness. So we study activity in our development of physical training which deals with measures which are: preventive, constructive, continuative, corrective, destructive.

(1) *Preventive* measures are dealt with by lectures on the anatomical and physiological hygiene of the body, pointing out those elements of daily existence which are harmful to the development of a healthy and active frame. Anatomy and physiology naturally come prominently into this heading.

(2) *Constructive* measures speak for themselves. Here is the science of applying bodily exercise to the young which will ensure as perfect and fit a body as it is possible to acquire. This is dealt with under the following headings:—

(*a*) *Physical Drill.*—This may be defined as the application of any

PHYSICAL EDUCATION.



THE GAME OF LIFE.

I.—RULES: (1) Don't play foul. (2) Don't chuck the sponge up. (3) Go all out to win. (4) Play for your side and not for yourself.

II.—REQUIREMENTS: The achievement of the highest human endurance through

(1) Stomach.	Food.	
(2) Health.	Full bodily movement.	
(3) Sentiment.	Song and Dance.	
(4) Happiness	Indoor and Outdoor Recreation	On the 90 per cent. principle.
(5) Comradeship		

III.—ACQUIRED BY: Simplicity and Attractiveness.

IV.—RESULT: MENTAL AND PHYSICAL ACTIVITY LEADING TO PERFECT MORALE.

bodily movement to produce a spectacular effect rather than a constructive effect. An instructor can apply drill, but a teacher works for the constructive effect on the bodies and minds of those he is training.

(*b*) *Controlled exercises* embody the minute study of the application of the Swedish system of gymnastics, made attractive so as to suit the characteristics of our nation—which I honestly believe has not been so far understood in this country.

(*c*) *Controlled games* consist of a large series of those games, taught in the uncontrolled stage under recreational training, which a teacher may wish to employ in his gymnastic table to combine constructive exercise with attractiveness.

(*d*) *Applied subjects* are the connecting link between physical and recreational training, but more easily dealt with under the former heading.

(3) *Continuative.*—No system of education which does not ensure continuity should be worthy of consideration. It is necessary to understand the difference between what may be called educative physical training, that is to say, the definite constructive development of the bodies of young and growing people, and the maintenance of physical fitness in the fully developed bodies of those of maturer age. There comes to the latter, sooner or later, a desire for physical improvement, and with it a conviction that some form of daily exercise is necessary to ensure bodily health and activity. Activity exercises have been compiled for this purpose. They are simple in character. They take two minutes to do. They provide full and free movement of the body in an interesting and attractive form.

(4) *Corrective.*—Orthopædics is a subject which at once calls for the knowledge and assistance of the medical profession, and for the research work embodied in the last heading on the chart. When it is realized that the following percentages of abnormalities were found to exist, before the war, in certain scholastic establishments selected as a test, it will be readily understood how necessary it is for this subject to be seriously considered: Flat feet, weak ankles, and knock knees, 33 per cent.; chest malformations, 63 per cent.; spinal irregularities, 15 per cent. (This is only a brief summary of some twenty headings under which malformations were classified by the authority who gave me these details.)

(5) *Destructive.*—A teacher of physical education should know what evil effects may accrue from an injudicious use of bodily activities,

especially at such times as these when our papers are filled with advertisements of so-called physical culture experts. This subject is dealt with by lectures and practical demonstrations of such unscientific exercises, done with or without apparatus, which can be proved to be destructive both anatomically and physiologically.

Recreational Training.

Here we have a more than essential element in the scheme of physical education; it is the very foundation of such a science. It provides measures for dealing with all types of pure recreation, and must be applicable to the majority rather than the minority. We all know the inestimable value of the sports and games we were fortunate enough to be taught and get facilities to participate in, in our school and 'Varsity days. How they literally moulded our character and taught us almost unconsciously to play the game for our side and not for self, how that foul play was penalized, how that energy and honour were essential to success; but what of the masses that were not so fortunate? Those who had not the opportunities of obtaining the variety of childish games which teach the importance of obeying rules; whose only games of childhood consisted in bettering their rivals at marbles or knuckle bones and the such like; whose football and cricket consisted more often of noisy squabbles and fightings in the streets and squares in their neighbourhood; whose only chance of learning something of our so-called national sports was by perhaps being lucky enough to attend a football match, where twenty-two paid players performed before thousands and thousands of onlookers; or perhaps it may be a boxing contest in some hall, where anything but clean sport was exhibited to their inquiring minds. Think what chance these millions have had of learning what is meant by playing for their side, for their native town, their country! What chance have they had of moulding their characters on fine lines?

Let us think what this means to a nation. Recreation develops energy. We must conclude that energy is dwindling because more people are willing to watch, than to take part, and recreation does not attract. To put it in another way, 10 per cent. wish to play, and 90 per cent. are content without. This term—the 90 per cent. principle—is used in connexion with the recreational training scheme depicted on the chart, and requires brief explanation. Say, it is agreed that 10 per cent. of the population are born to excel at sports and games. In the past,

the needs of these have been specially catered for, and obviously will continue to be, I trust. The 90 per cent. have never indulged systematically in sports and games, principally because they have never had the opportunity; or perhaps they have been discouraged by their own lack of ability to compete with the 10 per cent. This scheme of recreational training (though still encouraging the expert to further ambitions) applies principally to the 90 per cent., otherwise it would not be an educational scheme, and provides for them:—

(1) An immense range of physical and recreational activities.

(2) A new and hitherto undeveloped incentive to enjoy them.

(3) An organization whereby these may be applied in such a way as to secure the utmost advantage as regards the development of energy and character.

The six sub-headings: (1) Development of the fighting spirit, (2) athletics, (3) aquatics, (4) field games, (5) boat pulling, (6) other amusements (ship recreation) must be understood to cover something quite apart from their everyday meaning.

A recreational training handbook, which deals only with this subject, has been compiled, but not yet published, and contains six sections as follows: (i) Energy exemplified, (ii) the classification of physical effort, (iii) the standardization of all physical activities (300 and more types to date), (iv) the method of rewarding 90 per cent. (collective) effort, (v) the introduction of the scheme, (vi) musical activities.

The headings on the chart under which this recreational training is developed speak for themselves.

The development of the fighting spirit is obviously the practice of such sports and use of weapons which from time to time may be considered necessary to be included in the training of the sailor or soldier. From a national point of view such pastimes may be considered unadvisable or likely to cause adverse comment. It may be said they produce the idea of hateful militarism, to savour of war and the hideous clash of arms. In that case the heading would naturally be left out or the subjects taught be those which develop the spirit of self defence, the combative spirit, or some such element. Be that as it may, the methods of teaching the sport chosen are conducted on the same lines —viz., as an uncontrolled game; then controlled, with at first one simple rule which must be kept, then another rule and so on until the game in question is completely controlled and the pupils themselves acting as judges to see fair play or to penalize any breach of the rule; 90 per cent. contests, in three degrees and an expert stage, so that all

can compete without having their interest in the game cramped by the ten per cent. who excel; 10 per cent. competitions, championships as usually held to-day. In this manner all athletics, aquatics, field games, boat pulling, ship recreation (which for a national scheme would read "other amusements") are also conducted.

For the purpose of giving a short example of how any such sport is conducted, after learning the methods laid down in the "Recreational Handbook," let us suppose a school wishes to run an athletic sports meeting, in which *every single boy* is to compete, and his individual effort is marked so as to have an important bearing on the result of his house, for whose honour and glory he is going to go "all out."

We look at our classfication of physical effort to choose an event from each heading, so that the programme of sports will be representative of all the necessary degrees of effort, of which we find eleven headings. Now for one of these (the agility part of our programme) we have chosen the 100 yards race. Every boy has to run, and the time he takes to cover the 100 yards is converted into a percentage on a known factor of skill, which represents his share towards the aggregate marks of his house. Then by a simple score sheet (the arrangement of which is all explained in detail) the average marks of the whole team or house are arrived at, and every single competitor can feel he has done his bit, and thus energy and the desire to "go all out" for his side is cultivated and encouraged in the youth. So on with any of the hundreds of forms of recreation and games dealt with.

Of the heading "Ship Recreation" or "Other Amusements" I would like to add a few remarks. For *upper deck* and *mess deck* games could be substituted, outdoor and indoor games (of which we have collected a great number).

Massed Singing.—Here we have a really interesting psychological element. We sing when we are happy and cheerful, and by encouraging massed singing we can intoxicate crowds into being happy and cheerful. How often we have heard of the thrill of joy given to some unhappy prisoner or exile when the strains of some familiar song of his childhood reach him in his lonely vigil. Music deals with the sentimental side of a nation's character, and true sentiment is an overpowering force which can be guided for good or evil. The custom of singing national songs *en masse* is dying out in the country, except in certain parts of Wales and perhaps some isolated districts. Patriotism and love of our Alma Mater are developed and stimulated by suitable songs. Realizing this

the subject is revived here and the singing of simple songs encouraged. It has been a surprisingly interesting thing to me to find how the big burly bluejackets simply revel in participating in our Musical Activity Evening at our Portsmouth school, when they are encouraged to learn the old folk songs, national songs, and sea chanties—even carols and other ethical songs.

This from a national point of view would benefit no doubt by development on an organization based on the territorial methods.

Massed Dancing (another way of dealing with sentiment).—The war opened our eyes to the power of dancing as a recreation. Evening after evening, on the upper deck of our big ships lying at Scapa Flow, there were hundreds of sailors going round and round to the rhythm of some stately dance beaten out by one of their messmates on a piano. Hour after hour we could hear over our heads the swish-swish, stamp-stamp of their feet pacing out the De Albert or Valeta. That dancing appeals to the mass of our nation is more and more apparent. By dancing I am not alluding to the jazz craze or the fox trot, but to the fact that the type that appeals to the mass (certainly to the sailor) is the more stately and graceful kind, which existed a hundred years or more ago, and has every appearance of coming back to stay once again. Under this heading of massed dancing are included all the simple country dances, morris dances and sword dances of Britain, with some 200 and more of which we have already got in touch, all of which are surprisingly popular, at any rate when practised in the evening at our school.

We come to the last of our three main headings:—

Character developed by Morale Training.

Herbert Spencer wrote: "Of the ends to be kept in view by the legislator, all are unimportant compared with the end of character making—this alone is national education."

Here, as you will already have realized, the development of character permeates the whole working of this scheme of physical education. In the chart it is dealt with under headings which are particularly the work and teaching of the medical man. It deals in a practical way with a subject that has hitherto been left, more or less, to chance. It teaches each individual to respect himself and others. Patriotism and discipline are developed in a simple and attractive way. Lectures on psychology and on the psychology of teaching are given.

The attempt to interest the individual in his own existence is made by lectures, assisted by the microscope, magic lantern and specimens, which deal with the simple truths of cell life, of botany and of reproduction. The human body is built up in a simple and useful way so as to enable the teacher to understand the why and wherefore of physical and recreational training. Further the sex question is developed at some length, puberty and adolescence in the boy is dealt with, along with the moral and sexual difficulties of young adult life—subjects around which there is so often an element of mystery and secrecy, whereas, knowledge wisely imparted may protect innocence, while knowledge withheld may provoke contamination. I am one of those who have learned by experience the importance of instructing the young idea, simply and normally, to know the truths of their bodies and of life—thus endeavouring to cultivate self reverence and self control.

These are all included under the headings on the chart of The Mind; Psychology; The Body; Anatomy and Physiology.

Though the subject is dealt with here in as practical a manner as is possible it is thoroughly realized that in a national scheme, where its survival in a nation must be aimed at, the study of character would be further developed under other headings—e.g., religion, idealism, ethics, aesthetic appreciation, constructive thinking, &c.

Research and Laboratory Work is needed and will undoubtedly come in time. I am an optimist on this point.

Discipline is a subject of the utmost importance these days: both the employer and employed should be trained to understand its intricate nature. We deal with it on the lines of *esprit de corps* and interest as opposed to a discipline by fear of punishment, as far as the young idea is concerned. So important has this reconciliation of discipline become that in future, if war ever has to come again, the very "survival of democracy" may hang on the solution of this problem to-day. Nowhere has this been realized more than in the U.S.A., and by our sister states of Australia and Canada.

This brings us to the last item on the chart, which is put down as "The Game of Life" for want of a better title. Call it what we may, it embodies in it the whole ideal of this system of physical education; it must not only be understood, but be brought into every development in order to ensure success. It speaks for itself on the chart.

This spirit is being inculcated into the nation by that magnificent movement inaugurated by General Sir Robert Baden-Powell—Boy

Scouts. Further, I cannot help feeling myself that had it been so embodied into our whole educational system for the last ten years or more, the so-called industrial unrest of to-day and the use of the strike weapon might never have shown themselves.

Such are the lines on which we are experimenting to reconstruct our methods and teaching at the Naval Physical and Recreational Training School at Portsmouth, and if anyone here would care at any time to come and see the work in practice he can be assured of a warm welcome.

That some system of national physical education is necessary to-day has been made only too forcibly apparent to us all, from the lessons learnt in the war, so that one need not dwell on this aspect but take it for granted. It must have been more than a shock to every thinking man to realize that during the critical days of the last year or two of the war, when every man who could reasonably be expected to help was urgently required at the front, that only 36 per cent. of our manhood of the fighting age was fit to go. I think the words used by General Sir H. S. Horne, whilst speaking at Bradfield College early last year (words which I have never seen contradicted, though I had hoped they might be), bring the facts to us like a blow in the face. The papers quoted him as having said :—

"Could we not in some way manage to introduce throughout the whole country some form of training which would enable us to develop the physique of the boys of the country? It was utterly scandalous that the British should have ranked at the bottom (he had not the figures, but he believed at the bottom) of the list as regards physical fitness of the nations from whom they were able to obtain statistics during the war."

I personally feel that there are three great reasons why we found ourselves to be a C3 race :—

(1) The fact that as a nation we have allowed the spirit of competition to become concentrated on to the training of a few specialists in one or other of our national sports, to the detriment of the mass of less fortunate citizens, who are not considered worthy of attention, as they perhaps do not shape well, or offer any promise of profitable form in this or that direction.

Whilst looking up material for this paper I happened to come across a passage in a paper I read before the United Services Medical Society in 1909; when discussing this very point of specializing I apparently said :—

"This specializing in one or other kind of sport is, I consider, one of the evils which is gradually depressing the all-round physique of the whole nation. If we were to go to war and had to call upon our men for normal feats of endurance, it is the all-round physique we should want, not the man who says he cannot march so many miles or use a shovel because he is a hurdle racer, or the man who says he is unable to pull an oar, but that he could put a shot farther than any other man in the South of England."

This was June, 1909, five years before the War. It apparently was not considered of much moment at that time!

(2) The fact that the subject of physical education has not been taken up seriously by the nation, but has been left in great part to be conducted by private enterprise, or by a number of so-called experts to practise their various methods. The instructors in a vast number of cases having had only a few weeks' instruction in how to conduct certain stereotyped tables of exercises on fixed and rigid lines. It is a rare, an extremely rare thing, to find a real *teacher* of physical education these days, simply because we have no central school of thought to impart the necessary knowledge to him. I think I am right in saying that so far as men are concerned the Swedish system of gymnastics has never been understood in this country. There are several women's colleges where two years' training is the minimum, and I am now informed that they are extending this to three years. When it is realized that every child in Sweden has three-quarters of an hour daily physical training from the age of six until going to the universities, it will be understood that the teachers must have sufficient knowledge to be able to make their subject interesting over such a long period. The type of instructor employed is another point in this connexion. We need the university student, the officer rather than the man, because physical education is a science to-day needing study and knowledge, and central control by the Government is essential.

(3) The most important reason in my opinion is the fact that the subject has not been studied to the extent it should be by all members of the medical profession. Our medical schools teach anatomy and physiology more with the ultimate object of curing disease rather than of preventing it. A most illuminating address bearing on this point was delivered by Professor F. W. Andrewes, on October 16 last, before the Abernethian Society, at St. Bartholomew's Hospital, where the lecturer pointed out:—

"I ask qualified men to look back upon their medical education and reflect how small a part the *prevention* of disease has occupied in what they have

been taught, as compared with the diagnosis and treatment of disease already existing. I verily believe that if but half the energy and effort, which we now put forth in endeavouring to cure those already ill, were devoted, with equal intelligence, to preventing people from being ill, the effect upon the total sum of human suffering and death would be vastly greater."

Sentiments with which I am in whole-hearted sympathy and because of this very omission in our training I have styled my present paper—"The Position of the Medical Profession with regard to a scheme for National Physical Education." I am certain I shall be voicing the opinions of my superintendent of the Naval School of Physical and Recreational Training, as well as that of the heads of the same departments of the Army and Air Force, when I say they would welcome with both hands the collaboration of the Medical Profession in their arduous and intensely interesting work.

This very chart I have placed before you to-day is far from being my work or emanating from the brains of any medical man; but it is the result of seventeen years' practice and constant study on the part of a retired naval officer of the combatant branch—Commander B. T. Coote—who is at present assisting us at Portsmouth in the capacity of civilian adviser to our school. The teachers (where there are any) and the instructors at our boys' schools are in nearly every case devoid of all medical training, and in most cases, even the necessary support of their local medical practitioner. No, I hold that until the subject is taken up whole-heartedly by the medical profession it will lack the support it so essentially deserves.

This brings me to my crucial point, which I have set myself to place before you. The country urgently needs a more comprehensive and a better *organized* system of physical education; and, Gentlemen, it is up to our profession to see that it gets it. I do not presume that you should go away with the idea that this chart depicts THE system that is wanted; I place it before you for want of anything better. It certainly treats the subject in its proper sphere of importance and magnitude but that it is not open to improvement is far from being the truth. It at least forms a basis for thought and future development, as more research work is devoted to the subject. Neither do I wish to infer that the Navy alone realizes the necessity for recreational training. The Army and Air Force have adopted such methods for some time past and we are all working on the same ideas for one national object. The Board of Education has recently published a new syllabus of Physical Training for Schools (1919) giving suggestions to their teachers

in which the importance of games of all sorts is distinctly realized and a certain number of indoor and outdoor games are suggested. What is required is a central control of this subject and its recognition as a science worthy of a chair at one of our universities for the professorship of physical education. We have now a Ministry of Health; could not this or any other Government body be approached by a strong appeal from the medical profession of Great Britain for recognition of this subject of physical education as a large branch of preventive medicine, by:—

(i) The formation of a National Council of Physical Education to study and elaborate the lines our national system of physical education should take:

(ii) The formation of a British Physical Educational Association on some such lines as our present British Medical Association, to control all members practising physical education in Great Britain:

(iii) The formation of a central gymnastic institute where teachers of physical education could be educated on such lines as would be directed by the National Council, and obtain the certificate of the association mentioned above?

Upon the fact that the matter is urgent I think we are all agreed. Sufficient proof of its being considered so is daily shown in the papers. Such a doctrine, which strikes at the very heart of the health and morale of the nation, is not dependent on party. It is equally important to any Government which may be in power, whether it be a Coalition or that dictated by nationalized ideals, or for the maintenance of private enterprise. The Prime Minister himself coined the expression that we are a C3 nation from the facts given him by the medical profession, and in a great speech in the House he said we must become an A1 nation if we want to remain a great empire. But a *lethargy* seems to overshadow the question as to who is to take action? Early last year the headmasters of our big public schools included the subject of physical education in their annual conference. The President of the conference has given me permission to quote as follows. He says:—

"I approached the Board of Education in my capacity as Chairman of the Headmasters' Conference to push for proper Government control with regard to training experts of physical education—i.e., the necessity for a national college of physical education. I was given clearly to understand that the Board of Education could not view matters in this light, and considered such a development should be left to private enterprise."

Last May, the United Services held a physical training conference at which were present representatives of our Colonies and a member of our Board of Education. There were many issues under discussion, but the most important one was round the problem of creating a central organization such as a physical training university for the empire as a whole, to embrace all classes, civil and military. We had a great and earnest discussion, and it was more than interesting (it was almost shaming us here at home) to hear how progressive and how alive to the necessity of a national scheme of physical education were the Governments of our various Colonies. The proceedings reached a sort of climax, and ended in the formation of a sub-committee, consisting of one member from each of the Services, with the representative of the Board of Education to sit as chairman (subject to approval of the Board of Education), and I had the honour of being elected as the naval representative. This sub-committee was to deal with the subjects under discussion and prepare a paper which would be submitted to the main conference with a view to the possibility of its being put before the Government for their decision. This sub-committee was to be called as soon as information was received that the Board of Education had sanctioned their representative sitting as its chairman. That was on May 20, 1919; it is now January 21, 1920, and no sub-committee has been called.

Who is to Take Action?

The headmasters have failed, the desire of the United Services met with no response; but may it be a feather in our cap, that the medical profession succeeded in awakening the country to the necessity of such a measure of preventive medicine becoming a national asset. I beg of you not to let the matter end with this meeting here this evening. I would that a committee might be formed by this, the Royal Society of Medicine, to consider the subject most seriously, and invoke the action of all medical and surgical universities and societies throughout Great Britain, so that a strong and representative petition may be presented to the Government, showing that the medical profession is alive to the huge importance of this subject being tackled at once during this period of reconstruction of our national ideas. Immediate action in the matter is truly of the greatest urgency; convinced as I am myself, I may have failed to convince you all that this is a truth. If so, it is probably due to the fact that I have failed to place the subject before you to-day in its proper and truthful aspect.

DISCUSSION.

The PRESIDENT (Sir Humphry Rolleston): Surgeon-Commander K. Digby Bell, R.N., is an expert in physical training and has put his knowledge to excellent practical effect during the War. After being in the ill-fated *Audacious* he went successively to the Flagships H.M.S. *Iron Duke* and *Queen Elizabeth*, and kept the Commanders-in-Chief, Sir John Jellicoe and Sir David Beatty, and their staffs, in good physical condition during the long days of watching and waiting at Scapa. Throughout the War it was of the greatest importance to maintain the morale of the men; this was most successfully effected by the organization of games, entertainments, boxing and rowing competitions, and in other ways, and in all of these Surgeon-Commander K. Digby Bell played a leading part. I hope Sir Robert Hill, as his chief, and with special knowledge of the subject, will open the discussion.

Sir ROBERT HILL: The question of physical training in education is of such national importance that I am more than ever sorry that Dr. Addison, the Minister of Health, has not been able to accept the invitation of the Society to be here to-day. Physical training has made giant strides since the old days when in our gymnastic classes our great desire was to increase the circumference of our chest and the size of our biceps, up to the present day when we see such a thoroughly worked out scheme for physical education as Surgeon-Commander Digby Bell has portrayed on the board, and in his lecture. The necessity for medical supervision was brought to prominent notice some ten years ago in consequence of the high standard required to qualify as a physical instructor, which caused many cases of heart strain with consequent breakdown of many fine men. Our American cousins apparently lay great stress on swimming as a necessary accompaniment to mental culture, as some of their university authorities insist that in addition to passing his examinations the candidate must swim the college lake before gaining his degree.

Colonel J. F. C. FULLER: Out of the many interesting points which have been raised by the lecturer there are two which I should like to accentuate. The first is the weakness of individual or even class enterprise, and the second is the strength of co-ordinated or national effort which permits of the highest form of individual initiative being developed. Take, for instance, the great science of medicine; four hundred years ago it was difficult to differentiate between the magician and the physician: why? The science of medicine, though crude, was not exactly in its cradle, but all work was based on individual effort, and the individual had not the necessary strength to overthrow the prejudices of popular opinion or the vested interests of the Church. Then, little by little, medicine combined, until to-day it practically rules society. So, also, with National Physical Education: at present physical education is not in its childhood, but its organization is

mediaeval. During the war the fighting services, at least, discovered its supreme value, its power of improving not only body and mind but its power of creating solidarity by collective contentedness. To-day, though the men of the fighting services do not look for fitter recruits in order to create a more spectacular war, they, belonging to a small class of the community, are still working too individually to be able to overcome the opposition of public opinion to what is classed as "militarism." We soldiers and sailors, I think, have proved the supreme value of physical training for war, and we should like to see its value applied to peace, so that no longer may the slur of a C3 nation be cast upon us. Our uniform, however, prevents us speaking with profit. The medical profession is handicapped in no such way, for it is based on the ideal of saving life, it has enormous power and a wonderful prestige. Its object is surely higher than curing disease, its object is to prevent disease and, I think I am voicing the opinion of many of us present when I say that we look on the great profession of medicine not only to suggest and speak on this vital question but to act and do.

Wing Commander MARTIN FLACK: I believe that my observations made upon successful flying officers show the value of physiological research in relation to physical training. A comparison of the results obtained from the examination of the effective and non-effective officers shows that the former possess good co-ordination between eye and limb movement and a great resistance to the onset of fatigue. To this the practice of the various forms of athletics greatly contribute. It is better to cultivate an aptitude at acquiring various forms of sport rather than a marked proficiency in any one. For eye and limb co-ordination, the various "eye" games are important, e.g., tennis, badminton, &c., also fencing. Endurance is brought out by swimming, football (especially Rugby) and cross-country running. My investigations show that this resistance to fatigue is particularly associated with a good respiratory capacity. An efficiently working diaphragm and a firm-toned abdominal wall are particularly important, since these greatly aid the return of blood to the heart, as well as effectively expanding the lungs. In regard to respiratory exercises it should be emphasized that efficient expiration is of the greatest importance, and special expiratory exercises should be practised. I believe also that a certain number of exercises performed with the breath held may be usefully incorporated in systems of physical training, always provided that efficient breathing has been previously learnt. Athletes who perform severe work with the breath held—e.g., under-water swimmers, boxers, sprinters, Rugby forwards, make very efficient high fliers. I agree with Commander Digby Bell as to the great value of singing and dancing.

Sir ANTHONY BOWLBY: I think that what Commander Bell has told us makes it clear that physical training should be made as interesting in civilian life as he and others have made it in the Navy. The difficulties are many, and the greatest difficulty is in providing suitable playgrounds and recreation rooms in large cities. It is to be hoped that the Ministry of Health will include in

their scheme for the well-being of the young of both sexes some system by which the opportunity for the healthful exercise may be extended. I can confirm the excellent results obtained by teaching men the old "country dances." They were most popular in convalescent camps in America, and hundreds of men could participate in them.

Sir CHARLES GORDON WATSON: It is most essential that the medical profession should awake to the meaning of physical education and its far-reaching results and that they should be the prime movers in its development. The very instructive and suggestive table which Surgeon-Commander Digby Bell has put up is especially valuable because it gives a graphic demonstration of the scope of physical education, and defines a subject which cannot be adequately expressed in words, and I hope this table will gain wide publicity. The man in the street, if he considers the subject at all, looks upon physical education as a dull system of drill with Teutonic tendencies. The Englishman has much the same opinion of drill as he has of the Teuton and until the full meaning of physical education can be brought home to the British public—i.e., until they realize it is the keystone to health and happiness and is indeed the very essence of preventive medicine—there is no hope of establishing an organized universal system of physical education in this country. America has realized it and is acting on it. Surgeon-Commander Digby Bell has shown us to-night that the British Navy also realizes it and is acting on it, and he has shown how a comprehensive system of physical education can be carried out. There can be little doubt that much of the labour unrest of the present day is due to the empty leisure hours of the so-called working classes and to the absence of any organized facilities, especially in industrial centres, for the rest and recreation of the workers. I should like to see *rest* included in the table before us. Facilities for rest, both physical and mental, in the form of indoor clubs and libraries, are almost as necessary as facilities for recreation. There can be no doubt that the long duration of the war with the corresponding increase in loss of life and treasure was in the main due not to any deficiency in population but to the high percentage of physical ineffectives in our population. This consideration alone should be sufficient to point to the need for active effort on the part of those responsible for the health of the nation. If however we wish to succeed we must avoid all suspicion of militarism, and if we would create a popular movement we must be free from Government control. A few months ago, with Sir Anthony Bowlby and others, I put forward in a letter to the *Times* the following suggestions:—

(1) The formation, under the Ministry of Health, of a Directorate of Physical Education which will evolve a definite system of physical education and which will advise, when asked, any volunteer movement connected with the improvement of the physique of the people.

(2) A National Council of Physical Education composed of representatives elected by those associations whose work and influence would warrant their inclusion.

(3) The establishment of a peripheral organization on a territorial basis with the requisite power, money and personnel to maintain an organized system.

The British Red Cross Society and Order of St. John have such an organization in their Voluntary Aid Detachments and they have the requisite power, money and personnel to maintain an organized system. I have reason to believe also that such a scheme would be favourably received by their Society.

Some two months ago I attended a conference with the Minister of Health and a representative of the Board of Health to discuss proposals based on these suggestions. I am now convinced that the first thing to do is to create a National Council. A National Council which is representative of all forms of athletics and games, of boy scouts, girl guides, polytechnics and the like would be representative of a very large body of the electorate; and as such would have a voice which could always command a hearing from the politician. If you, Sir, as President of this Society were to call a preliminary meeting of representatives of the great athletic associations under the presidency of Lord Desborough or some other well known and popular patron of sport, I believe you would succeed in creating a representative National Council. Time will not permit me further to elaborate the scheme. The important thing to do is to act and there is no one more fitted to take action than the President of this Royal Society.

Sir JOHN GOODWIN: I have great pleasure in proposing a hearty vote of thanks to Surgeon-Commander Digby Bell for the extremely interesting address to which we have all had the privilege and pleasure of listening. Commander Digby Bell has succeeded not only in interesting us in the subject but also in carrying conviction as to its extreme national importance. I suppose that there is nothing which is more conducive towards mental and physical deterioration than monotony. It is, I think, evident that the Royal Navy had more adverse conditions, as regards monotony, with which to contend than had the Army, but nevertheless I know full well how liable men were to deteriorate after a long spell under the very adverse and trying conditions of weather, mud, and constant exposure to danger. I know of few units in France which did more than the convalescent camps towards overcoming the evil effects of such an existence. I have, many times, seen men come into the convalescent camps tired, war-worn, depressed and dispirited, yet after a few weeks in the camp I have seen them leave again for the front in the best of spirits, fit, cheery, and quite ready to return to the front again. I was much interested in the high tribute which the speaker paid to the results attained by dancing. It was really astonishing to observe the keenness with which the men, without exception, took up this form of recreation; they could all take part in it, they all enjoyed it, and they got themselves physically fit unconsciously and without any of the irksomeness of physical drills or "set" exercises. I entirely agree with the lecturer in what he said as to the

necessity for *all* men to enter into the various forms of exercise, in preference to the vastly inferior method of selecting certain specially suitable men and training them up to a high standard. As regards boxing I must confess to a predilection in its favour, but here again close supervision is absolutely essential, and it must be ensured that no youngster when learning to box is "overmatched." I feel strongly that not only the masters but also the medical officers of our public schools might, with very great advantage, take a close personal interest in the physical training of the boys. I have often thought that, while the boys who are sufficiently gifted and developed to be in the Eleven or Fifteen are known to everyone in the school, yet the others are liable to be somewhat overlooked. The public schools have done an enormous amount for the nation in this war, but I feel that, from a *national* point of view it is not the public school boy who is most in need of physical training but the boys and youths in the lower strata of the community, and here I fear that the difficulties to be overcome would be great and many. I am sure that I voice all your sentiments when I say that we have immensely enjoyed Surgeon-Commander Digby Bell's address and thank him most sincerely.

Colonel FULLER seconded the vote of thanks.

Surgeon-Commander DIGBY BELL (in reply): I cannot thank sufficiently General Sir John Goodwin and Colonel Fuller for the kind way they have proposed a vote of thanks to me, and all present for the generous way they have accorded the same. I only hope this means my effort will bear fruit and that the Government will be approached as I have implored, so that this subject of national physical education may become a great science of preventive medicine under complete control. It is getting too late to touch on all the interesting side issues brought forward by the several speakers this afternoon but I want to say a few words with reference to General Sir John Goodwin's remarks about the spirit inculcated into our public school life by games teaching boys to "play the game for their side." It was well shown in the first year of the war. We all know how these men rushed at once to their country's assistance, leaving cattle ranches in South America, tea plantations in Ceylon and Assam, &c., &c., forsaking all private interests, to "play the game for their side." It is all too pathetically apparent to-day in the long list of British families whose entire manhood has been wiped out. I verily believe that if some such opportunities of learning the value of such games and recreations, as I have briefly alluded to as being incorporated in this scheme of physical education, had been given in the past and this spirit had been instilled into all our masses—such a thing as conscription would not have been necessary in the last two years of the war in order to obtain the services of our manhood to "play the game for their side" and to answer their country's call for help, and I certainly am positive we would not have found ourselves a C3 nation.

The Royal Society of Medicine.

President—Sir Humphry Rolleston, K.C.B., M.D.

The President: We are fortunate in having the opportunity of seeing Dr. A. D. Waller's demonstration of the exact measurement of the effects of human emotion by the psycho-galvanic reflex. He has been working for about three years on the manifestations which take place in the skin as a consequence of emotion, and has described his results before the Royal Society, and also in the *Lancet*.[1] He pointed out that it might be of practical use in medicine, and I hope he will show us how this exact method of measuring the psycho-galvanic reflex of the skin can be adapted to psychology and the practice of medicine: for example, whether a man who has suffered from shell shock has sufficiently recovered to go back to his work.

The Measurement of Human Emotion and of its Voluntary Control.[2]

By A. D. Waller, M.D., F.R.S.

I am fortunate this afternoon in being able, I hope, to demonstrate and justify the method of measurement at which I have been working during the last few years. I have my thoughts arranged under three heads. I hope this will be a demonstration of fact, because notwithstanding the previous efforts of many workers, the fact itself meets with some scepticism. Finally, I hope to lead up to what is the final fruit, which ought not to be left without mention before this Society, the possibility of its application to the elucidation of knowledge concerning the person who is suffering.

[1] *Lancet*, 1918, i, pp. 311, 380, 916.

[2] At a meeting of the Society, held February 10, 1920.

As regards the preface, I should like to take hold of the subject in a manner that I have not yet ventured to do, that I could not venture to do in previous years during which I have been engaged upon it.

I should like to spend a few minutes in laying down the principles, without argument, the principles underlying this psycho-galvanic reflex, sometimes called—as I prefer to call it—the emotive response. All who have gone through the curriculum are familiar with the idea of trophic nerves. We all, doctors and laymen alike, are acquainted with the expression of the emotions. We all know that in emotion the manifestation of some dominant feeling in the brain expresses itself in a variety of ways through the ordinary muscles, through the vascular muscles as in the blush or the pallor, through the vasomotor nerves, through the secretomotor nerves, in the action upon the salivary or skin nerves, and so on, which I pass by rapidly, in order to state that there is a yet more fundamental action of nerves upon the peripheral system. When an emotion occurs, when any action is willed or provoked in the brain, when it gives rise to the explosion which, passing along the nerves, provokes action at the periphery, it does one thing besides secretomotor action and vasomotor action: it acts upon the cells of the body in a way that alters their nutrition. And the keynote to the picture of what happens when a nervous impulse meets with a living sheet of cells like an epithelial membrane, like the rows of epithelial membrane which form the skin, that form especially the skin of the palm of the hand, what happens there is something which I venture to call, to christen, poro-dilatation. A strong emotion happens, and alters nutrition. What is nutrition? It is a commerce of ions, an electrical unit which passes in and passes out of the body through a membrane which essentially consists of holes or pores, of ultra-microscopic pores, of ionic order. So in the commerce which takes place in the last act of respiration, CO_2 passes out, and O_2 passes in, and that is the essential fact of the nutrition. These pores, which are invisible, which are ultra-microscopic, are, in my view, under the governance of the nerves, and the demonstration which I hope to put before you in a moment is, I believe, a demonstration of the action of what we have known, more or less obscurely, for many years past under the name of trophic nerves. Essentially, what will happen will be what is shown by this diagram. I am going to put a patient in the fourth arm of a Wheatstone bridge, and show that when an impulse from her brain passes down to the skin of the palm of the hand, there is an alteration of electrical resistance, a poro-dilatation, which will be shown

in the usual way by the disturbance of the balance that will be established in a moment. Here is what is going to happen (sketch of scheme exhibited, fig. 1, p. 53). I bring equality between the known and the unknown, in precisely the same way as the grocer brings equality between a pound of sugar and the pound weight. If one is increased, or the other diminished, there is a disturbance of balance, in response to which the indicator will go in a definite direction.

I think I have said enough to make clear that we are at a very general standpoint, that it is strictly physiological, and it has not yet led me to make use of any psychological expressions. Whether I shall succeed, in the hour at my disposal, in getting so far as to justify the title of the lecture, I must leave for the moment. Whether I shall be able to convince you that we have, in this method, a means of measuring, in precise physical terms, the intensity of the emotions and the power of the will, the inhibitory power of the brain over those emotions, or the physical signs which accompany those emotions, I shall also have to leave. I am certain it would be going far beyond my province to do more than sketchily indicate the possible use which could be made now of this additional weapon, and what forecasts will be made in years to come.

I have four slides, which will serve to limit, very strictly, the remarks I am about to make. I may explain that these four slides are the photographic fixing of the experiments that I am going to put before you in a fugitive manner. The hand is in the fourth arm of the Wheatstone's bridge. At *this* moment the threat of a burn is made. The striking of the match causes an emotion, which you may like to term fear of injury, and that causes a diminution of resistance in the palm of the hand, a poro-dilatation, and there is a movement in the direction of diminished resistance, of a magnitude which you see indicated *here*. At *this* point the reality is applied, and you see the response on the application of the match itself. Then there is the pin-prick: a gesture of pricking is made, and that causes an emotive response, consisting in this poro-dilatation (fig. 1, p. 53). And you see, too, the result of the actual prick.

The next slide justifies the specification I made. In slight degree, the manifestation extends all over the body, but so slight that I would put the evidence of it before you without note or comment, and I am not sure that I should receive an equality of opinions as to whether it is manifested at all in any part except the palm of the hand. Although the palm of the hand and the sole of the foot are the great places

for the manifestation of these phenomena, there is a general result over the whole of the cutaneous periphery. At the moment I show you, the voluntary explosive discharge from the nervous system is made, consisting in a voluntary cough, and with that explosive discharge there is a poro-dilatation in the hand (fig. 7). The same is done for the forearm, and nothing happens. On this item of evidence you will be inclined to say it is manifested in the hand, it is not manifested in the forearm. However, at the end of this lecture I shall put some demonstrative experiments, showing that, to some extent, it does happen in the skin of the forearm.

The next slide is put in for the purpose of emphasizing an attempt at evidence justifying the title of my lecture. This patient, or subject, is in connexion with the apparatus for something like thirty to forty minutes, and during the first half of that period is in complete voluntary control of her emotions: she is remaining as quiet as possible; she is not allowing any disturbing thought to pass through her mind. That is indicated by the state of the line, which is, practically, immobile. But half-way through the experiment she "lets go," allowing herself to enter into excitement, by thinking of painful experiences, by thinking of a variety of experiences, which I need know nothing about. If I have a patient as to whom I am anxious to learn the power of imagination, I simply say, "Think about the most disagreeable thing which ever happened to you, or can happen to you." And I need not enter into the confidence of the victim or patient.

Here is another plate, which seems to show quite a different thing. I am showing it for the sake of getting you to appreciate what, to me, is a very cogent proof of the reality of the phenomenon: the loss of time which occurs between the voluntary discharge and the action at the periphery. You realize, of course, that here we have a rather more quickly-travelling surface. The balance is disturbed by the discharge from the nervous system which occurs at the moment indicated by these two black teeth; they are movements of a lever connected with the larynx, and indicate the moment at which the subject coughs. The balance is not disturbed until actually two seconds after the explosion from the nervous system. There is such a huge loss of time, that you cannot fail to appreciate it if I bring off the experiment successfully.

I will now proceed to the trial of the patient (demonstration).

I am first doing what I always do at the commencement: seeing how quiet the patient can keep.

I now apply the first test, and you note a movement of the bright spot to the right. With a new patient, merely standing behind the chair causes some apprehension, and the spot moves and indicates an emotive action. On my applying a stimulus, you see a definite movement of the spot in the direction of excitement. I have now applied a burn, and judging by the excursion of the spot, it was severe. Laughter consists of a series of explosions down the nerves. This lady coughs, and you see the effect.

I should now like a volunteer from among the audience. Sir Kenneth Goadby has tested the matter with me; he knows the apparatus, and so forth, and I should be very much interested to see whether he reacts in the way I have learned to expect him to react.

Sir Kenneth coughs, and you note the two seconds lost time. His responses here have been distinctly less evident than under the normal quiet conditions of the laboratory. I think, judging by my own feelings and experience, that it is always more difficult to demonstrate the size of an excitation on occasions such as the present, because one is oneself already more or less under the influence of emotive excitement from the fact of having to speak. But that is not Sir Kenneth Goadby's case. Here is the effect of the burn, but it is distinctly less than I have had. I hope he will take part in the discussion, but I think he is, consciously or unconsciously, under the influence of a certain amount of excitation from the unusual circumstances of the moment. I am very inquisitive, and I would like to make myself the patient, to see if I would give smaller or larger reactions. But before I make myself the victim, I would like to describe an experiment I made two hours ago, when I was interviewed by a newspaper correspondent, who, by the way, is not here. I was interviewed at a moment when I had tested myself, and found I was much below par. Contrary to one's expectations, one gets small responses when one is not as excitable as one should be, or more excited than one should be. One puts on the wire a person who is, apparently, greatly and regularly excited for long periods—an hysterical person—forgive me if I use the wrong word: I have not got accustomed to the term "pithiatic"; people do not mind being regarded as pithiatic, but they do not like to be considered hysterical; one finds such a person makes a great fuss, and there are movements which, one would say, would give great responses. But we find he, or she, is not responsive at all. This newspaper correspondent was one of the best instances which have presented themselves to me of a pithiatic person:

he told me so quite frankly in the course of a conversation afterwards, so I do not know how the interview is going to appear, and I shall get the evening paper, with some sensitiveness, to see what he gets out of me, compared with what I got out of him, because I got out of him a most admirable illustration of the fact, which I have learned in hospital, of course, by the kindness of my friends—namely, that these pithiatic persons are far less mobile than is the normal person.

I will trust to luck, and ask for some slides to be put through the epidiascope, because they are so many records of experiments, each of which has taken an hour or so to make. The slide gives a record of what the spot of light has done.

I take the opportunity of insisting on one point, which is one of many I ought to put before you—namely, that nothing is done until one measures these things. The resistance was 17,000 ohms, that is below the normal, and it is a sign that the subject's pores were in a state of dilatation, and I imagine he would have a low resistance. Mine are, almost certainly, in a state of dilatation at this moment.

One of the things I am working at is to find out whether the reaction has the same response on the two sides of the body. It is of no use taking a galvanometer and a series of impulses, first on one side and then on the other; one must take a record of the two hands with two galvanometers which one has tried to make equal, and one is happy if one can get two galvanometers to give equal responses.

Here is the record on a gentleman who remained quiet for ten minutes, and that is a great test to ask anyone coming into the laboratory to undergo. I have now reduced the requirement to five minutes, and I have an egg-boiling indicator for that time. You see, this subject is quiescing down, the pores are gradually contracting, the resistance is gradually increasing. At these two spots he was asked to think of something exciting of a moderate degree, and of something very exciting. The very exciting thought sent the light right off the plate, and the deflection caused by the thought which was moderately exciting is shown.

The next gives the same subject, with the same experiment. Ten minutes' quiet, interrupted by throwing in one-tenth of a volt in order to know how conductive the hand was at the moment.

Here is the record of an experiment I shall not be able to repeat, I am happy to say; it is upon a Belgian lady, who was in extreme anxiety at the time in regard to the welfare of her family in Belgium,

and the suggestion of that to her was sufficient to produce a strong emotive response (fig. 3, p. 54). I then reminded her of the scenes she had witnessed, and the fear and excitement brought an enormous diminution of resistance and augmentation of conductivity. The experiment on the same lady now fails.

This shows the sort of record I have been getting of late, and I would ask for your opinion as to whether these are true responses or not. It is the simultaneous record of two galvanometers, for the left hand and left leg, and you see the responses that happen. From the amount, I do not think you would swear there was any effect in the leg—they are inconsiderable in magnitude in comparison with those in the hand. It is the palm of the hand that has the special sensitiveness in this respect. If I apply the electrode to the dorsum of the hand, I get "no"; if to the palm, I get "yes." It is not a question of relative richness in sweat glands, though I have not time to argue that point at the moment.

I have taken a plate which was lying on the table, which is what I have been doing in the last few days. Somebody asked, "Will it not happen in the head?" I said, "No," and proceeded to make this experiment. You see the hand record, and this is the elastic band round the head, and a pair of electrodes applied to the temple; and here are a couple of waves, which, to all appearance, seem to show that something happened in the head. But it is not so, they are spurious. Corresponding to the true excitations there is nothing over the temple; these are accidental movements from the movements of the jaw.

The next is an air-raid photograph. It was a subject whom I put upon the wires ten minutes before the air raid broke out; then there came the whirr of the Gothas, the noise of our guns, and the dropping bombs. You see the enormous response which happened. There is a sudden dilatation of the pores, and the emotive reaction which took place. The air raid continued during the whole time, and all the time the pores of the palm of the hand continued to be dilated (fig. 2, p. 53).

Here is where I applied a stimulus by a small induction shock to a distant part. There were a pair of electrodes on the hand, and a pair on the arm, and this is the reaction. It goes up, and I leave you to consider whether it is the reaction of the skin of the arm. It is one of the things I hoped to demonstrate, but it would be too much trouble; but I shall be happy to make a demonstration for anyone in my laboratory.

Now, here is the only reasonably good document I am able to put before you to show that parts of the body other than the palm of the hand and the sole of the foot respond. Here are two largish responses, with two galvanometers, as before: one galvanometer connected by a pair of electrodes to the left hand, the other galvanometer in the same way connected to the left leg, its tibial and calf aspect. I think the response is fairly clean for both. Note the manifest large response which takes place in the palm of the hand, yet hardly sufficient to impair one's general idea that, as regards these responses that of the palm of the hand is the special one.

What am I to say to this one? Is there a reaction in the arm? I hardly think so. Everybody says "yes," however, to the palmar reaction. And here is a tenth of a volt for the two galvanometers.

This is the record of a Royal Air Force candidate who was examined.

The next one belongs more to the title of my lecture than does any other—namely, there is, besides the intensity of the emotive discharge to be considered, the inhibitory power, or what we call the voluntary inhibition for such emotive discharge. Here is a half-plate of twenty minutes of quiet, followed by twenty minutes of "letting oneself go," in regard to exciting thoughts, especially painful thoughts, because it is the painful thoughts which are the most effective neuro-stimuli.

I will now stop and subject myself to any questions which you may desire to put to me.

DISCUSSION.

The PRESIDENT: The demonstration has aroused a universal feeling of extraordinary interest, and we must express our sincere thanks to Dr. Waller. It is curious that the response is practically limited to the palms of the hands. Years ago Charcot pointed out that the resistance of the body generally in exophthalmic goitre is very much diminished, but I do not remember that those observations were confined to the palms of the hands or the soles of the feet. It was generally explained on the hypothesis that there was an increased secretion of fluid on the surface of the body, but that we now know from Dr. Waller does not hold good for his results. It would be interesting to know if adrenalin on the one hand or sedative drugs on the other hand had any effect upon the psycho-galvanic reflex.

Dr. ORMEROD: I had not the least intention of speaking. The subject is very interesting because it brings forward another physical test in relation

to the emotions. We know that sundry physical processes go on when we feel emotions; Dr. Waller has shown us that there are electrical processes, and has placed before us a delicate test of them. I am chiefly interested in this from the standpoint of what I still call hysteria; for I do not commit myself to the theory which is implied in the new name. What Dr. Waller told us about hysterical patients is remarkable, that is, that they do not show these reactions well, because I think most people believe emotion has much to do with hysteria. You know that Déjérine, one great authority, says hysteria is essentially a matter of emotion. Janet, who is, perhaps, a still greater authority, has constructed what he calls the mental characteristics of hysteria, and, to one's surprise, says that hysterical patients are not emotional, that their emotions are not deep, but on the surface. That coincides with what has been shown by Dr. Waller's experiments. If I might make one criticism from the emotional aspect, it would be that the test seems to be rather too delicate, that is, it seems to work in regard to things other than emotions. For instance, the cough showed as good a reaction as did the burn. With regard to the sensitiveness of the palm of the hand I have nothing to say, except that we are familiar with shell-shocked soldiers who have sweating palms; but perhaps Dr. Waller would not wish that to be imported into his theory, because he says the electrical response has nothing to do with the sweat glands.

Sir KENNETH GOADBY: I have been very interested in the psycho-emotive response, and I have had the pleasure of working with Dr. Waller a little on the question. I have also had the opportunity of testing the response in those suffering from nerve lesions, and it is somewhat interesting in this respect. In the case of a man who has a divided median and ulnar nerve in the upper arm, there is no response on the injured side, but a full response on the normal side. If, however, the ulnar is intact, and the median divided, a diminished reaction is given. If the ulnar is divided and the median is intact, you get a larger but not completely normal reaction compared with the other side. So the phenomena would hardly seem to be purely sympathetic. During a certain period, I had the opportunity of testing a number of men who were not obviously suffering from shell shock but who had been wounded in circumstances which, perhaps, had rather fixed the matter on their minds. It was rather interesting to, as it were, dive into their minds and find whether, in the case of a phlegmatic old sergeant of the Argyll and Sutherland Highlanders, the wounding had left any impress on his mind. One could not get any indication of it from talking to him; he seemed well balanced, and declared he was all right. But directly we got him to tell us of the circumstances of his wounding, his resistance diminished to an extraordinary extent, showing there was a content in his mind following upon the injury. Dr. Waller has tested me in many ways, and one or two of the records he has shown were mine. I can, under certain circumstances, excite myself without a specific disagreeable concept, and then relax the mind and think of nothing. I can create a general

state of excitement without definite thought or cognition, and under these circumstances my resistance immediately diminishes. The question of sweating palm is one with which I am very familiar, because whenever I have been on a bad ice slope the palms of my hands sweated. I am certain that if this curious emotive reaction is used clinically, being approached with a scientific mind, with a consciousness of its limitations, it may be of extreme use in many cases of difficulty of diagnosis.

Dr. DAWSON WILLIAMS: It has been suggested to me that we ought to pass a formal vote of thanks to Dr. Waller for having given us this compendious account of a research which has, I know, been going on for a considerable time. It seems to me, from the general point of view, apart from pathology, to be extremely interesting. As myself a subject of experiment, I may say, as Dr. Waller himself has hinted, that the reality of the phenomenon is much more impressed upon one in a quiet room, when one's mind is adjusted to it, and free from the herd instinct incidental to being here. But I want to utter a note of warning. Dr. Waller and I have been friends for many years; he astonished me one day by stopping me in Piccadilly and beginning to gossip, which in my experience is not at all his habit. He gossipped about a variety of subjects, and it was only afterwards that I realized how he had been shifting about from one subject to another. And presently he chanced on a matter which had annoyed me very much, and the memory of which still annoyed me. As soon as he had done with me he wanted to talk no more to me, but asked me to go to his house for an experiment. He did all the things to me that he has done here, and then suddenly started this subject again; the result on the screen was remarkable. My recollection is that the memory of this annoyance produced a much greater result than either a pin-prick or a burn. A very remarkable feature is the definite delay before the response. I think Dr. Waller will tell us that part of this occurs in the higher centres.

Dr. MARY BELL: I would like to ask if any work has been done on the question of the relation of the output of adrenalin during preparation for flight or fighting and electro-motive response. It is very interesting that the emotion of fear produces this diminished resistance. During periods of terror and of exertion you get an increased output of adrenalin. I do not know whether there is any reaction time between the terrifying of the animal and the output of the secretion into the system. It seems to me there might be a correspondence in the reaction time, which it might be of interest to work out at some time. Miss Waller told me that during the summer she tested people whom she had tested during the winter, and she found that during the summer the resistance was very much higher than during the winter, in the same people. I should like to know what is the explanation, or whether any other experiments have been done on this point. I have thought of setting up an apparatus myself for psycho-analysis; it would be very interesting to analyse a patient while actually sitting in the circuit, and I think it might be a great help to the physician in trying to understand the patient's unconscious mind.

Dr. SCRIPTURE: I hope Dr. Waller will, in his publication, state the reasons why the sweat glands are not an important factor. Some years ago Dr. Peterson and I thought we had proved conclusively it was the sweat glands, but we had no idea of the wonderful new suggestions which Dr. Waller now brings forward to remove all difficulties.

Dr. R. H. COLE: I simply wanted to ask about the movement of the light following on a cough. It was very marked, and I wondered whether it was due to pressure.

Dr. F. PARKES WEBER: I would like to ask whether Dr. Waller has made observations of this nature on myxœdematous patients. If not, I think it would be well worth while to do so. In regard to the moisture of the palms of the hands, needless to say, there is a great variation in different persons. At one time I was an enthusiastic collector of medals and rare coins; there are some people to whom one dare not entrust the handling of modern so-called "proofs," because the sweat from their hands would at once remove the "bloom." But I can myself generally handle even the finest proofs without damaging their appearance. (Of course one would, however, hold them ordinarily by the edges, if only not to set a bad example.)

Dr. WALLER (in reply): If I were to attempt to answer properly the points which have been raised, I should keep you here until an undue hour. In reply to Dr. Cole, there is no emotion with cough. It is really a question of definition of words. I am afraid I am using "emotion" in the literal sense, so I should use the adjective "emotive" in order to indicate that I am not dealing with the psychological aspect. I do not use the adjective "emotional," but "emotive," imagining a flux from the nervous system, a discharge down the nervous system, which flows down the efferent nerves, whether sympathetic or not, and is an emotive response as to its action on the skin. The caution inculcated by Sir Kenneth Goadby I am very sympathetic with indeed, and you, Sir, I am sure, feel in the same sense. This will be a horrible thing if it is going to get into the hands of "half-baked" people who have not a knowledge of the physiological principles involved; and if it gets into the hands of unscrupulous people who exploit the public, there would then be great mischief done. That has been in my mind from the beginning, and I have limited myself to three places where I have spoken about it, the Royal Society, this Society, and the Academy in Belgium. I have not thought it was suitable for popular lecturing or demonstration, and I would like it to be kept clean and away from "half-baked" people if possible. I did not catch all that my friend Dawson Williams said about the gossip story, but I have had interesting séances with different people, and I have asked them to think about the most disagreeable things they could. One officer back from the Front thought of the time when he was in a shell-hole with a bullet wound in his arm: he was there three hours, and it was very disagreeable indeed, but did not cause much response. Then I said: "Think of the War Office," and then it was all right!

That was the sort of experience I had with Dr. Dawson Williams: I had not asked him for that stimulus. With regard to adrenalin, I am afraid I know nothing about it. In reference to resistance in hot and cold weather, it varies with the emotion, and it may swallow up the physical changes. With regard to the delay, it is a skin delay. Children I have not yet worked at. Then there is the question of the exclusion of the sweat glands in the explanation. I do not precisely exclude them, because I think that the cells of sweat glands are protoplasm, and nerves act on sweat glands as they act on a lot of other things. It is the exclusive attribution of the phenomenon to the sweat glands to which I object: I believe it to be a much more general thing than to be concerned with the sweat glands. I think it is better to say the nerves act upon the membranes through which the commerce of nutrition is taking place: it is the poro-dilator action of these membranes, which consists in the facilitation of nutrition. With regard to the resistance story, I set up a galvanometer in my dressing-room to test my resistance at all hours of the day and night, because measurement of resistance is one of the fundamental principles which have got to be studied. It is what will save one from being "half-baked" about this. There is a beautiful diurnal change. In the morning our forces are at their shallowest: there is very little commerce taking place, very little nutrition. Whereas in the early hours of the afternoon, when we have our highest temperature, we are at our maximum physiological activity. The resistance varies during the twenty-four hours between 20,000 and 200,000 ohms: it is a variation amounting to 1,000 per cent. in the tensing which takes place. The lowest conductivity is in the small hours of the morning, when there are the narrowest pores, and the highest conductivity and widest pores in the small hours of the afternoon.

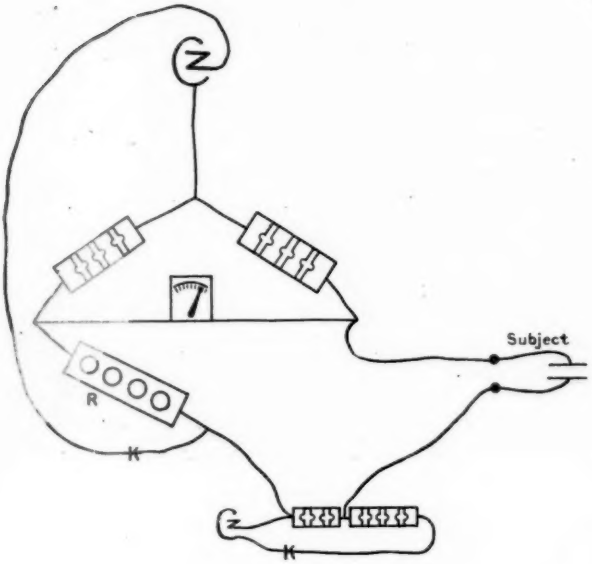


FIG. 1.

The hand of the subject is in the fourth area of a Wheatstone bridge. When any emotion occurs—whether spontaneous or excited—there is a diminution of resistance (poro-dilatation) of the skin of the palm of the hand. The accessory battery is used for calibration of the deflection.



FIG. 2.

Galvanometric observation of the emotivity of an English lady during the German air raid upon London of Whit Sunday, 1918. (The observation is reported in detail in the *Lancet*, 1918, i, p. 916.)

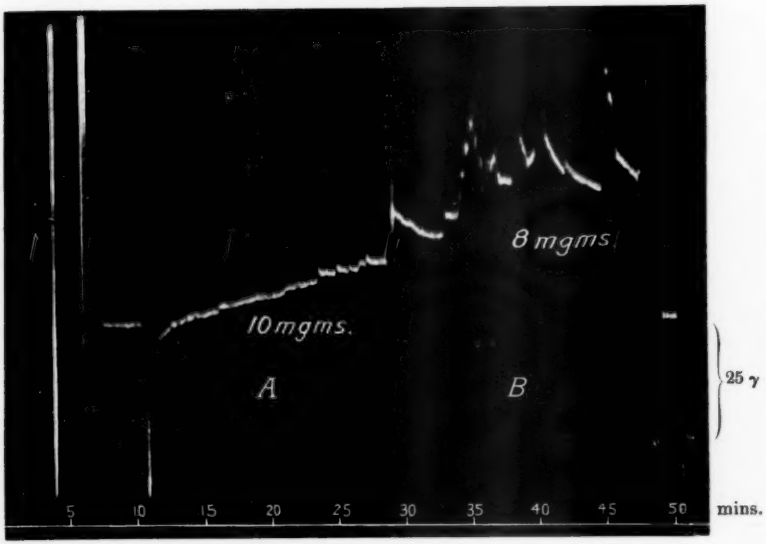


FIG. 3.

Galvanometric record of the emotivity of a Belgian lady: A, for fifteen minutes, during which she preserved a quiet mind; B, for 15 minutes, during which she recalled to mind scenes of which she had been an eye-witness at the German occupation of Termonde in 1914. The secretion of sweat by the palm of the hand was measured by chloride of calcium capsule fastened to one hand while the other hand served for the electrodes connected to the recording galvanometer; 10 mgr. of sweat were secreted during the quiet period, 8 mgr. during the painful period.

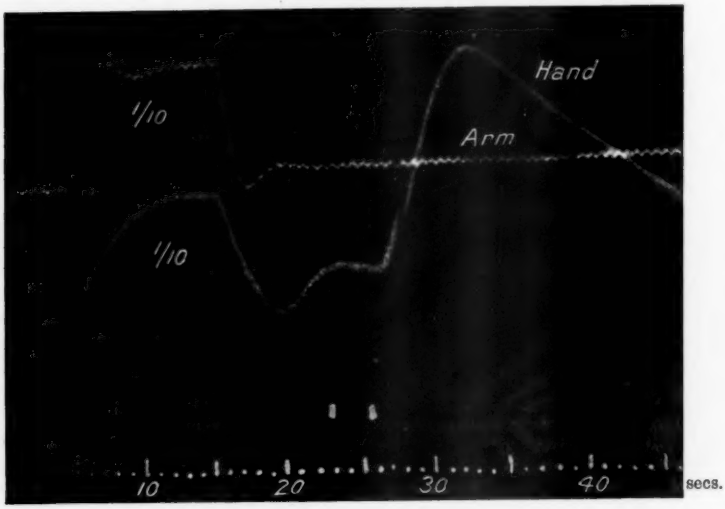


FIG. 4.

Simultaneous record by two galvanometers on the same plate of response from the hand and absence of response from the arm in consequence of a single strong induction shock applied to the leg.

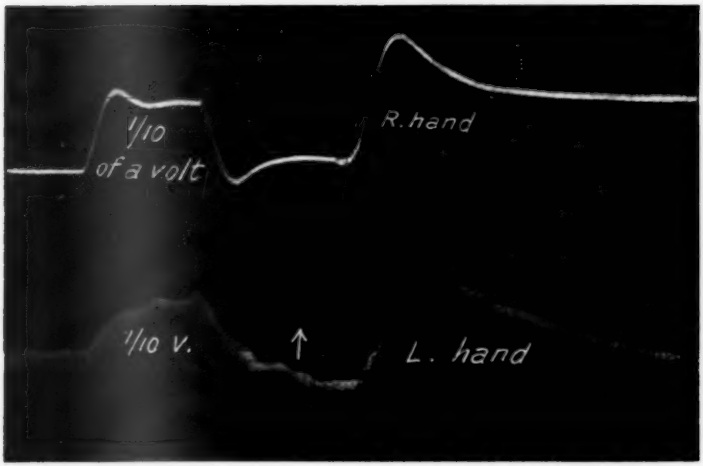


FIG. 5.

Simultaneous and approximately equal response in the two hands in consequence of a single strong (painful) induction shock applied to the right leg.

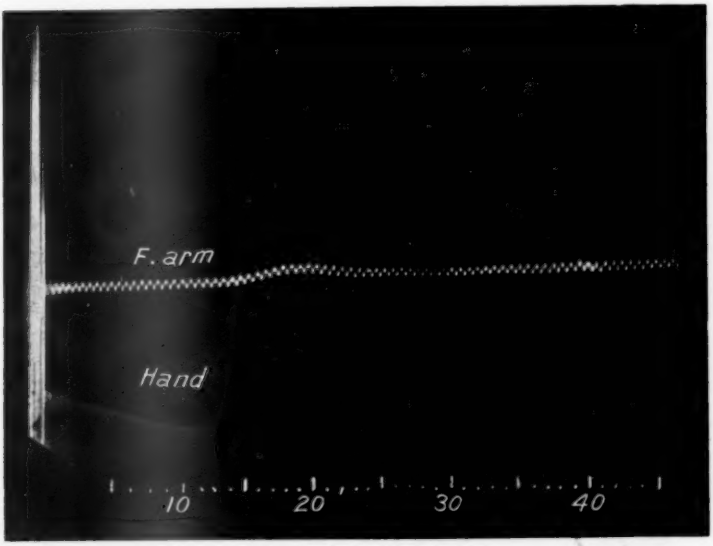


FIG. 6.

Simultaneous record of the response to the threat of a pin-prick with two galvanometers and two pairs of electrodes, one pair on the hand and the other pair on the forearm. (The tremor visible in the forearm record is only instrumental, probably from shaking of the laboratory by a lift.)

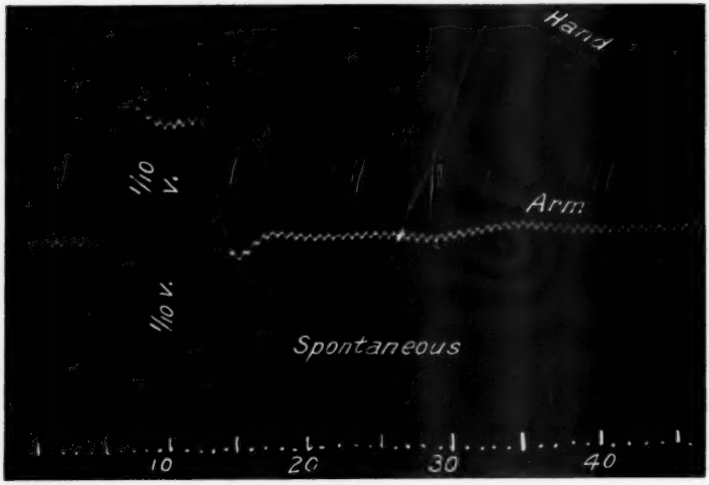


FIG. 7.

Simultaneous record of the response in the hand and in the forearm to a spontaneous (painful) thought. The preliminary deflections are by $\frac{1}{10}$th volt, and show that the two galvanometers are of about equal sensitiveness; the response of the hand is large, while there is hardly any response of the forearm. (The tremor seen in the forearm record is instrumental.)

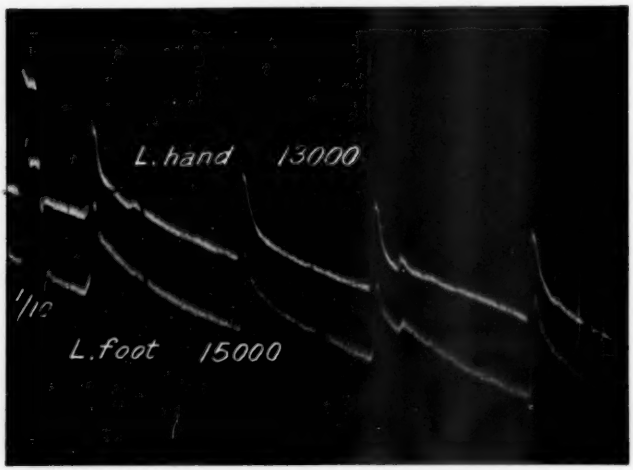


FIG. 8.

Simultaneous record, palm of hand and sole of foot to four strong (painful) induction shocks applied to the right leg.

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Medical Experiences in Macedonia and the Caucasus.[1]

By ARTHUR G. PHEAR, C.B., M.D., Colonel, late A.M.S.

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IN no other war area can there have been greater wealth of clinical material from the physician's point of view than in Macedonia and the Caucasus, and in this lecture I shall endeavour so far as time permits to give an account of some aspects of the medical work, which was throughout of absorbing interest. The only drawback was the difficulty, owing to stress of work, of making continued observations on a systematic basis, and I must ask the indulgence of the Society for the fragmentary nature of the records in many of the cases I shall have occasion to quote.

The accommodation in the base area for British sick during the summer of 1918 comprised fifteen general hospitals, most of them containing 1560 beds, a few with 2000 beds. In the preceding summer the number of hospitals was nearly as large, additional hospitals having been provided when it became necessary, owing to the difficulties of sea-communication, to modify the policy of evacuation on a large scale to Malta. The large number of beds thus provided was none too great, and often patients had to be discharged to convalescent depots earlier than one would have wished in order to maintain the necessary reserve of empty beds for fresh convoys. The hospitals and convalescent depots were situated away from the town, by the sea or among the hills, in comparatively healthy surroundings. A group of four hospitals, three

[1] At a meeting of the Society, held March 26, 1920.

of which were hutted, were placed at Kalamaria, on a low promontory, about 8 km. from "Piccadilly Circus," the centre in Salonika to which all roads converged. One of these hospitals occupied an ideal position overlooking the sea, with an uninterrupted view across the bay to Mount Olympus in the distance, and even on the hottest day relief was afforded by a fresh breeze from the sea, which made its appearance in the early afternoon with unfailing regularity. Here was the section for cases of tuberculosis, which as a rule did very well, and one often regretted the necessity of evacuating these cases, in order to provide much-needed beds, to less favourable surroundings in Malta or at home. Adjoining this, and on an equally good site, was one of the two hospitals for sick sisters. Another group of hospitals and convalescent depots was situated high up among the hills, about 16 km. from the town, on the plateau immediately below Mount Kotos, a well-wooded mountain of some 4000 ft. which dominated the Salonika district. A couple of hospitals were placed farther away across the bay to the south of Salonika, on the promontory of Karabaroun, with communication by water. This was, perhaps, the healthiest of all the sites, as the promontory was swept by sea breezes, the only drawbacks being the complete absence of natural shade, and a certain brackishness of the water, which had the effect on newcomers of a mild saline aperient. There was excellent sea-bathing for convalescents, and in addition to the hospitals the colony included two convalescent depots and a rest camp. It was necessary to close these hospitals during the winter owing to their exposed position and difficulties of communication in stormy weather. Supplementary to the British hospitals there were four general hospitals for Serbs, staffed entirely by British personnel, two of which were near Salonika and two at Vertekop, about two and a half hours' run on the Monastir road not far from the Serbian front. The fortnightly visits to Vertekop were always interesting, as, owing to temperament or circumstances, or both, the resisting powers of the Serb to disease were on a different level from those of the British, and common diseases frequently presented unusual features; also one saw examples of conditions, such as scurvy, that were practically unknown among our own troops. The most attractive medical unit in the command was at Stavros, a four hours' journey to the east, on the shores of a sheltered bay with high wooded mountains rising steeply from the sea. The tents were pitched by the seashore, and between them were well-grown plane trees which gave good shade; the water supply was abundant, and on the fertile soil all kinds of produce were grown for

hospital consumption. It was curious that a well-wooded site like this should be practically free from anopheline mosquitoes. Though nominally a field ambulance, the unit was practically on the scale of a stationary hospital. Closely rivalling Stavros in natural beauties was a unit in the opposite direction at Naoussa, beyond Vertekop, where a convalescent depot was established in connexion with the Serbian hospitals, high up among beech forests on the slope of a mountain which arose behind the depot to a height of over 6000 ft. Unfortunately this was too far away to be used to any large extent by the British, though accommodation was unofficially available for a limited number of officers, and was much appreciated by those who were run down and in need of a holiday.

Much has been said in disparagement of the climate at Salonika, but with all its drawbacks we might in many respects have been worse off. The heat in the summer was sometimes very trying, specially for those who had to reside in Salonika itself; the hills, which during spring and early summer were covered with a profusion of flowers, towards the end of June became parched and arid and of a monotonous faded yellow aspect, all vegetation being dried up with the exception of patches of the dwarf prickly oak. But outside the town the days were few when the heat did not become mitigated in the afternoon by the breeze from the sea, and the nights were usually tolerable. The winter climate was of two kinds—periods of three to five days during which the bitter "Vardar" wind blew from the snow-covered mountains to the north, bringing with it cold rain and a state of indescribable mud, or blizzards of fine-driven snow with twenty or more degrees of frost. During these spells transport was a matter of the greatest difficulty, and there was nothing in the climatic conditions to make life attractive except the certain knowledge that in the course of a few days matters would improve, and two or three weeks would follow of perfect weather, practically windless, with cold nights and warm sunny days, when convalescent patients could sit out and enjoy the sunshine.

Malaria.

It is no disparagement of the fighting qualities of the Bulgar to say that by far the most formidable foe we had to contend with throughout the Macedonian campaign was the mosquito. Compared with the havoc wrought among our troops by malaria, the casualties from wounds, even including the periods of greatest military activity, sank into relative insignificance. There may be some consolation in the reflection that

the mosquito is no respecter of persons, and levelled its attacks with impartial pertinacity against Bulgar and British alike.

The extent to which our troops suffered may be partly realized from the fact that the total admissions to hospitals for malaria during the years 1916, 1917, and 1918 amounted to 161560. Expressed as a ratio per 1000 of strength (fig. 1) this number is equivalent to 1053 per 1000, the ratios for the respective years being: for 1916, 331 per 1000; 1917, 353 per 1000; 1918, 369 per 1000. These figures, large as they are, give a very incomplete picture of the incidence of malaria, since they include only admissions to general hospitals, stationary hospitals, and casualty clearing stations. They do not include cases treated at field ambulances and discharged as fit from there, nor cases returning to

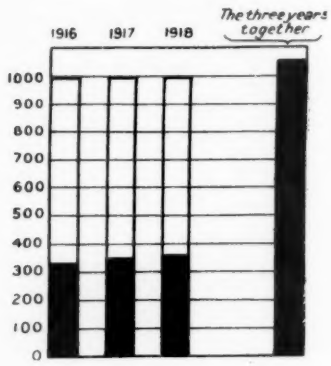


FIG. 1.

Malaria. Admissions to General Hospitals, Stationary Hospitals, and Casualty Clearing Stations. Shown as a ratio per 1000 troops.

duty after treatment by regimental medical officers, nor do they take into account the numerous instances in which officers and men with slight attacks carried on without reporting sick. The mortality was at the rate of 2·02 per 1000 troops during 1916, 1·17 per 1000 during 1917, and 1·69 per 1000 during 1918. The last figure is probably too high, as there was an enormous increase in the number of reported deaths from malaria in the earlier part of the influenza epidemic, and the figures undoubtedly include many cases in which death was due, not to malaria, but to influenza or its complications.

The seasonal incidence of the disease is well shown by the accompanying curve (fig. 2), which is based on the return of weekly

admissions during 1917 shown as a ratio per 1000 of strength. It is seen that the first material increase in the number under treatment, representing the earliest fresh infections of the year, was at the end of June, from which date there was a steady rise to a maximum in the beginning of October, followed by an abrupt fall during the latter part of that month and continued through November. The cases responsible for the early part of the flow of the malarial tide are exclusively benign tertian. This is probably due to the fact that subtertian infections die out in the course of the winter, and that from February onwards relapses are generally attributable to the more resistant benign tertian parasite. The benign tertian relapsing cases

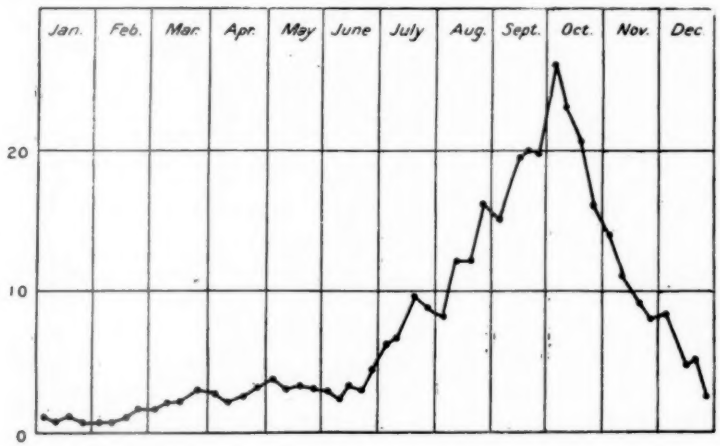


FIG. 2.

Malaria. Weekly admissions shown as a ratio per 1000 of strength, 1917.

act as carriers from which the mosquitoes become infected as their activity increases in early summer. About the latter part of August the number begins to be appreciably swelled by the inclusion of malignant tertian cases. Coincident with this is a distinct change in clinical type; the severity both of benign tertian and malignant tertian cases is greater, cerebral cases become more frequent, and the death-rate rises. It has been impossible to determine the exact ratio of benign tertian to malignant tertian cases, as such a comparison could only be effected by a complete series of blood examinations; the amount of pathological work entailed by this procedure would have been over-

whelming, and in 1917 an order was made that in cases where the diagnosis was clear on clinical grounds blood films were not to be sent to the laboratories. The curve shown in fig. 3 was constructed by Colonel Wenyon, C.M.G., C.B.E., A.M.S., from various data, with the object of giving a relative view of the seasonal incidence of benign tertian and malignant tertian infections during 1917; it must not be taken as possessing exact numerical accuracy. During the early part of the malarial season the prevailing species of mosquito is *Anopheles maculipennis*. With the appearance of malignant tertian malaria in August the mosquito ranks are swelled by the addition in

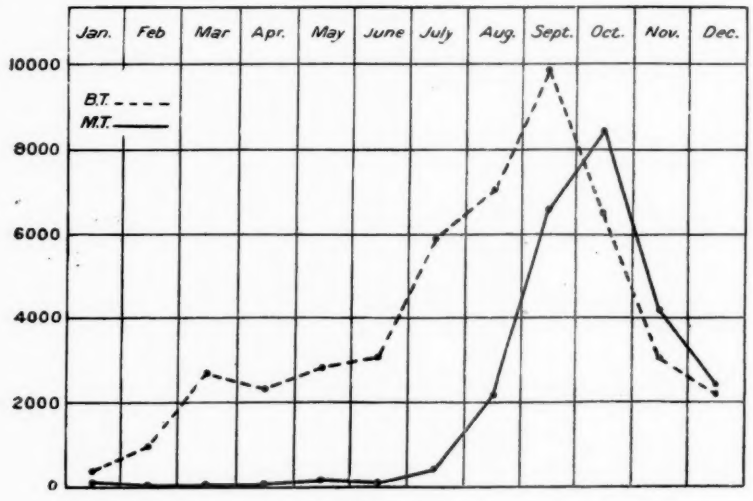


FIG. 3.

Calculated number of cases of benign tertian and malignant tertian malaria in hospitals during 1917.

large numbers of *Anopheles superpictus*. It might be supposed that there is some causal connexion between the two events, but I am told by Colonel Wenyon that there is no direct evidence that *Anopheles superpictus* possesses any superiority over *Anopheles maculipennis* as a carrier of malignant tertian infection; both species appear to infect themselves equally readily with malignant tertian parasites.

Clinical Features.—In attacks of primary benign tertian malaria the clinical phenomena usually formed so definite and characteristic a picture as to be recognizable even by those who had had no previous

practical knowledge of the disease. But the same cannot be said of the manifestations of many cases of subtertian and recurrent malaria, and a very short experience in Macedonia sufficed to bring home the difficulties with which diagnosis might be beset. In malaria the parasitic invasion, which is a general one, often falls with disproportionate emphasis on some special tissue or organ, thereby producing a symptom-complex which may lead to a remarkable simulation of other forms of disease. Thus cases were admitted with symptoms of an acute enteritis, with frequent watery stools, sometimes fæculent, sometimes containing blood and mucus. The clinical resemblance to dysentery might be very close, but no dysenteric organisms were present, and in many cases an examination of the blood disclosed evidence of malaria. The condition yielded rapidly to prompt treatment with quinine, and there is every reason for regarding these as cases of malarial enteritis. In other cases the intestinal symptoms were more severe, the stools being profuse and watery and associated with vomiting, collapse, and cramps, simulating cholera. Other cases were seen of prolonged pyrexia of indeterminate type in which one or another member of the enteric group was suspected, and it was only after weeks of illness and repeated blood examinations that the nature of the case could be proved by the demonstration of parasites in the peripheral circulation. A mistake made by almost everyone at some stage of his experience was the diagnosis of appendicitis in cases of malaria where initial symptoms of marked pain and tenderness with muscle rigidity in the right iliac region were prominent, and in some of these instances an opportunity was provided by the surgeon of proving by direct observation the healthy character of the appendix. The absence of anything like a definite tumour, and the results of blood examination, including differential count of leucocytes, were the distinguishing criteria in these cases, though it must be allowed that exceptionally the simulation of appendicitis was so close that the matter could only be decided by an exploratory laparotomy. Further pitfalls for the unwary were provided by cases of malaria with lumbar pain and a transient hæmaturia, rapidly subsiding under quinine; cases with symptoms of acute nephritis also rapidly controlled by quinine; cases with acute gastric symptoms and hæmatemesis; cases of acute pleurisy with effusion, the fluid sometimes containing blood; cases with acute pulmonary symptoms, cough and copious purulent sputum often streaked with bright red blood, and signs of a patchy consolidation of the lung; cases with acute mental symptoms; cases with coma of abrupt onset resembling apoplexy; cases with epileptiform fits, or

symptoms suggesting a cerebellar or a bulbar lesion; cases simulating meningitis with prostrating headache, head retraction, and rigidity of neck muscles; in an example of this, occurring on an hospital ship and diagnosed as a fulminant form of cerebro-spinal fever, the blood examination showed a heavy infection with benign tertian parasites.

It is not intended to discuss the exact pathology of these conditions, nor to make any attempt to define the extent to which the symptoms in some instances may be due to a complication of the initial malarial invasion by an added bacterial infection. What I wish to emphasize is the importance, in the first place, of not overlooking the malarial factor, and secondly, of giving due weight to this factor in determining the treatment to be adopted.[1]

With a widening realization of the manifold and far-reaching activities of the malaria parasite, there arose an almost inevitable tendency on the part of the clinician to fall into a different error, and, attaching undue weight to probabilities, to arrive too readily at a diagnosis of malaria in doubtful cases. Such diverse post-mortem conditions as cerebral abscess, meningitis, hepatic abscess, gastric ulcer with perforation, appendicitis, were found in cases diagnosed as malaria. These mistakes were sometimes due to lack of thorough examination, and therefore unjustifiable; but, on the whole, it is remarkable that the standard of diagnosis was so high, and serious mistakes were so infrequent, considering the strenuous conditions of work in the hot season when hospitals were crowded and heavy convoys were coming at short intervals, and bearing in mind that of medical officers arriving in the country there were very few indeed who had any previous experience of tropical diseases.

The difficulties of diagnosis were enhanced in the influenza epidemic when the two infections frequently ran concurrently in the same individual; and the clinical threads became still more tangled when, as happened at one time, a considerable outbreak of sandfly fever was added to the prevailing epidemics of malaria and influenza. There

[1] Lieutenant-Colonel Falconer, D.S.O., C.B.E., R.A.M.C., in an article on "The Pulmonary Manifestations in Malaria" in the *Journal of the Royal Army Medical Corps*, February, 1920, describes a group of cases presenting the physical signs of more or less extensive consolidation of one or more lobes. He considers that in most of these cases the physical signs depend on massive collapse and not on true consolidation, and regards the condition as representing a pure malarial phenomenon. I was unfortunate in seeing very few cases in which the physical signs could be interpreted as due to pulmonary collapse, and formed the opinion that the most common pulmonary complication of malaria was a fine-tube bronchitis, with patchy consolidation of one or both lungs, probably due in all cases to a superadded infection.

were many difficult cases in which the only proof of malaria lay in the demonstration of the parasite in the blood. An ideal course would have been the examination of a blood film in every case; but with such large numbers this procedure was impossible; and in order to relieve laboratories of unnecessary strain it was ruled that blood films were not to be taken in straightforward cases progressing favourably under quinine treatment. The information afforded by examination of the blood was especially useful in the more chronic forms of malaria with irregular pyrexia, in which there might be long periods without demonstrable parasites in the peripheral blood. The diagnosis in these cases was often proved by repeated blood examinations, resulting sooner or later in a positive finding. In the apyrexial stage the information afforded by a white blood corpuscle count was always valuable, as the combined relative preponderance of large mononuclear cells, with reduction in the total number of white cells, gave a picture which was the reverse of that obtaining in many of the acute inflammatory conditions from which malaria had to be distinguished. Enlargement of the spleen taken by itself was of little help; but a palpable and tender spleen, or tenderness over the splenic area combined with increase upwards of the splenic dullness was valuable evidence of acute malaria.

The *after-effects* of malaria were numerous, and examples were commonly to be seen not only in the wards of general hospitals but also in the various special departments of the medical service. In the cardiological department a large proportion of the cases of D.A.H. with unduly sensitive exercise-response were traceable to malaria. In the neurological department, in addition to cases of peripheral or central lesions of the nervous system directly due to malaria, the influence of the disease as a contributory factor in various functional conditions was clearly manifested. In the mental department a majority of the cases under treatment was attributable to the same cause. In an analysis published in the *Lancet*, of January 3, 1920, by Major Forrester, R.A.M.C., who was latterly in charge of the department, it appears that the commonest type of psychosis following malaria was either some form of mental confusion, or depression. The outlook in these as in other post-malarial psychoses is favourable, and after a course of treatment it was generally found possible to transfer cases to the general wards whence they could be sent home by hospital ship as ordinary patients.

No one passing through the malaria wards of any general hospital could fail to be impressed by the large proportion of cachectic cases, with profound anæmia, enlarged spleen, marked wasting, more or less

pyrexia, and persistent tachycardia increased on any slight exertion. Very low blood counts were recorded, e.g., 900000, 880000, 810000, and in one case 690000. Accurate hæmoglobin estimations were unfortunately seldom available as many of the instruments had deteriorated through climatic influences and the colour readings were not reliable. In some instances the blood picture was of the " pernicious " type, with poikilocytosis, megaloblasts, and a high colour index. In others the clinical picture was that of pernicious anæmia, plus a large spleen, plus myelocytes in the blood. In other cases there was an aplastic anæmia with little or no attempt at repair. In two cases at least blood was transfused. One of these, a patient with malignant tertian malaria and profound anæmia, under the care of Major A. C. Sturrock, R.A.M.C., was transfused twice, 18 oz. of blood being introduced on the first occasion, 24 oz. on the second. Previous to transfusion the patient was going rapidly downhill and it seemed that a fatal issue could not long be delayed. The immediate effect of the first transfusion was very striking, but it led to no permanent response on the part of the blood-forming tissues, and again the patient lost ground and a fortnight later was rapidly approaching his former condition. The second transfusion was followed after a short interval by a gradual and lasting improvement in the blood state, the first evidence of returning functional activity being a remarkable increase in the white corpuscle count from 3200 to 12300, this leucocytic phase definitely preceding the period of red cell reconstruction. The patient was evacuated by hospital ship three months after his first transfusion. A full account of this interesting case will be duly published by Major Sturrock, who has kindly supplied me with the above details.

Treatment.—I do not propose to spend time in describing the routine measures of treatment which came to be adopted as the result of our experience of malaria in Macedonia. I have dealt with these in some detail in a recent article in the *Lancet* (January 24, 1920), and will here limit my remarks to the discussion of certain points of general interest.

In endeavouring to estimate the value of quinine in the treatment of malaria one is at once impressed by the remarkable contrast between its controlling influence over the acuter manifestations and its comparative failure to prevent relapses in the more chronic forms. In acute malaria the results obtained by quinine properly administered and in sufficient quantities are little short of marvellous. As our experience grew the tendency was to give increasingly large doses in primary

attacks, as it was found that this was the only stage of the disease at which permanent results in the sense of preventing relapses could be hoped for. In cerebral malaria the prompt use of intravenous quinine undoubtedly saved many lives. But as a means of controlling the incidence of relapses in recurrent malaria the efficiency of quinine was found to be on a very different plane. Numerous observations were made, some on a considerable scale, having for their object the comparison of large bodies of men, others in special malaria wards, where with comparatively small numbers to deal with it was possible to carry out a systematic comparison of various methods on a scientific basis. The practical outcome of an immense amount of work was the realization of the impossibility of eradicating the malarial parasite from the system in cases of relapsing malaria by any known method of treatment. While quinine was being taken in effective doses the disease was kept more or less under control, but on the cessation of treatment relapses again made their appearance, and after a few weeks became as frequent as in the period before the treatment was begun. The best results in relapse-control were obtained in the special observation wards under the direction of Major A. G. Anderson, R.A.M.C., by the "week-end" system of 30 gr. of quinine on each of two successive days in each week; an important advantage of the method is that treatment can be continued for an indefinite period. Under this system the percentage of non-relapsing cases was raised to 78 as compared with 32 per cent. in cases in which no quinine was given except for the treatment of relapses as they occurred. But for all its limitations no drug was found to exceed or even to approach quinine in efficiency. At one time, with the introduction of galyl it was hoped that a parasite destroyer of considerable power had been discovered, but the promise of early trials was not fulfilled, and although the preparation undoubtedly possesses some parasite-destroying properties, specially in subtertian cases in which quinine has been pushed to the limits of tolerance, its chief value was found to lie in stimulating the blood-forming organs in cases of profound cachexia and in the anæmia of blackwater fever.

On the much debated subject of the channel by which quinine may with the greatest advantage be introduced, a very decided impression was formed that a given quantity of quinine is more effective if injected intramuscularly than if taken by the mouth. It is difficult to say how far this impression was correct; observations made in the Special Malaria Laboratory on the rapidity with which parasites disappear from the blood and the rate of quinine excretion suggests that the advantage

lies with oral quinine. Personally, I hold the view that if the desired effect can be obtained by oral quinine there is no justification in submitting the patient to the inconvenience of intramuscular injections. But instances in which quinine requirements could not be met by the oral method were very numerous, and the value of the alternative channel was amply proved in such conditions as vomiting or intestinal disturbance, when absorption from the alimentary tract is impaired, and in cases of pernicious type, where it is imperative to produce a greater effect than can be obtained by mouth administration only, and in cases of cerebral malaria as a supplementary measure to intravenous injections. The risk of local complications is practically negligible if ordinary precautions are taken to prevent such mishaps as sepsis and injury to nerve trunks. In a recent number of the *Journal of Hygiene* (October, 1919), Colonel Dudgeon, C.M.G., C.B.E., A.M.S., has drawn attention to the serious degree of necrosis which follows intramuscular injections of quinine in mules and rabbits, and has suggested that medical officers and officers in charge of divisions in the Salonika hospitals were not fully alive to the injuries they might inflict on their patients. It may be said that, apart from cases of faulty technique, and a few instances of extensive necrosis that followed intramuscular injections in patients who were debilitated from the effects of dysentery, such untoward results were practically unknown in our clinical experience. The number of injections given was enormous and there is no means of accurately estimating them; but assuming that each of twelve medical officers in each general hospital had occasion to give one intramuscular injection on each day during the malarial season of, say, one hundred days—an assumption which is undoubtedly considerably below the mark—the total number of injections for the season would amount to 18000. And yet with the exceptions mentioned above, it was the rarest event for any ill-effect to follow, beyond a localized induration which rapidly subsided, leaving no permanent disability.

The treatment of malaria in patients who at the same time were suffering from dysentery presented special difficulties, as on the one hand oral quinine was badly absorbed and often of little use, and on the other hand there was the risk of intramuscular injections, if repeated, leading to serious damage to the muscle. With this combination of diseases it was often impossible to push quinine to the same extent as in a case of uncomplicated malaria. Many of the acute Shiga cases showed a marked febrile reaction, and in order to assess the pyrexia at its true value the examination of blood films was found to be quite as important, if not more so, in the dysentery hospitals as in any other.

Of unfavourable symptoms as the result of the large doses of quinine which it was often necessary to give we had fortunately small experience. A few cases of idiosyncrasy were seen in which a moderate or even a small dose of quinine consistently gave rise to hæmoglobinuria. An instance of this was met with in a Greek boy, who had had malaria and was given a dose of quinine with resulting hæmoglobinuria and the rapid development of a very marked anæmia. It was thought the matter might be a coincidence, and as there were special reasons for quinine treatment a further dose of 10 gr. was given after a short interval with the same dramatic result of an intense hæmolysis and severe anæmia. This condition should be sharply differentiated from blackwater fever which I believe comes and goes quite independently of quinine. A variety of skin lesions, such as erythema, scarlatiniform rashes, urticaria, and less commonly purpura occurred in definite association with quinine treatment. More serious than these was a condition of exfoliative dermatitis, of which I saw two examples. In each there was a protracted illness, in the course of which the exfoliative condition became generalized, and ultimately septic infection occurred notwithstanding the most careful nursing, and the patient succumbed. The total duration of illness in one of these cases was six months.

In the course of 1917 twelve cases of quinine amblyopia were reported. Further cases occurred in 1918 but I do not know the exact numbers; they were fewer than in the previous year. Of the 1917 cases two followed the taking by mistake of a large but unknown quantity of quinine; in one two "mouthfuls," in the other four to six tablespoonfuls of quinine in solution were taken. The remainder were cases of severe malaria in which the need for pushing quinine in large doses was imperative. Most of these were cases of subtertian infection; in three the clinical condition was complicated by blackwater fever. One patient, an officer, while on 30 gr. daily, developed cerebral symptoms and was given while unconscious 70 gr. of quinine intravenously and 90 gr. intramuscularly, 160 gr. in all. On regaining consciousness he was amblyopic. There was a gradual but slow return of vision, and when evacuated by hospital ship several weeks later there was still a considerable degree of amblyopia. Another case, of malignant tertian malaria with multiple rings, was given on October 14, 30 gr. of quinine intramuscularly, on October 15, 60 gr., half intramuscularly, half by mouth, and on October 16, 60 gr. intramuscularly. After the evening dose he could hardly see, but the pupils reacted well to light. At 8 a.m. on October 17 he was totally blind with widely

dilated pupils which did not react to light. In the evening he could see a little. On October 18 he was delirious, but apparently could see. On the next day his malarial symptoms were so urgent that it was decided after careful consideration to give more quinine, and 15 gr. were administered intramuscularly. After the injection there was rapid dilatation of the pupils and he became totally blind with pallor of the discs and contraction of the retinal arteries to a mere thread. On the following day he was given galyl but in the evening the general condition was so grave that it was decided that further quinine treatment was imperative, since it afforded the only hope of saving his life, and that it must be undertaken even at the risk of permanent damage to the sight. Further doses of intravenous quinine were given, but the treatment was unavailing and the patient died. In a third case of cerebral malaria 55 gr. of quinine were given on November 21, 60 gr. on the 22nd, and 20 gr. on the 23rd. He then became completely blind, and when I saw him a few hours later the pupils were widely dilated, failed to react and the fundus was pale with very small threadlike arteries. No further quinine was given after the onset of blindness, but notwithstanding this there was not the slightest recovery of vision and the patient was evacuated by hospital ship several weeks later, still blind. In this case continuous sluggish movements of the iris were observed independently of light stimulus. These cases fall into three groups: (1) Those in which there is fleeting amblyopia followed by full recovery of vision; (2) cases of incomplete recovery in which useful vision is restored but with a varying degree of lasting contraction of the fields; (3) a comparatively small group in which blindness is permanent.

Y Scheme.—Owing to increased submarine activity in the first half of 1917, facilities for invaliding by sea became greatly restricted, and there ensued a gradual accumulation in the Salonika command of men whose usefulness for military purposes was practically *nil*, since by reason of repeated attacks of malaria they spent the greater part of their time at hospitals and convalescent depots, in many cases being readmitted from convalescent depots to hospital for a malaria relapse before their period of convalescence was completed. These men were not only unfitted for active service and were an unnecessary burden on the military machine, but as possible carriers they constituted an actual danger to the community. On the other hand there was a fair prospect that under less trying climatic conditions a proportion of them would recover to a sufficient extent to be of use in the home command or in France. These circumstances led to the inception and organization of

the Y scheme, and by the latter part of December, 1917, the necessary arrangements had been completed and the first drafts were evacuated from Salonika under the scheme. The men were chosen with the greatest care, every case being inspected either by the D.M.S. or the consulting physician before being passed as suitable for evacuation. The scheme worked well, and by October 31, 1918 a total number of 26001 officers and other ranks had been evacuated, to the great advantage both of the command as a whole and of the men themselves. I am told that these officers and men, not only useless but a danger to their companions in the East, have in a large proportion of cases regained their health and strength, and have been responsible for much good work both at home and on the western front.

Malaria in the Caucasus.—The shifting of the troops, after the armistice, from Salonika to the neighbourhood of Constantinople and to the Caucasus was followed by a marked improvement in the health of the Army and at the same time there was a considerable change in the character of the sickness. The dominance of malaria over all other conditions dwindled, and diseases such as typhus, small-pox and relapsing fever, of which only occasional isolated cases cropped up in Salonika, made their appearance in increasing numbers during the early spring of 1919. Thanks to protective measures and early isolation of cases these diseases were always under control, and did not assume anything approaching the dimensions of the epidemics by which large sections of the Turkish and Russian armies were devastated. The approach of the malaria season was awaited with some curiosity, as we were anxious to see how far the prediction of a heavy malarial incidence in this new country would be fulfilled. In June, when I had an opportunity of visiting the medical units in the Caucasus, except for recurrent cases carried over from previous seasons, there was no malaria. My first note of primary malaria was on July 14 when a man from Batoum, who came out from home in December, 1918, was admitted to a general hospital in Constantinople with a benign tertian infection. There may have been a few isolated cases before this, but, in contrast to our experience in Salonika, primary malaria in the early summer in the Caucasus was a negligible quantity. On a subsequent visit in the latter part of August the state of things was very different, and there were serious outbreaks of malaria in many parts of the country. At an aviation post at Petrovsk, on the shores of the Caspian, fifty out of 150 men had gone down in the course of a fortnight. A battalion on its way to Shusha, a post among the hills, had its orders changed while *en route*, and encamped at Barda

on the plain, on a site chosen among other reasons for its propinquity to a stream. I was told by the C.O. that no mosquitoes were noticed until about ten days later, when men began to go down with malaria. I visited the battalion a few weeks later after they had moved to a healthy camp on high ground above Tiflis, and by that time out of a total of 305 N.C.O.'s and men 303 had fallen victims to malaria. A large proportion had been sent into hospital, but fresh relapses were occurring every day, and it was with great difficulty that fatigue parties could be mustered for the performance of the ordinary camp duties. At this period there was a general movement of troops in progress from Baku and Tiflis to the Black Sea coast at Batoum, where malaria was also prevalent, most of the available camping ground being heavily infested by anopheline mosquitoes.

Blackwater Fever.

During the year ending October 31, 1918, 136 cases of blackwater fever were reported in the Salonika command; of these thirty-six died, giving a case mortality of 26·5 per cent. In the previous year only eighteen cases were reported. The circumstances of the two years are not strictly comparable, as in the course of 1916 malaria cases in large numbers were being invalided to Malta, and during the following winter blackwater fever was common in Malta. Owing to submarine risks and consequent restrictions in sea communication between Salonika and Malta the evacuation of malaria cases on a large scale became for the time impossible, and the accumulation of these cases in Salonika was doubtless largely responsible for the rise in the blackwater figures. The chart in fig. 4 shows the seasonal incidence of the disease; it is seen that the great majority of cases occurred between December and April with a maximum in February, and that during the summer there were no cases except for a short outbreak during September. This happened to correspond with the military advance, and may be related to the strenuous conditions and special hardships of that time.

During the winter there were three short spells of exceptionally severe weather. The first of these was in early December and coincided with the beginning of the blackwater season; the second was in early January; and the third, at the end of March, may have been responsible for checking the fall in the number of blackwater cases that had already taken place. There was no specially severe weather in the last three weeks of January, or during February, when blackwater fever attained its maximum incidence.

The outstanding feature in the ætiology of blackwater fever is its relation to malaria. Of a series of seventy-eight cases which I had special opportunities of investigating, in all except one a definite history of malaria was obtained, and seeing how prevalent malaria was, it is more than probable that this single case was really no exception to the rule that malaria is an invariable antecedent of the condition. But notwithstanding this general relationship, it was rare to find evidence of more than slight malarial activity during the actual blackwater attack. In only 27 per cent. of a series of fifty-eight cases in which special care was given to the examination of the blood were there positive findings, and in nearly all of these the number of parasites

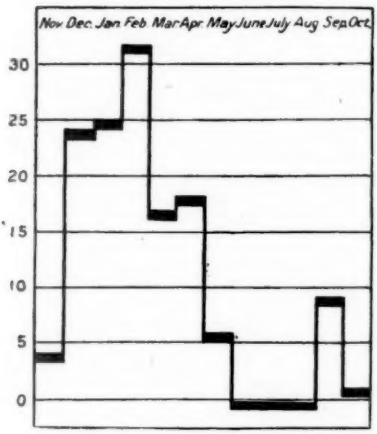


FIG. 4.

Blackwater fever. Number of cases reported monthly during the year ending October 31, 1918.

present was very small. The type of parasite was benign tertian in seven cases, malignant tertian in four, and indeterminate forms, probably malignant tertian, in five cases. That some agent other than the malarial parasite is the determining factor in the acute hæmolysis of blackwater fever is further suggested by its seasonal incidence, which in Macedonia is at its height, not during the malaria season, but at a time of year when malarial activity is at its lowest level. What the determining factor is, remains at present obscure. No support was found for the hypothesis that quinine is the activating agent. On the one hand blackwater occurred in cases in which no quinine had been

taken for a considerable period; on the other hand in several cases quinine was given in large doses without prolonging the period of blackwater; it was the practice of some medical officers, in the belief that blackwater fever was the expression of a specially malignant type of malaria, to treat the condition energetically with quinine, and I have many charts which illustrate the rapid and complete clearing of the urine under these conditions. Clinically the cases presented no special features. The abrupt destruction of enormous numbers of red corpuscles, leading to a drop to 30 per cent. or even 20 per cent. of the normal number; the state of collapse with extreme pallor and small compressible pulse; the rapid darkening of the urine with characteristic absorption

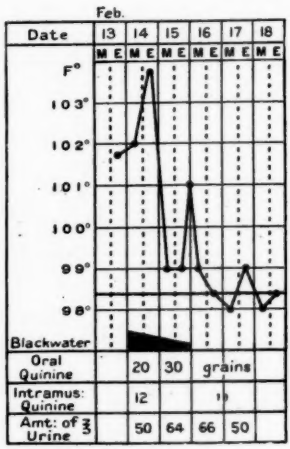


FIG. 5.

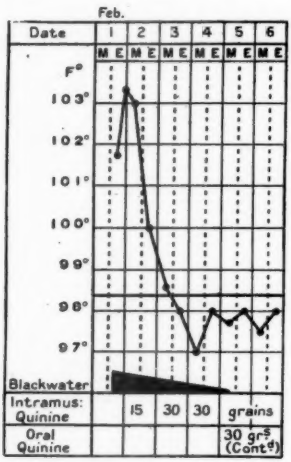


FIG. 6.

Fig. 5.—Blackwater fever. No quinine had been taken for seven weeks previous to onset of blackwater. The urine rapidly cleared while quinine was being given.

Fig. 6.—Blackwater fever. Rapid clearing of urine during administration of quinine.

bands; the pyrexia with shivering, vomiting, headache, and lumbar pains all went to form a picture which differed in no essential from that described in connexion with outbreaks elsewhere. The average duration of hæmoglobinuria was three days; the longest period was five days; in one case characteristic dark red urine was passed on a single occasion only; all subsequent specimens were clear. Two symptoms we learned to regard as of specially grave significance, jaundice and suppression of urine. Enlargement and tenderness of the liver with slight icteric

staining of the skin was a common feature of nearly all cases; but deep jaundice was a symptom only of the more serious cases and was nearly always followed by death; in every one of thirty fatal cases there was well marked jaundice; in seventeen of these the icteric staining was intense. In all the fatal cases except two there was either complete suppression of urine or the amount was reduced to a few ounces *per diem*. In the two exceptions a fair secretion of urine was maintained to the end; in both of these cardiac dilatation with pale and flabby muscle was found post mortem. Suppression was often associated with persistent vomiting and intractable hiccough causing great distress to the

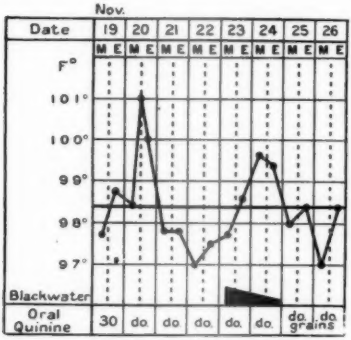


FIG. 7.

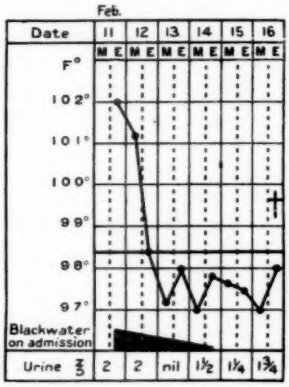


FIG. 8.

Fig. 7.—Blackwater fever runs its course independently of quinine treatment.

Fig. 8.—Blackwater fever. No quinine for six weeks previous to attack. Almost complete suppression of urine. Blackwater clears two days before death.

patient and rapidly leading to exhaustion. In some cases hypodermic injections of hyoscine ($\frac{1}{100}$ gr.) combined with morphia ($\frac{1}{6}$ gr.) were successful in controlling these symptoms when other means had failed.

The most important measures in treatment were found to be warmth and the introduction of large volumes of fluid either by mouth, rectum, subcutaneously, or in exceptional cases intravenously, with the object of diluting toxins and maintaining a free flow through the kidneys. The use of Souttar's apparatus was of great value as by this means continuous rectal administration of saline can be effected, and as much as 5 or 6 pints introduced in the course of twenty-four hours. The life of one patient, a nurse, under the care of Major A. G. Anderson, R.A.M.C.,

was undoubtedly saved this way. The general rule in regard to quinine was to withhold it unless there was evidence of concurrent active malaria. If parasites were present, or the spleen was enlarged and at the same time tender, quinine was given in full doses and with beneficial results.

The charts (figs. 5 to 9, pp. 74-76) selected from a series published in the *Journal of the Royal Army Medical Corps*, January, 1920, illustrate some of the above remarks.

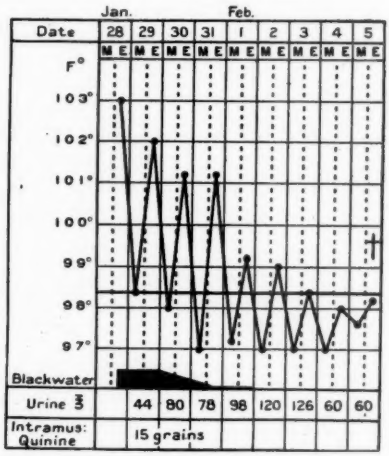


FIG. 9.

Blackwater fever. Abundant urine up to date of death. Death from cardiac failure.

DYSENTERY.

Next in numerical frequency to malaria, and hardly less important in its consequences, was dysentery. The total number of admissions for the three years was 21147, representing a ratio per 1000 troops of 64 in 1916, 29 in 1917, and 58 in 1918. The total number of deaths was 414, equivalent to 1·9 per cent. of admissions; or caculated as a ratio per 1000 troops, 1·4 in 1916, 0·6 in 1917, and 0·98 in 1918 (fig. 10). The above figures are probably in excess of the actual numbers, specially for the year 1918, as, owing to the importance of early treatment and the impossibility of bacteriological examination in all cases, it was agreed to accept as the basis of diagnosis clinical criteria irrespective of bacteriological proof. This policy was deliberately adopted as affording the maximum of protection to the community,

though it doubtless involved a diagnosis of dysentery in a certain number of cases of acute enteritis which were really due to some other cause.

There are unfortunately no figures available to show the relative frequency of the amœbic and bacillary forms of the disease. The latter was the dominant form, and though amœbic cases were present throughout, it was only late in the season of 1917 that the extent to which this form prevailed in the command came to be recognized, partly owing to the indefatigable work of a protozoologist who spent his whole time in visiting laboratories, giving his opinion on doubtful specimens, and advising as to methods. Evidence gradually accumulated of the existence of a widespread amœbic infection in its most insidious form, being carried by apparently healthy individuals, with or without a history of previous dysentery, and often suffering from no

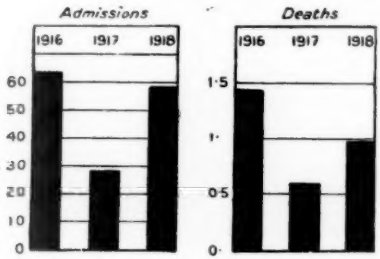


FIG. 10.

Dysentery. Admissions and deaths shown as a ratio per 1000 troops.

disability beyond occasional attacks of diarrhœa. The policy with regard to evacuation of dysentery cases became more and more stringent; first, all proved cyst carriers were sent home; then all cases which came under observation with a history of repeated attacks of diarrhœa; then all dysentery cases with a history of a previous attack; and finally (in 1919) all cases of dysentery, amœbic or bacillary, first attacks or otherwise, clinical or bacteriologically proved, were evacuated by hospital ship. These successive measures indicate the importance which came to be attached to the rôle played by carriers in spreading the disease in both amœbic and bacillary forms. It is impossible to say how far these measures might have succeeded in reducing the disease had troops remained in Salonika during 1919 in the same numbers and under similar conditions; with the rapid evacuation of

troops after the armistice and the shifting of headquarters to Constantinople the conditions were radically changed; but it is worth noting that the type of dysentery seen in the hospitals in the Caucasus during 1919 was very different from that which we had been familiar with in Salonika; the bulk of the cases were mild and of short duration; serious cases were uncommon, and the death-rate for the year was only 0·43 per 1000 troops.

Of equal importance to human carriers, and commonly forming an intermediate link in the chain of infection, came flies. The plague of flies was at its worst at two periods of the year, in the early summer, and in the autumn; at both these seasons the heat was accompanied by a relatively high humidity; during the intervening months of great

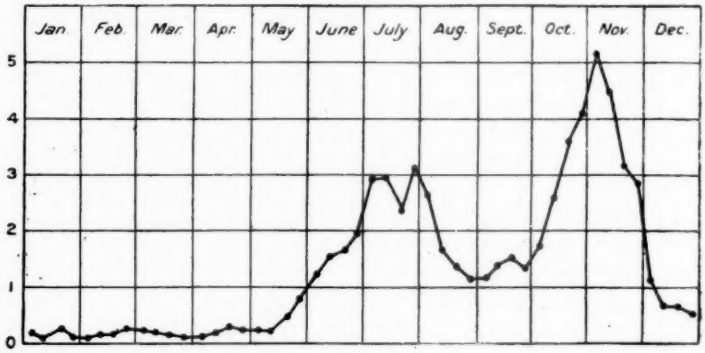


FIG. 11.

Dysentery. Weekly admissions during 1918 shown as a ratio per 1000 of strength.

heat with dryness the fly nuisance was largely mitigated. Turning to the dysentery curve, it is seen to present a precisely similar seasonal variation, with an initial outbreak in the spring, reaching its maximum in late June and July, followed by a substantial reduction in the number of cases during August and September, and a recrudescence of disease in the autumn, at its height at the end of October or early November (fig. 11). This coincidence, obviously on the larger scale, was worked out on a more exact basis for a group of hospitals near Salonika by Captain J. F. Taylor, R.A.M.C., whose observations go far to incriminate the fly as a carrier of dysentery organisms. Not only did he prove an exact parallelism between the incidence of dysentery in these hospitals and the prevalence of flies as measured by the numbers caught

in traps which were exposed from 11 a.m. to 2 p.m. on each day, but he also showed experimentally that the fly is capable of carrying both Flexner and Shiga bacilli; in one instance he recovered from a fly captured in a hospital kitchen Shiga bacilli with typical cultural and agglutination characters.

Clinical Features.—As with malaria, so in dysentery a few cases were seen in which the initial symptoms appeared with such dramatic suddenness, and the intestinal disturbance was of so severe a character, that the possibility of the case being one of cholera could not be dismissed without serious consideration. The possibility of an outbreak of this disease had always to be reckoned with, though fortunately the organization which had been prepared to meet such a contingency never had to be put to the test. In the cases referred to all the initial symptoms of cholera were present, copious rice-water stools, profuse vomiting of watery fluid, cramps, low blood-pressure, and rapid collapse; but an immediate examination of the stools showed an absence of vibrios, and the cases, which ultimately were proved to be dysentery, responded rapidly to intravenous injections of antidysenteric serum combined with hypertonic saline.

Another interesting group of cases was characterized by *œdema*. This sometimes developed during the acute phase of cases of a severe Shiga infection, in which there appeared to be a rapid flooding of the system with dysentery toxins. In other cases the symptom appeared at a later stage, and in the absence of any of the usual antecedents of œdema; the cardiac condition, though feeble, was not in itself sufficient to cause œdema; the urine was not reduced in quantity, and though containing albumin in varying amounts it had none of the characters which are distinctive of nephritis; the possibility of the condition being due to a food deficiency, and connected with the drastic restrictions in diet which were found essential in the routine treatment of acute dysentery, deserves consideration. A noteworthy feature is that ascites was often a prominent symptom, and out of proportion to the œdema elsewhere; thus in one case 10 pints of fluid were removed from the abdomen, and on the following day, when I saw the patient, there was still a large amount of fluid present.

Other cases of special interest were those in which, after the cessation of all intestinal symptoms, pyrexia continued and the patient rapidly lost ground and became emaciated. The tongue was usually dry, smooth and shiny. A liberal diet made little or no difference to these patients. They were able to take and digest a considerable

amount of semi-solid food without diarrhœa or any symptoms of gastric or intestinal disturbance, but their assimilative powers seemed at fault and they steadily lost weight.

Hæmorrhage and *perforation* were both very rare complications. The first event was more common in connexion with the ulceration of long-standing amœbic dysentery than in cases of bacillary origin. In cases of perforation the actual rupture of intestine was almost always preceded by an adhesive peritonitis, which sufficed to prevent a general infection of the peritoneum. The resulting abscess formed slowly and was strictly localized. The whole process was very insidious, and, being marked by no special symptoms either at the time of perforation or during the subsequent stage of pus formation, might easily be overlooked. In one instance of acute Shiga dysentery a generalized acute peritonitis occurred without perforation; except for rather marked meteorism the features of the case were in no way distinctive; but at the post-mortem examination general peritonitis was found, with 2 pints of thin purulent fluid in the abdomen. The intestines were examined with the greatest care, but no perforation was discovered.

Although *arthritis* cannot be called a common complication of dysentery, there were during the season always a few cases under treatment, arising more often in complication with Shiga than with Flexner infection. The condition usually appeared in the course of the third week, and involved one or more of the larger joints, especially the knee. In the acute stage there was considerable enlargement and tenderness of the joints, with effusion of fluid, and pyrexia of irregular type. The acute stage was followed by a protracted period during which the fluid was absorbed and some thickening of the periarticular structures took place, giving rise to a condition not unlike that of rheumatoid arthritis, with considerable stiffness and pain on movement (fig. 12). In some cases the course was so prolonged that a permanent disability seemed inevitable, but I remember no case, however protracted, which had not made some progress towards recovery before leaving hospital. In the acute stage rest is necessary, and when the joint was tense with fluid much relief was afforded by tapping and the removal of the fluid. Some cases were treated with anti-dysenteric serum, with apparently good results, as shown in fig. 13 (p. 82); it is undesirable to use this method of treatment in cases that have previously been given serum during the stage of acute dysentery, on account of the risk of anaphylaxis. After the acute symptoms have subsided it is a mistake to keep the joint

absolutely at rest; massage and carefully regulated passive movements should be employed, and radiant heat is useful. The joint fluid is generally turbid, and in one case of a series of ten examined bacteriologically by Captain Elworthy, R.A.M.C., a Shiga bacillus was isolated from the fluid and cultivated. In the remainder of the series the fluid was sterile. Dysenteric arthritis is readily distinguished from serum arthritis by the lateness of its onset (usually between the fourteenth and twenty-third days of the disease), and by the comparative severity and long duration of the lesions. An interesting point is the almost constant association with conjunctivitis, which was a useful herald of the advent of arthritis, since it appeared usually about forty-eight hours, but sometimes several days, in advance of the joint condition. This association is illustrated in figs. 13 and 14 (pp. 82, 83).[1]

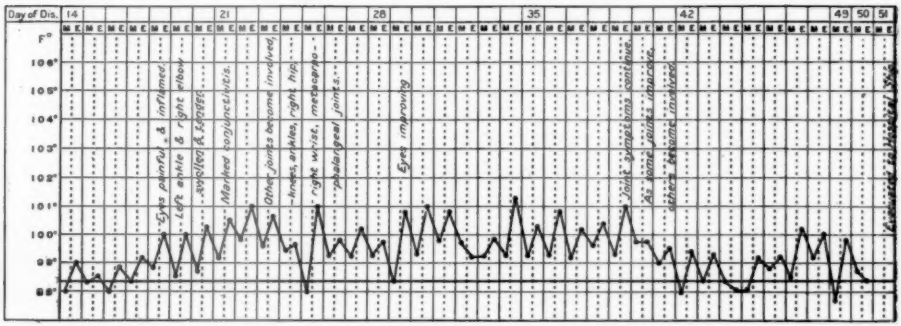


FIG. 12.

Dysentery with severe arthritis, showing characteristic fever and prolonged course. (No serum was given in this case.)

Nervous complications were observed in sequel to both amœbic and bacillary forms of dysentery. A man, who had just completed a second course of emetine treatment for an attack of amœbic dysentery, after three months from the onset of his symptoms developed paresis of the legs. On the next day (July 14) the knee-jerks, which had been present on the 13th, were unobtainable, and there was weakness of the grasp on both sides. On July 15 there was difficulty in swallowing, breathing

[1] The charts illustrated in figs. 12, 13, 14 and 17 were kindly supplied to me by Major H. R. Evans, who was in charge of a medical division at one of the dysentery hospitals.

was laboured, and the voice was hoarse. Two days later (July 17) there was complete loss of power in the legs, which were flaccid. Sensation was unimpaired. On July 18 there was anæsthesia on the inner aspect of both legs and knees, there was very little power in the recti muscles, respiration was chiefly intercostal, and the patient was unable to puff out his cheeks. The cerebro-spinal fluid was under slight pressure; its characters were normal. On July 21 there was incontinence of urine and fæces. In the course of the following days breathing became very difficult, the patient was irritable and restless, and on July 25 he died, twelve days after the onset of nervous symptoms.

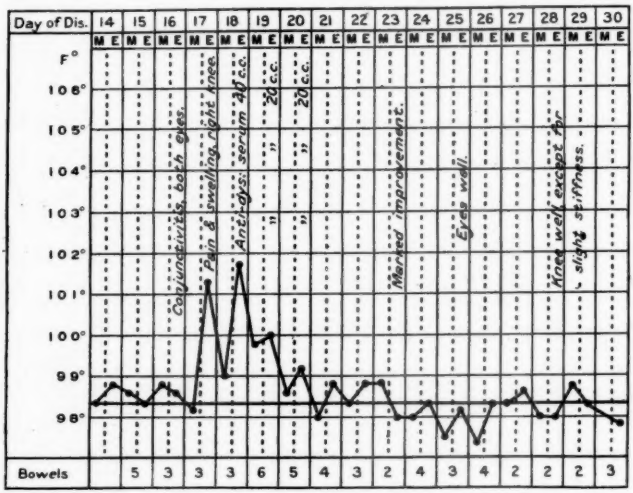


FIG. 13.

Dysentery with arthritis. Given anti-dysenteric serum; rapid subsidence of joint symptoms.

Post mortem the meninges of the brain were congested and the cerebro-spinal fluid was in excess, otherwise there were no macroscopical lesions of the nervous system. The intestines showed evidence of long standing dysenteric ulceration. Unfortunately no data as to the microscopical lesions were forthcoming. In another case of clinical dysentery, not bacteriologically proved, paresis of both legs and both arms with loss of deep reflexes supervened a fortnight after the cessation of intestinal symptoms. The cardiac dullness was increased both to the left and to

the right, the breathing became embarrassed, and the patient died of heart failure nineteen days after the onset of paresis. Post mortem all the chambers of the heart were dilated, the brain and meninges were congested and œdematous, and marked degenerative changes were found in sections of the popliteal and vagus nerves that had been removed for examination.

The subject of *amœbic hepatitis* is one that would be more fittingly dealt with by one of my surgical colleagues. Some scope, however, was provided for the exercise of the physician's art, both in diagnosis and treatment, as the diagnosis largely depended on the correct reading of the physical signs produced by an upward displacement of the

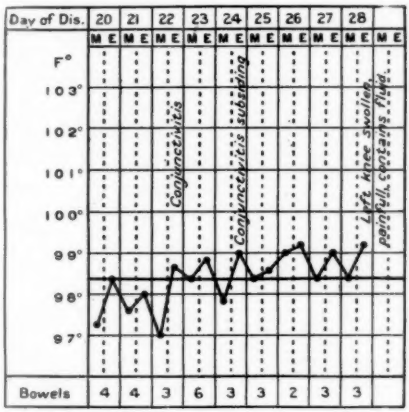


FIG. 14.

Dysentery with arthritis. Conjunctivitis six days previous to onset of joint symptoms.

diaphragm, usually on the right side, with shifting of the heart to the left, and collapse of the lower portion of the right lung; also by prompt emetine treatment the process was often arrested in the pre-suppurative stage. The chart in fig. 15 (p. 84) illustrates a case of this kind, which for some time was regarded as one of malaria. The prompt effect on the temperature curve of substituting emetine for quinine is very striking. Brief particulars may be given of two cases with unusual features. The first case was complicated by general peritonitis, which supervened without obvious cause four weeks before pus was located in the liver, and was dealt with successfully by operation; subsequently

pleurisy occurred with clear effusion, which became converted into an empyema owing to sloughing of the diaphragm, and escape of the contents of the liver abscess into the pleural cavity, as was shown later at the operation. This patient made a good recovery. The second case, a fatal one, was remarkable for the presence of two large abscesses in the right lobe, and numerous small abscesses about the size of a pea in both right and left lobes of the liver; one of the two larger abscesses was incised and drained, but was found at the autopsy to have leaked downwards below the colon and cæcum. In view of the multiple foci of suppuration it is clear that no further operative measures could have saved the life of this patient. For the records of these three cases I am indebted to the kindness of Major St.

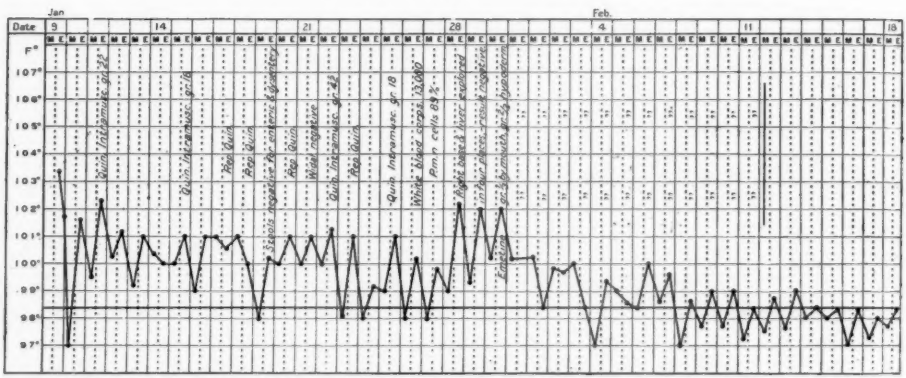


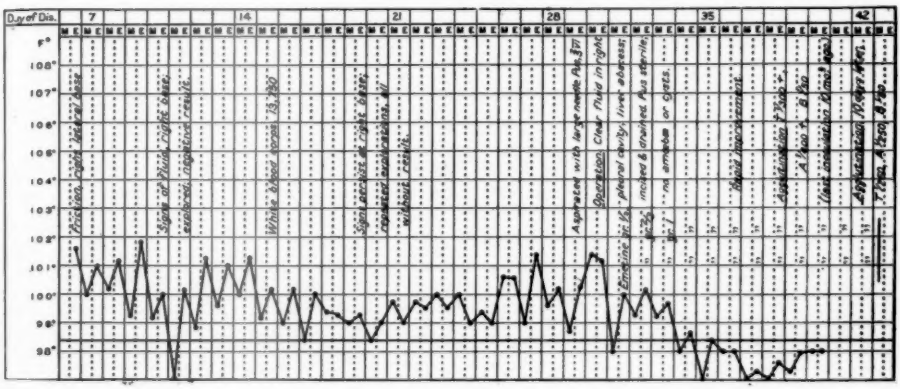
FIG. 15.

Case of acute hepatitis, probably amœbic, with pyrexia subsiding under treatment with emetine. History: no dysentery, frequent malaria. Taken ill first week in January—feverishness, shivers, flatulence. On admission: slight jaundice. Triangular patch of dullness at right base with distant breath sounds and increased vocal resonance. Abdomen distended; liver, almost to umbilicus, not tender. Spleen not palpable. Blood film negative.

John D. Buxton, R.A.M.C. The association of a clear pleural effusion with pus in the liver was observed in another case (fig. 16), the nature of which was obscure until in the course of the fifth week a large aspirating needle was introduced into the right pulmonary base and 6 oz. of very thick pus were withdrawn. In the course of operation on the following day clear fluid, through which the aspirating needle must have passed, was found in the pleural cavity; below this was a liver

abscess, which was incised and drained. In this case no direct evidence of an amœbic infection was discovered, either in the pus or the stools, and on the strength of the agglutination results the condition was regarded by Colonel Dudgeon as one of paratyphoid A, though there was nothing in the clinical course of the case that could not be accounted for by the hepatic abscess ultimately discovered.

In the dysenteric wards there were always to be seen the best instances of *combined infections*, with a complex clinical picture in which it was often difficult to assess the relative importance of the various infective agents. As an example, the case may be quoted of a patient, admitted to hospital as a proved Flexner case. As the result


Day of Dis.
7
14
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28
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42
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supplied with a polyvalent serum of the specific properties of which we knew very little beyond the fact that in different samples agglutinins against the various types of dysenteric organisms were represented in very varying degrees. The situation was to some extent cleared by Colonel Dudgeon in 1918, who by his observations on seventeen samples of sera showed that many of them were very deficient in agglutinins, especially for Shiga's bacillus, which were precisely those of which we had the greatest need, as the Shiga bacillus was undoubtedly the organism responsible for the bulk of the more serious cases of acute dysentery. If it had been practicable to supply us with pure anti-Shiga and anti-Flexner sera more striking results would doubtless have been obtained. Even as it was, working with sera of undefined therapeutic properties, a favourable impression on the whole was formed of the value of serum treatment, given two conditions—(1) that the serum was administered at the earliest possible moment, and (2) that sufficiently large doses were used. The tendency with serum in dysentery, as with quinine in malaria, was to give increasingly large doses as our experience grew, and latterly amounts of 80 to 100 c.c. were given subcutaneously in all cases with the clinical features of a severe bacillary infection immediately on admission, without waiting for a bacteriological report. The dose was usually repeated on the following day, and again on each of the succeeding two or three days, in the same or reduced amount, according to the course taken by the disease. In cases of special severity serum was given intravenously, in combination if necessary with normal saline or isotonic glucose solution. The value of intravenous saline was very great in dehydration cases with copious watery stools, a condition in which the maintenance of the body fluids is of the utmost importance. The prospective benefit of serum treatment in any given case rapidly lessens with the lapse of time from the onset of symptoms, and the opinion was formed that nothing is to be gained by the inauguration of serum treatment later than the sixth or seventh day of the disease. Serum rashes and transient joint symptoms were not uncommon; these were accompanied by severe febrile reaction in many cases (*see* fig. 17): true anaphylaxis was fortunately very rare. A valuable article on the diagnosis and treatment of dysentery in the British Salonika Force was read by Lieutenant-Colonel Graham, C.A.M.C., at a meeting of the Salonika Medical Society in May, 1917, and published later in the *Lancet*. In the light of subsequent experience it is interesting to note his opinion, expressed at the time, that less than 3 per cent. of the dysentery in the Salonika area was of the amœbic type.

In the treatment of amœbic cases emetine was of the greatest value in controlling acute symptoms and causing the disappearance, at least for a time, of cysts and amœbæ from the stools. But in a large proportion of cases the benefit was temporary, and periodical examination of the stools sooner or later disclosed the reappearance of organisms, though the accompanying intestinal disturbance might be of very slight severity, amounting only to looseness of the bowels, with or without the presence of mucus in the stools. The tendency to relapse is well shown by figures supplied to me by Captain Budden, R.A.M.C., who was in charge of the special section at the convalescent depot to which

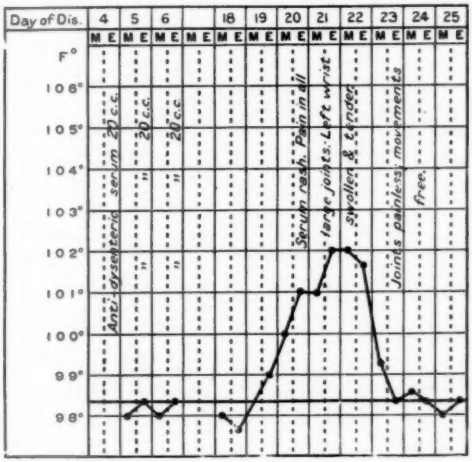


FIG. 17.

Dysentery. Severe type of serum reaction, with joint symptoms.

dysentery cases were discharged. His figures proved that of all dysentery relapses 65 per cent. were in amœbic cases, and that of all amœbic cases which came under his care 75 per cent. never became sufficiently fit to return to duty. It cannot be said that these patients had not received adequate emetine treatment; in every refractory case at least two and sometimes more than two full courses of emetine were given, though it was eventually realized that if a patient was not cured by a second course, it was waste of time to give further treatment in Salonika, and that the only prudent course was to evacuate him from the command as soon as possible. The value of emetine in acute dysentery was very

similar to that of quinine in malaria, that is to say, of the greatest use in controlling the acute manifestations of the infection, but disappointing when used for the purpose of eradicating the parasites from the system. A course of emetine lasted for twelve days, during which the patient either received $\frac{3}{4}$ gr. hypodermically twice daily, or 1 gr. hypodermically plus $\frac{1}{2}$ gr. by the mouth daily, or 3 gr. of bismuth-emetine-iodide by mouth, either alone or in combination with hypodermic treatment. From statistics in No. 15 of the Special Report Series of the Medical Research Committee it appears that emetine treatment meets with a greater measure of success here in England than was the case with us. This is no more than one would expect, and justifies the policy of the early evacuation of cases which did not rapidly clear up.

The criteria adopted for the discharge of bacillary cases from both hospitals and convalescent depots was a purely clinical one; after a week on full diet and in the absence of intestinal symptoms the patient was regarded as fit to leave hospital, and similarly if there was no recurrence of symptoms during his stay at the convalescent depot, and his general health was good, at the end of three weeks he was passed as fit for duty without bacteriological examination. Special importance was attached to the presence of mucus in the stools as evidence that the dysenteric process had not completely subsided.

Enteric Group.

Of the enteric group I have very little to say. The troops were well protected and the incidence was small. Fig. 18 shows the incidence of all forms grouped together for the three years 1916-18. It is seen that there is a marked drop from 11·77 per 1000 troops in 1916 to 0·84 per 1000 in 1918. In fig. 19 is shown an analysis into the various members of the group for each of the three years. I cannot explain the large number diagnosed as "enterica, clinical," during 1916; it is out of proportion, not only to the pathologically proved cases in the same year, but also to the number of "clinical cases" in the following years. Of cases pathologically proved it is seen that most were paratyphoid, para-A being in excess of para-B, specially in the two later years. The disease with few exceptions was of a mild type, and only sixty-five deaths occurred during the whole period. The case-mortality in 1916 was 3·62 per cent., in 1917 3·78 per cent., and in

1918 4·44 per cent. The majority of severe cases were due to *Bacillus typhosus*, a large proportion of these being admitted to hospital from vessels arriving at the port.

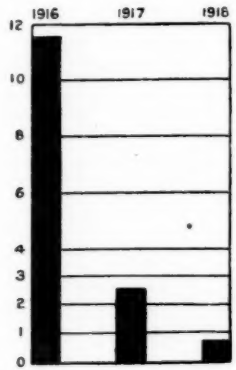


FIG. 18.

Enteric group, showing incidence of all forms as a ratio per 1000 of strength.

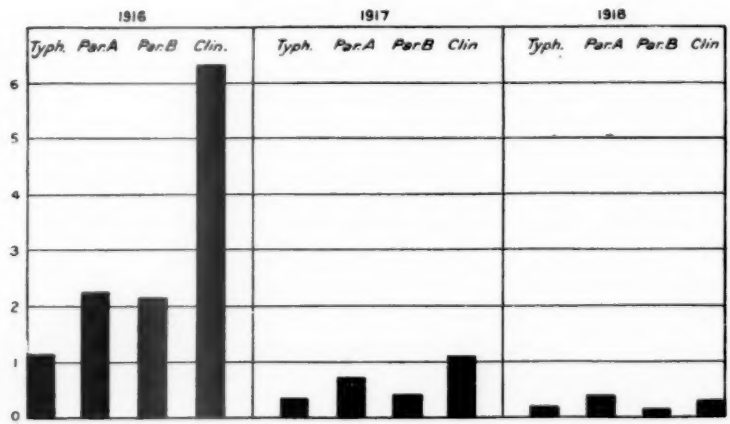


FIG. 19.

Enteric group, showing relative incidence of different varieties as a ratio per 1000 of strength.

Influenza.

The outbreak of influenza with its attendant complications took a heavy toll of the forces, and was the cause of special anxiety owing to the fact that the height of the epidemic corresponded to the military operations in September, 1918, which led to the defeat of the Bulgars and the signing of the armistice with Bulgaria. The total admissions for influenza and pneumonia during 1918 were 17813, being at the rate of 111·33 per 1000 troops, and the total number of deaths was 1387, or 8·67 per 1000; that is to say, almost one man out of every hundred troops fell a victim to the disease. In fig. 20 is shown a comparison between admissions and deaths from malaria and influenza (including complications) during the year expressed as a ratio per 1000. It

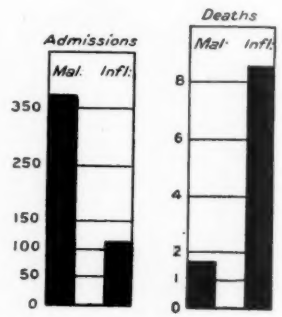


Fig. 20.

Admissions and deaths due to malaria and influenza (with complications) during 1918, shown as a ratio per 1000 troops.

illustrates graphically the enormous mortality from influenza. During the five and a half weeks from September 22 to October 30 deaths were occurring at the rate of 112 per week.

The epidemic first made its appearance in June in an outlying prisoners-of-war camp near Ekaterini, where there was an extensive outbreak of more than 800 cases, nearly all of mild type. By the end of June cases of what had come to be described as the "prevailing epidemic," many of them with serious pulmonary symptoms, were common in the Janes district, and as at the same time and in the same area there was an outbreak of sandfly fever, and every day showed a rising rate of admissions for malaria, the differentiation of these

diseases was often difficult and a certain degree of confusion in diagnosis was inevitable. At the end of August there was a serious outbreak of disease among Indian troops and Turkish prisoners of war at two adjacent camps at Arakli Farm near Llangaza Lake; some of these were cases of malaria, but in the majority the illness differed from malaria in the sudden onset without rigor or shivering, the absence of enlarged spleen or splenic tenderness, and the presence of a respiratory tract catarrh. Of the malaria cases in this outbreak there was a great preponderance of malignant tertian infection, as proved by examination of blood films. Another serious outbreak occurred a week or two later in a battalion 590 strong on the Doiran front; of these 130 fell ill, chiefly from two companies, in the course of three days; the infection spread rapidly, and in a couple of weeks the whole battalion was, in the words of the medical officer, "almost burnt out." In the earlier stage of the influenza epidemic, attempts were made by segregation of the sick to prevent its spread; but these proved unavailing, and the disease rapidly became practically general throughout the command.

The clinical features of the epidemic in Macedonia do not appear to be essentially different from those of the epidemics at home and on the western front, and I have little to add to the full and vivid account given by Major Burnford, R.A.M.C., in a communication to the Section of Medicine of this Society on April 29, 1919.[1] The disease usually began as an intense tracheitis spreading to the bronchial tubes and in the more severe cases rapidly involving the finest elements of the bronchial tree, with a patchy consolidation of lobular type. A true lobar pneumonia was comparatively uncommon, but frequently so many lobules were involved that the condition became confluent and the physical signs were indistinguishable from those of a lobar consolidation. Pleurisy and empyema were common complications. Other grave complications were necrosis of the lung leading to multiple abscesses, pericarditis, and meningitis. In one case next to no normal lung tissue was left; the lower lobes, middle lobe, and central part of the upper lobes were in a state of necrotic disintegration, crumbling under the finger, and readily separated from the pleura and the connective framework of the lung. The remainder of the upper lobes was emphysematous, some of the bullæ being of the size of a Tangerine orange.

The course of the disease was sometimes very rapid. In the case of a V.A.D. a state of generalized bronchiolitis was developed within a few

[1] *Proceedings*, 1919, xii (Sect. Med.), p. 49.

hours, causing intense dyspnœa and cyanosis with fine bubbling râles audible over the whole surface of the chest. Death took place within twenty-four hours of the onset. A common feature was the abundance of thick purulent sputum, sometimes streaked with bright red blood. This was difficult to expectorate, and its accumulation in the bronchial tubes led to marked dullness on percussion and complete abolition of breath sounds over the affected part. Voice vibration was diminished or absent, and in some cases these "silent" lungs could hardly be distinguished from pleural effusion except by the exploring needle. In some cases there was a rapid poisoning of the cardiac mechanism

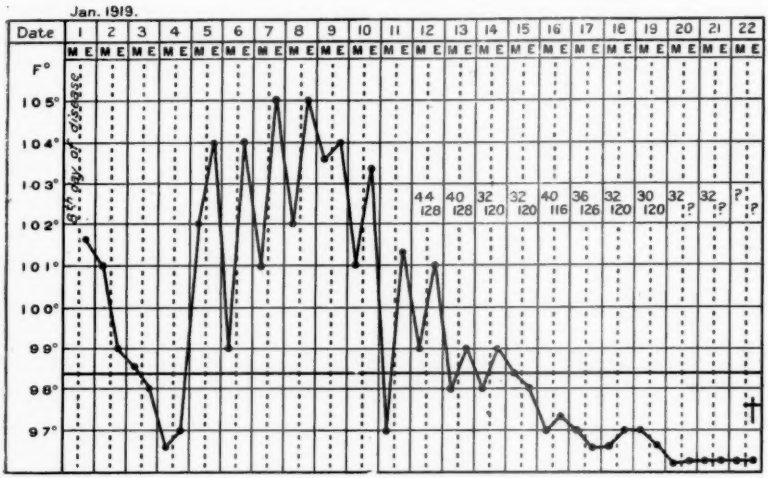


FIG. 21.

Influenza with bronchopneumonia. Apyrexic period of ten days during which patient steadily loses ground. Total duration thirty days. Confluent bronchopneumonia with small multiple abscesses of lung found post mortem.

by toxins, and death was due to heart failure; in others there was prolonged resistance, the pulse remaining good till near the end, death apparently being due to the mechanical effects of a prolonged blocking of the bronchial tubes. Little could be done for the relief of this condition, which was most distressing, the patient literally fighting for breath and life over a period of many days. An interesting group of cases was that in which the temperature came down by lysis, and there ensued a long apyrexic period during which the patient steadily

lost ground and ultimately succumbed. The chart of a case of this kind is shown in fig. 21. In other cases sudden death occurred from cardiac failure after the fall of temperature, and when the patient was apparently progressing towards a satisfactory convalescence (see fig. 22). Cases were unfortunately common in which influenzal pneumonia was complicated by an acute attack of malaria, a combination which usually proved fatal.

I cannot leave this part of my subject without paying a tribute to the devotion and utter disregard of personal risk displayed by the

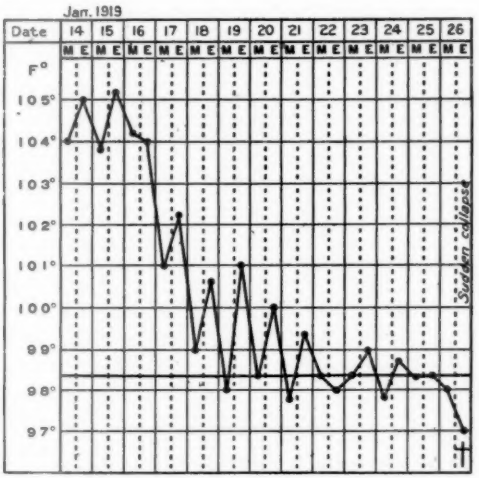


FIG. 22.

Influenza with bronchopneumonia. Sudden death during early convalescence, due to myocardial degeneration. Post mortem, heart dilated and flabby, subendocardial fat in excess, myocardium pale and friable.

nursing staff on duty in the wards reserved for serious cases during this epidemic. The work in these wards was extremely heavy and the risk of catching a disease which might prove fatal was a very real one; but they did not spare themselves, and the nursing of the sick throughout the emergency was worthy of the best traditions of the profession.

Sandfly Fever, &c.

Outbreaks of *sandfly fever* occurred at various places during the hot seasons, specially at Janes and at Chanak, and on more than one occasion the routine work of a medical unit was carried out only with great difficulty owing to the rapidity with which one member after another of the community fell a victim to the disease. Distinguishing points from malaria, apart from the blood examination, were the conjunctival injection with pain behind the eyes, the severe character of the pains in back and limbs, and the fact that with an equal degree of pyrexia the patient was not so much under the weight of his illness as during the actual attack of malaria. Convalescence, on the other hand, was more protracted. Initial rigors were rare, and the temperature curve, with its maximum on the first day, and its gradual fall during the two following days, was unlike that of any type of malaria. The subjective feelings of the patient were different in the two diseases; a patient with sandfly fever who had had previous experience of malaria was always emphatic that his present illness was something different from malaria. The presence or absence of sandfly bites gave no help, as it was common in sandfly districts for patients with malaria to bear the marks of previous bites; on the other hand, sandfly fever might occur without any marks on the skin, or any knowledge on the part of the patient that he had been bitten.

Sporadic cases of *acute meningitis* were seen from time to time, the majority being due to the meningococcus, while others were in connexion with the influenza epidemic. In one case of meningitis involving both brain and spinal cord, films and cultures from the meninges showed a Gram-positive coccus; cultures from the heart's blood obtained post mortem also showed a Gram-positive coccus which was morphologically identical with *Staphylococcus aureus.*

In 1917 a *Neurological Section* was started at one of the general hospitals in which invaluable work was done by Captain T. Graham Brown and Captain R. M. Stewart, R.A.M.C., who were in charge of the department. As I understand it is their intention to publish a full account of the work of the department I will make no further reference to it here.

Shortly after the armistice I made two expeditions to Bulgaria, as far as Sofia, and from there to Pirot, just beyond the Serbian frontier. In the Bulgarian hospitals at Sofia there were a few of our men who had been taken prisoners of war, and were being treated for wounds

or sickness. None of them were seriously ill, and in conversation they all expressed themselves as satisfied with the way in which they were being looked after; they were evidently receiving the same attention as the Bulgarian patients, and so far as could be seen the Bulgarian doctors were well trained and skilful, though much handicapped through shortness of medical supplies and appliances. There was also great scarcity of provisions, and our men found it necessary to supplement their scanty rations by buying bread, as far as their resources permitted, at the exorbitant prices which were ruling in the town. One of the hospitals was equipped with a complete set of Zander apparatus, a gift from the German Red Cross at the beginning of the war; when the Bulgarian collapse was imminent and the Germans left Sofia they wanted to take this costly apparatus away with them, and had to be reminded by the Bulgars that it was a gift, not a loan. Very different was the treatment of Serbian prisoners of war patients by the Bulgars. At Radomir, an hour's run from Sofia to the west, there was a concentration camp for prisoners of war, and a so-called hospital was attached to this. The dysentery ward was crowded with patients lying on filthy mattresses on the floor, two or three to each mattress; these patients were mostly suffering from acute dysentery, and were supplied with no conveniences and no attention beyond what they could give one another. A Bulgar doctor was supposed to see them daily, but I was told that before the arrival of the British he seldom visited this ward, and the only regular attention they received was a morning visit from orderlies to look round and remove the bodies of those who had died during the previous twenty-four hours. As I was leaving the ward one of the inmates in a far corner called out to me in English; he was one of our men who by some mischance had got mixed up with the Serbian patients; it was not many hours before he was being cared for in a section of a field ambulance that had recently arrived at Radomir, to his unspeakable relief. He was suffering from a severe attack of dysentery, but when I last heard from him he was doing well. Thanks largely to the energy of Colonel J. Ward, D.S.O., backed by the Bulgarian D.M.S., who gave what help he could, hospitals for sick and wounded Serbs were rapidly organized and staffed by our R.A.M.C. at Sofia, Radomir, and later at Pirot, where Serbs once more after long disuse were able to enjoy the advantages of good treatment and careful nursing.

A few cases of *typhus* and *relapsing fever* occurred in the Macedonian area; both these diseases became relatively more common after the

movement of troops to Constantinople and the Caucasus, though there was never anything approaching an outbreak on a large scale. The Weil-Felix reaction was of value, specially in mild cases, though its usefulness was restricted, owing to the fact that a positive result could not be expected earlier than the fifth day, by which time the diagnosis had in most cases become clear on other grounds. From investigations made by Captain Warnock, R.A.M.C., it appeared that the rise in the agglutinating power of the serum was very rapid, and in the course of twenty-four hours there might be an increase from 1 in 25 or 1 in 50 to 1 in 100, which was taken as the standard for a positive result. This standard was reached on the fifth day, sometimes earlier, and was followed by a rapid rise in agglutinating power until at the end of the pyrexial period (tenth to fourteenth day) a reaction was obtained with a very high dilution, e.g., 1 in 3000, 1 in 8000, and in one case 1 in 32000. The subsidence of fever was followed by a rapid fall of the reaction. Control observations were made in cases of relapsing fever, cerebro-spinal fever, small-pox, typhoid fever, malaria, influenza, pneumonia, syphilis, and in normal individuals, with negative results.

As an example of the utility of the reaction a case may be quoted in which marked meningeal symptoms were present at the onset; as a result of lumbar puncture a few drops of semi-gelatinous mucoid material were obtained containing scanty lymphocytes and no micro-organisms; the Weil-Felix reaction was positive on the seventh day with a dilution of 1 in 100; on the tenth day it was marked at 1 in 1600 and slight at 1 in 3200.

In the state of profound toxæmia which characterized the severer forms of typhus, benefit was derived from repeated intravenous salines; in the case of a patient at Tiflis who was desperately ill, in a semi-delirious state with low temperature and irregular pulse almost imperceptible at the wrist, and dry thickly coated tongue which could hardly be protruded, this procedure seemed to be the turning point of the illness, and when seen a week later he was convalescent. Among the complications and sequelæ of typhus observed in our own units and in Russian hospitals, I may mention suppurative parotitis, otitis media with mastoid disease, bronchitis and bronchopneumonia, femoral thrombosis and hemiplegia. In one case a complete unilateral paralysis of head, arm and leg developed gradually in the course of three days during convalescence.

While travelling in the Caucasus and Southern Russia with Colonel

Wenyon in the summer of 1919 we had opportunities of visiting hospitals, both civilian and military, other than our British ones. At Batoum we visted the general hospital for the province and the infectious hospital for the town, both civilian hospitals but under different management. The first was fairly well run on very limited resources; they were short of drugs and all kinds of medical appliances; but the hospital was clean, and they were making the best of a difficult situation. The cases were chiefly relapsing fever and convalescent typhus. The town hospital was a contrast in every way; the wards were crowded and filthy, the bed-clothes were grimy; flies were everywhere, and the atmosphere on a hot afternoon with closed windows can be better imagined than described; outside one of the wards was a cesspool overflowing into the hospital grounds. At Tiflis Colonel Wenyon and I were taken over the Georgian Municipal Hospital, a fine building in extensive grounds, with clean and well arranged wards, and adequate laboratories. The methods here were up-to-date and the standard of clinical work was high. The clinical material was excellent, and we spent a long morning in the wards, seeing many interesting cases. From Tiflis we went up to Erivan to investigate the circumstances of a case of suspected cholera among our troops, and were shown round one of the military hospitals by an Armenian doctor. The chief clinical interest was in the wards where cases of "typhus recurrens" or relapsing fever were under treatment. Of the numerous charts shown to us there were only one or two with more than a single relapse, notwithstanding the fact that no arsenical preparation was used in treatment. When asked why he did not give salvarsan, the doctor replied that it was unnecessary, as the cases were mild, and invariably recovered without special treatment; he said he had never had a death from the disease; but I think the real reason for withholding salvarsan was the fact that he had none to give. At many of the other hospitals I made the same inquiry and received a similar answer; it was clearly the local view that relapsing fever was a comparatively mild disease and that deaths were very rare.

From Batoum we went north to Novarossisk and Ekaterinodar, which at that time was General Denikin's headquarters. At both these places in early June the prevailing disease was relapsing fever; there were also many cases of typhus in the hospitals, but the majority were in the convalescent stage. At Ekaterinodar some of the hospitals were well arranged, others were the reverse; a reproach common to all was the failure to keep the patients free from lice. The part played by lice

in the spread of disease was well known, but when it came to practical measures there was a lamentable failure in the application of their knowledge. Real difficulties were present in the shortage of change of clothing and bedding, shortage of nurses, and sometimes shortage of water. The first of these was being remedied as far as possible by grants of linen from the British Military Mission, but little or no attempt was made to grapple with the louse problem, with the result that large numbers of the medical and nursing personnel fell victims to typhus. An incidence of 30 to 50 per cent. was regarded as a normal state of things; at one hospital in a vodka factory, with beds almost touching in unventilated and stuffy wards, *all* the personnel had had typhus, a record of which they were proud, instead of regarding it as a mark of the inadequacy of their hygienic service. It is a curious illustration of the mentality of the Russian authorities, that while neglecting such obvious precautions, they were ready to experiment with unproved and speculative methods of prevention and treatment. At Ekaterinodar some of the personnel in typhus wards were being inoculated with what was described as a "vaccine," consisting of sterilized blood drawn from typhus patients at the date of the appearance of the rash; good results were claimed for this procedure. Also, cases of typhus were being treated with serum removed from convalescent patients three to five days after the temperature had become normal; a daily dose of 10 c.c. was given subcutaneously for three to four days; the account of the benefits derived from this method of treatment was not convincing, and hardly justified the removal of large quantities of blood from those who were recovering from a grave and debilitating illness. At Novarossisk intravenous injections of cyanide of mercury in doses varying from 0·001 to 0·01 grm. were given in 2 c.c. of water every second day; 300 to 400 cases of typhus had been treated in this way with alleged good results.

The hospitals at Constantinople were on the whole clean and well ordered in most respects, except in their nursing arrangements, which were far below the standard which we regard as essential. Everywhere there was a shortage of drugs and of medical and surgical necessaries and all schemes for improvements and developments were held up for want of funds. The children's hospital on the outskirts of the town was a model of its kind, consisting of a number of separate pavilions standing well apart from one another in wooded park-like grounds overlooking the Bosphorus.

I cannot conclude my remarks without paying a special tribute to

the D.M.S., Major-General Sir M. P. Holt, K.C.B., K.C.M.G., who was always ready with his advice and whole-hearted support in carrying into effect any recommendations made with the object of improving the details of the medical service, and whose unfailing interest in the clinical aspects of the work was of immense encouragement to all concerned in maintaining a high standard of clinical efficiency. I also wish to express my thanks to Colonel C. B. Martin, C.M.G., Brevet Lieutenant-Colonel J. A. Anderson, and Major W. R. Galway, O.B.E., M.C., for supplying me with the figures and curves on which the foregoing statistical diagrams are constructed; to my predecessor Sir J. Purves Stewart, K.C.M.G., C.B., for many kindnesses; to Major H. W. Wiltshire, O.B.E., D.S.O., and Major H. R. Evans for valuable criticisms and suggestions, and to officers-in-charge of medical divisions and medical officers of the various hospitals, on whose willing co-operation in showing and discussing cases of interest so much depended in collecting the data on which this communication is based.

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The Royal Society of Medicine.

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OCCASIONAL LECTURE.

Plant and Animal Response.[1]

By Professor Sir Jagadis Chunder Bose, Kt., F.R.S., M.A., D.Sc., LL.D., C.S.I., C.I.E.

Founder and Director, Bose Research Institute, Calcutta.

At first sight few things would appear so strikingly different as the life-activities in plants and in animals. But if, in spite of the seeming differences, it could be proved that these activities are fundamentally similar, this would undoubtedly constitute a scientific generalization of very great importance. It would then follow that the complex physiological mechanism of the animal machine, that baffled us so long, need not remain inscrutable for all time; for the intricate problems of animal physiology would then find their solution in the study of corresponding problems under simpler conditions of vegetative life. That would mean a very great advance in the science of physiology, of agriculture, of medicine, and even of psychology.

Vegetable life has always appeared to us very remote, because that life is unvoiced. The plant, in its apparent immobility and placidity, stands in strong contrast to the energetic animal with its reflex movements and pulsating organs. Yet the same environment which, with its changing influences so profoundly affects the animal, is playing upon the plant. Storm and sunshine, the warmth of summer and the frost of winter, drought and rain—all these and many more come and go about it. What subtle impress do they leave behind? Internal changes there must be, brought about by these many agencies, rendering the plant more or less excitable; but our eyes have not the power to see them.

[1] At a meeting of the Society, held March 11, 1920.

How, then, are we to know what unseen changes take place within the plant? The only conceivable way would be, if that were possible, to detect and measure the actual response of the organism to a definite testing blow. When an animal receives an external shock, it may answer in various ways; if it has a voice, by a cry; if dumb, by the movement of its limbs. The external shock is the stimulus; the answer of the organism is the response. If we can make it give some tangible answer in response to a questioning shock, then we can judge of the condition of the plant by the extent of the answer. In an excitable condition the feeblest stimulus will evoke an extraordinary large response; in a depressed state even a strong stimulus evokes only a feeble response; and lastly, when death has overcome life, there is an abrupt end of the power to answer at all.

I have shown elsewhere that definite answers to stimulus may be obtained from the plant: (1) from the mechanical response; (2) from the electro-motive response; (3) from the response by conductivity-variation; and (4) the response by variation of growth.[1] All these different methods of investigation, with different instruments for record, have been found to give identical results.

In obtaining mechanical response of plants we encounter many serious difficulties. In the animal response by muscle-contraction the pull exerted is considerable and the friction offered by the recording surface presents no serious difficulty. In the case of plants, however, the pull exerted by a motile organ is relatively feeble, and in the movement of the very small leaflets of *Desmodium gyrans*, or the telegraph plant, a weight so small as four-hundredths of a gramme is enough to arrest the pulsation of the leaflets. The difficulty could not be removed so long as the writing lever remained in continuous contact with the recording surface, but this was overcome by making an intermittent instead of a continuous contact, accomplished by an invention depending on the phenomenon of resonance. The principle of the resonant recorder depends on sympathetic vibration. If the strings of two violins are exactly tuned, then a note sounded on one will cause the other to vibrate in sympathy. We may likewise tune the vibrating writing lever with a reed. Suppose the reed and the lever are both tuned to vibrate a hundred times in a second; when the reed is

[1] Bose: "Response in the Living and Non-living" (1902); "Plant Response" (1905); "Comparative Electro-physiology" (1906); "Irritability of Plants" (1913); "Life Movements in Plants," *Transactions of Bose Institute* (1918 and 1919). (Longmans, Green and Co.)

sounded the lever will also vibrate in sympathy, and will in consequence no longer remain in continuous contact with the recording plate, but but will deliver a succession of taps a hundred times in a second. The record will therefore consist of a series of dots, the distance between one dot and the next representing one-hundredth part of a second. With other recorders it is possible to measure still shorter intervals. It will now be understood how, by the device of my resonant recorder, not only is the error due to friction eliminated, but the record itself is made to measure time as short as may be desired. The extreme delicacy of this instrument will be understood when by its means it is possible to record a time interval as short as the thousandth part of a second.

Further, devices have been introduced by which the plant attached to the recording apparatus is automatically excited by successive electric stimuli which are absolutely constant. In answer to this the plant makes its own responsive records, goes through its own period of recovery, and embarks on the same cycle over again without assistance from the observer at any point. In this way the effect of changed condition is seen recorded in the script made by the plant itself.

Having thus made it possible that the plant itself would give an answer to our questionings, we are in a position to investigate as to whether the reactions of plant and animal life are essentially similar or different.

Six years ago I had the great pleasure of giving a demonstration in this very hall by which I showed the essential similarity of physiological reactions in the plant and in the animal; an illustrated account of that lecture, "On the Action of Drugs on Plants," will be found in the *Proceedings* of the Society for 1914-15.[1] I was able to show: that the plant like the animal goes through a period of insensibility in the course of twenty-four hours; that the response of plants is subject to fatigue; that the plant perceives changes in its environment which are often imperceptible to us.

I was also able to demonstrate that there is a conducting tissue in the plant along which the excitatory impulse, analogous to the nervous impulse in the animal, is transmitted with a definite velocity. This transmission in the plant is not hydro-mechanical as has been previously supposed, for all the characteristics of the nervous impulse in the animal were shown to be present in the corresponding

[1] *Proceedings*, 1914-15, viii (Occ. Lect.), pp. 1-40.

impulse in the plant; thus rise of temperature accelerated the velocity in both, lowering of temperature causing a retardation or arrest. Anæsthetics and poisons arrested the impulse in an identical manner. I shall presently have occasion to speak of certain characteristics of "nervous" impulse in the plant which led to the discovery of corresponding reaction in the animal nerve.

I also showed that the characteristics of the rhythmic tissues in the plant are surprisingly similar to those of the cardiac tissue in the animal. The effect of various drugs on the plant and animal tissues are essentially the same. Thus an application of dilute acid induces in the animal heart an atonic reaction, in consequence of which an arrest of pulsation takes place in a relaxed or diastolic condition. The action of dilute alkaline solution is the very reverse of this—namely, a tonic contraction and arrest in systole. These characteristic effects are repeated in an astonishing manner in the pulsations of the leaflets of *Desmodium gyrans*. The application of dilute solution of lactic acid induces an arrest of the pulsation of the plant in a state of diastolic relaxation; the application of dilute NaHO solution on the other hand, induces exactly the opposite effect—i.e., an arrest at systole.

The physiological effect of a drug on the plant was further shown to be modified by the dose of application. Thus a poisonous agent which caused depression or death was shown to act as a strong stimulant when administered in minute quantities.

I also showed that the effect of a given drug was modified by the "constitution" of the plant. As a concrete example of this may be cited the different reactions given by three batches of seedlings primarily similar. These were kept for some time under three distinct conditions and afterwards subjected to the action of a given dose of dilute poison. The first batch succumbed to the poison immediately; the second struggled for a time against it, recovered and exhibited a moderate rate of growth afterwards. But the third batch was actually stimulated by the poison and demonstrated this by invigorated growth.

Death-spasm in Plants.

A contractile spasm takes place in the plant at the point of death under the action of poison. It may be exhibited more simply by subjecting the plant to a continuous rise of temperature. This death-point—at any rate for all the dicotyledonous plants observed and their

different organs—was found to be almost as definite as a physical constant; for, using very diverse specimens and methods, this critical temperature is always at or very near 60° C. The death-contraction in the plant is in every respect similar to the same phenomenon in the animal, and is an instance of true excitatory effect. Yet different plants have their characteristic death-curves, and the same species may exhibit variations under changed conditions of age and previous history. Thus when the plant's power of resistance is artificially depressed, whether by poisons or by fatigue, its death-spasm occurs at a temperature often considerably lower—even as much as 23°. An electric spasm also occurs corresponding to the mechanical spasm at death. The electromotive force generated at death-temperature is sometimes considerable: in each half of a green pea this may be as high as half a volt. If five hundred pairs of half peas are suitably arranged in series, the electric pressure will be five hundred volts, which may even cause electrocution of unsuspecting victims. It is well that the cook does not know the danger she runs in preparing the particular dish; it is fortunate for her that the peas are not arranged in series!

Localization of Organ of Perception in Plants.

The most difficult problem in connexion with the tropism exhibited by plants is its perception of direction of gravity. When the plant is laid flat it perceives in some unknown way the direction of gravity and responds by bending upwards. For the clear understanding of the physiological reaction which induces the orientating movement, it is necessary to get hold, as it were, of a single cell or a group of cells *in situ* and in a condition of fullest vital activity; to detect and follow by some subtle means the change induced in the perceptive organ and the irradiation of excitation to neighbouring cells, through the entire cycle of reaction, from the onset of geotropic stimulus to its cessation. The idea of obtaining access to the unknown geo-perceptive cell in the interior of the organ for carrying out various physiological tests appeared at first to be very extravagant. But I was able to attack the problem with success by the invention of an electric probe which enabled me to explore the excitatory electric distribution in the interior of the organ. I have shown that the state of excitation in a plant is exhibited by an electric variation of galvanometric negativity. With the help of the electric probe, I was able to explore the interior of the plant and detect the state of excitation in its different layers. As long

as the stem remains vertical, geotropic stimulation is absent, but inclination to the vertical initiates the irritation. The electric probe consists of an exceedingly fine platinum wire, enclosed in a capillary glass tubing; the probe is thus electrically insulated except at the extreme tip. When the probe, suitably connected with a galvanometer, is slowly thrust into the stem so that it enters one side and comes out at the other, the galvanometer will by its deflection show the state of irritation of every layer of cell throughout the organ. Holding the stem vertical, I sent the exploring probe step by step across the organ but found little or no sign of local excitation.

The case is very different when the stem is displaced from the vertical to a horizontal position. The geotropically sensitive layer now perceives the stimulus and becomes the focus of irritation; the state of excitation is, as explained before, detected by negative electric change exhibited by the galvanometer, and the electric variation would be most intense at the perceptive layer itself; the excitation at the perceptive layer will irradiate into the neighbouring cells in radial directions with intensity diminishing with distance. Hence the intensity of responsive electric change will decline in both directions outwards and inwards.

The distribution of the excitatory change, initiated at this perceptive layer and irradiated in radial directions, may be represented by light and shade, the darkest shadow being on the perceptive layer itself. Had excitation been attended with change of light into shade, we would have witnessed the spectacle of a deep shadow, vanishing towards the edges, and spreading over the different layers of cells during displacement of organs from vertical to horizontal; the shadow would have disappeared on the restoration of the organ to the vertical position.

Different shades of excitation in different layers are, however, capable of discrimination by means of the insulated electric probe, as it is pushed into the organ from outside. In actual experiment the probe exhibited increasing excitatory electric change during approach to the perceptive layer, which reached its climax when the probe came in contact with that layer. When it passed beyond this point the electric indication of excitation underwent rapid decline and abolition. The electric indication at the perceptive layer itself became abolished as soon as geotropic stimulus was removed by the restoration of the organ to the vertical position. I was thus able to map out the contour lines of physiological excitation inside a living organ.

Control of Nervous Impulse.

I have briefly referred to the characteristics of "nervous" impulse in plants which are similar to those in the animal. I have been able to induce artificial paralysis in the plant nerve, and try the efficacy of various curative agents on it. These investigations with the simple type of plant-nerve are calculated to throw considerable light on the obscure phenomenon of nervous impulse in general, and the causes operative in bringing about the degeneration of the normal function of the nerve.

In carrying out investigations on the characteristics of the impulse transmitted along the conducting tissue of the plant, I found that it was of a dual character, a positive followed by a negative. The positive gives rise to expansion, and erectile movement of the mobile leaf; the negative, on the other hand, gives rise to contraction and down-movement of the leaf. The positive is more easily obtained under a very feeble stimulus. I have recently obtained effects somewhat similar with the animal nerve; the investigation is still in progress, and I am trying to devise a special apparatus by means of which these effects could be recorded.

The nervous impulse causes response which may be either mechanical or sensory, according to the nature of the terminal organ, muscle or brain. Our sensation is coloured by the intensity of nervous excitation which reaches the central perceiving organ. And this is determined by two different conditions—namely, the intensity of external stimulus, and the power of conduction possessed by the message-bearing vehicle, the nerve. We may consider two extreme cases; there are happenings which elude us because the impinging stimulus is too feeble for the resulting nervous impulse to cause perception. In this case we desire to exalt the conducting power of the nerve, so that what was sub-minimal shall become perceptible. On the other hand, the external stimulus, on account of its character or intensity may cause sensation which is intolerably painful. Could the painful tone be modified in transit or blocked altogether?

There is some resemblance between the conduction of electric impulse in a metallic conductor and nervous impulse in a nerve conductor. In metal, the power of conduction is constant, and the electric impulse will depend on the intensity of electric force that is applied. If the conducting power of the nerve were constant then the intensity of the nervous impulse and the resulting sensation will depend

inevitably on the shock that starts the impulse. In that case the modification of sensation would be an impossibility. But there may be a likelihood that the power of conduction possessed by a nerve is not constant but capable of change. Should this surmise prove to be correct, then we arrive at the momentous conclusion that sensation itself is modifiable whatever be the external stimulus. For the modification of nervous impulse there remains only one alternative—namely, some power to render the vehicle a very much better conductor, or a non-conductor, according to particular requirements. We require the nervous path to be supra-conducting, to have the impulse due to sub-minimal stimulus brought to sensory prominence. When the external blow, on the other hand, is too violent, we would block the pain causing impulse by rendering the nerve a non-conductor.

Under narcotics the nerve becomes paralysed, and we can thus by its use save ourselves from pain. But such heroic measures are to be resorted to in extreme cases, as when we are under the surgeon's knife. In actual life we are confronted with unpleasantness without notice. A telephone subscriber has the evident advantage, for he can switch off the connexion when the message begins to be unpleasant. But it is not everyone who has the courage of Mr. Herbert Spencer, who openly resorted to his ear-plugs when his visitor became tedious.

Let us now form a mental picture of the character of that disturbance by which nervous message is transmitted. A blow or shock from outside causes a molecular upset in the nerve-ending. The transmission of nervous impulse is regarded as a propagation of molecular disturbance from point to point along the conducting nerve.

On Molecular Predisposition.

Let us go one step further in our reasoning, from molecular propagation of disturbance, to the effect of molecular predisposition on the speed and intensity of transmitted disturbance. It is obvious that conduction would become facilitated or retarded by imparting to the molecules a predisposition in a favourable or in an unfavourable direction. The molecular upset and propagation of disturbance may be represented simply by means of a row of books. A certain intensity of blow applied, say, at the right, would cause the book to fall to the left, hitting its neighbour, causing other books to topple over in succession. If the books have previously been slightly tilted to the left, a particular predisposition would have been given them which would enable a feeble blow to

cause the upset; moreover the books would topple over more quickly, and the speed would be accelerated. But if, on the other hand, the books had been tilted to oppose the blow, then the stimulus that was effective would now become ineffective.

The theoretical considerations which I have adduced will show how in consequence of definite and ordered disposition in one direction or the other of the constituent units making up the conducting system, the excitation induced by the same stimulus may be accelerated or inhibited during transit. The essential thing is to get hold as it were of the molecules, and coerce them at one's bidding to arrange themselves in the positive and favourable direction, or in the negative and unfavourable direction. By the application of an external force of a polar character—an electric current flowing in one direction or its opposite—I have been successful in inducing at will and in turn two different molecular dispositions. I shall now describe the actual experiments, carried out first with the nerve of the plant. I produced a favourable disposition of the molecules in the nervous tissue, and feeble stimulus below the threshold now produced an extraordinary large response; conversely a violent excitation was at once quelled during transit by inducing the opposite molecular disposition, and thus abolishing the conducting power of the vehicle. I was thus able to confer on the identical nerve a *supra*-conducting or *non*-conducting power at will. No test could be more severe in the complete establishment of the theory I held about the fundamental unity of life-reactions in the plant and animal than the production of identical results in the nerve of the animal. Thus experimenting on the frog I found that under favourable molecular disposition of the nerve, the animal responded vigorously to stimulus which had hitherto been below its threshold of perception. Under the opposite disposition the violent salt-tetanus became instantly quelled. On the cessation of the directive force, the nerve returned to its normal condition.

These "molecular predispositions," it is evident, can also be produced by suggestion, and the efficacy of this mode of treatment is known to you in the case of nervous patients. "Faith-cure" is only a variety of it. Similarly auto-suggestion, or effort of the will, has profound influence on molecular disposition of the nerve. Who can define the limit of this power of will, intensified by practice and concentration?

In the determination of sensation then, the internal stimulus of will may play as important a part as the shock from outside. And thus through the inner control of the molecular disposition of the

nerve, the character of the resulting sensation may become profoundly modified. The external then is not so overwhelmingly dominant, and man is no longer passive in the hands of destiny. There is a latent power which would raise him above the terrors of his inimical surroundings. It remains with him that the channels through which the outside world reaches him should, at his command, be widened or become closed. It would thus be possible for him to catch those indistinct messages that had hitherto passed by him unperceived; or he may withdraw within himself so that in his inner realm the jarring notes and the din of the world should no longer affect him.

The High Magnification Crescograph.

We have next to investigate a very important class of phenomena, namely of growth and its variations. The difficulty of the investigation arises from the extreme slowness of growth. We take a single step, covering 2 ft. in about half a second; during this period the plant grows through a length of $\frac{1}{100000}$ part of an inch, or half the length of a single wave of light. It is evident that some very strongly magnifying arrangement must be employed to observe growth and its changes. The instrument hitherto used in the botanical laboratory—the "auxanometer"—magnifies about twenty times or so. Even here several hours must elapse before growth becomes perceptible; but during this long period the external conditions such as light and warmth can hardly but change, thus confusing, if not even vitiating, the results.

The external conditions can be kept constant only for a few minutes; and it is therefore necessary to obtain growth-magnification to something like 10,000 times. The difficulty of obtaining such magnification is so great that it took me several years to overcome them. This apparatus not only produces this enormous magnification, but also automatically records the rate of growth and its changes, in a period as short as a minute. In this instrument I employ a compound system of two levers; the first magnifies a hundred times, and the second enlarges the first a hundredfold, the total magnification being thus 10,000 times. But the double system of levers introduces difficulty on account of their weight; this was surmounted by the employment of an alloy of aluminium, which combines great rigidity with exceptional lightness. The friction at the bearings increased by the deposit of invisible dust particles introduced a further difficulty; bearings even made of ruby did not obviate the trouble. I was finally able to devise a new form of support by which all difficulties were fully overcome.

AUTOMATIC RECORD OF THE RATE OF GROWTH.

Another great difficulty in obtaining an accurate record of the curve of growth arises from the friction of contact of the bent tip of the writing lever against the recording surface. This I was able to overcome by an oscillating device in which the contact, instead of being continuous, is made intermittent. The smoked glass plate, G, is made to oscillate to and fro at regular intervals of time, say one second. The bent tip of the recording lever comes periodically in contact with the glass plate

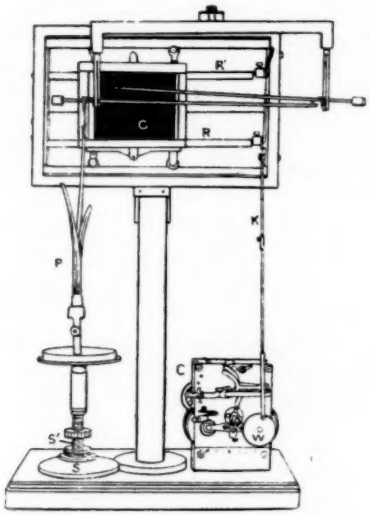


FIG. 1.

Complete apparatus. P, plant; S, S', micrometer screws for raising or lowering the plant; C, clockwork for periodic oscillation of plate; W, rotating wheel; K, crank.

during its extreme forward oscillation. The record would thus consist of a series of dots, the distance between successive dots representing magnified growth during a second.

The drawback in connexion with the obtaining of a record on the oscillating plate lies in the fact that if the plate approaches the recording point with anything like suddenness, then the stroke on the flexible lever causes an after-oscillation; the multiple dots thus produced spoil the record. In order to overcome this, a special contrivance

is necessary, by which the speed of approach of the plate should be gradually reduced to zero at contact with the recording point. The rate of recession should, on the other hand, continuously increase from zero to maximum. The recording point will in this manner be gently pressed against the glass plate, marking the dot, and then gradually be set free. It is only by strict observance of these conditions that the disturbing effect of after-vibration of the lever can be obviated.

This particular contrivance consists of an eccentric rod actuated by a rotating wheel. A cylindrical rod is supported eccentrically, so that semi-rotation of the eccentric causing a pull on the crank K pushes the plate-carrier gradually forward. On the return movement of the eccentric, a light antagonistic spring makes the plate recede. The rate of movement of the crank itself is further regulated by a revolving wheel which is released periodically by clockwork at intervals of one, two, five, ten, or fifteen seconds respectively, according to the requirements of the experiment. The complete apparatus is shown in fig. 1, p. 111.

I will now proceed to describe certain typical experiments which will show: (1) the extreme sensibility of the crescograph; (2) its wide applicability to different investigations; and (3) its capability of determining with great precision the time-relations of responsive changes in the rate of growth. In describing these typical cases, I give a detailed account of the experimental methods employed, and thus avoid repetition in describing subsequent experiments.

Determination of the Absolute Rate of Growth.

I will describe the results of a record on a stationary plate obtained with *Scirpus Kysoor*. The oscillation-frequency of the plate was once in a second, and the magnification employed was 10,000 times. The magnified growth-movement was so rapid that the record consists of a series of short dashes instead of dots (fig. 2, *a*). For securing regularity in the rate of growth, it is advisable that the plant should be kept in uniform darkness or in uniformly diffused light. So sensitive is the method of record that it shows a change of growth-rate due to the slight increase of illumination by the opening of an additional window. One-sided light, moreover gives rise to disturbing phototropic curvature. With the precautions described, the growth-rate in vigorous specimens is found to be very uniform.

After the completion of the first vertical series, the recording plate

was moved 1 cm. to the left; the tip of the recorder was brought once more to the top by means of the fine screw adjustment S (fig. 1), and the record taken once more after an interval of fifteen minutes. The magnified record for four seconds is 38 mm. in the first record. It is precisely the same in the record taken fifteen minutes later. The successive growth-elongations at intervals of one second are practically the same throughout, being 9·5 mm. This uniformity in the spacings demonstrates not only the regularity of growth under constant conditions, but also the reliability and perfection of the apparatus. It also shows that by keeping the external conditions constant, the normal growth-rate could be maintained uniform for at least fifteen minutes. As the magnified growth is nearly 1 cm. per minute, and as it is quite easy to measure 0·5 mm., the crescograph enables us to record a movement of

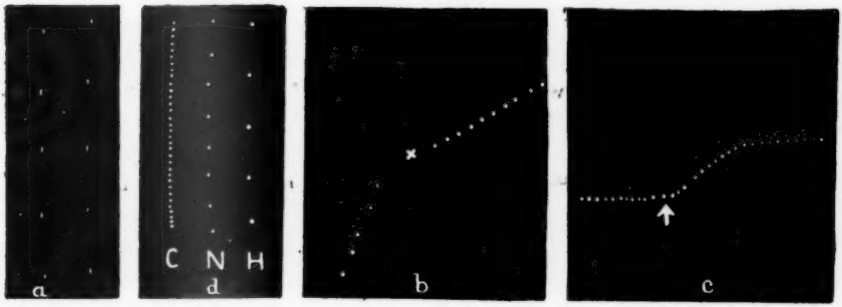


FIG. 2.

Crescographic records. *a*, successive records of growth at intervals of one second, × 10,000, with a stationary plate, *d*. Effect of temperature: N, normal rate of growth; C, retarded rate under cold; H, enhanced rate under warmth; *b*, record on moving plate, where diminished slope of curve denotes retarded rate under cold; *c*, horizontal record showing absence of growth in dead branch; physical expansion on application of warmth at arrow, followed by horizontal record on attainment of steady temperature. (× 2,000.)

0·00005 mm., that is to say, the sixteenth part of a wave of red light. The absolute rate of growth, moreover, can be determined in a period as short as 0·05 second. These facts will give us some idea of the enormous possibilities of the crescograph for future investigation.

As the period of the experiment is very greatly shortened by the method of high magnification, I shall, in the determination of the absolute rate of growth, adopt a second as the unit of time and μ or micron as the unit of length—the micron being 0·000001 metre, or 0·001 mm.

If m be the magnifying power of the compound lever, and l the average distance between successive dots in millimetres at intervals of t seconds, then

$$\text{rate of growth} = \frac{1}{mt} \times 10^3\mu \text{ per sec.}$$

In the record given $l = 9{\cdot}5$ mm., $m = 10{,}000$, $t = 1$ second.

Hence rate of growth

$$= \frac{9{\cdot}5}{10{,}000} \times 10^3\mu \text{ per sec.} = 0{\cdot}95\ \mu \text{ per sec.}$$

Precaution against Physical Disturbances.—There may be some misgiving about the employment of such high magnification; it may be thought that the accuracy of the record might be vitiated by physical disturbance, such as vibration. In physical experimentation far greater difficulties have been overcome, and the problem of securing freedom from vibration is not at all formidable. The whole apparatus need only be placed on a heavy bracket screwed on the wall to ensure against mechanical disturbance. To what extent this has been realized will be found from the inspection of the first part of the record in fig. 2, *c*, taken on a moving plate. A thin dead twig was substituted for the growing plant, and a perfectly horizontal record not only demonstrated the absence of growth-movement but also of all disturbance. There is also another element of physical change, against which precautions have to be taken in experiments on variation of the rate of growth with rising temperature. In order to determine its character and extent, a record was taken, with the dead twig, of the effect of raising the temperature of the plant-chamber through 10° C. The record, with a magnification of 2,000, shows that there was an expansion during the rise of the temperature, after which there was a cessation of physical movement, the record becoming once more horizontal. The obvious precaution to be taken in such a case is to wait for several minutes for the attainment of steady temperature. The movement caused by physical change abates in a short time, whereas the change of rate of growth brought about by physiological reaction is persistent.

Having demonstrated the extreme sensitiveness and reliability of the apparatus in quantitative determinations, I proceed to show its wide applicability for various researches relating to the influence of external agencies in modification of growth. For this two different methods are employed. In the first, the records are taken on a stationary plate; the first in the series gives the normal rate; the second is the record taken under the given changed condition. The increase or diminution

of the space between successive dots in the two records at once demonstrates the stimulating or depressing nature of the changed condition. In the second method, the record is taken on a plate moving at a uniform rate by clockwork. A curve is thus obtained, the ordinate representing growth-elongation and the abscissa the time. The increment of length divided by the increment of time gives the absolute rate of growth at any part of the curve. As long as growth is uniform, so long the slope of the curve remains constant. If a stimulating agency enhances the rate of growth, there is an immediate flexure in the curve. A depressing agent lessens the slope of the curve.

I will now give a few typical examples of the employment of the crescograph for the investigation of growth; the first example I shall take is the demonstration of the influence of temperature.

Stationary Method.—Fig. 2, *d*, gives records taken on a stationary plate. The plant used was *Kysoor;* the crescograph magnification was 2,000, and the successive dots at intervals of five seconds. The middle series, N, was at the temperature of the room. The next, C, was obtained with the temperature lowered by a few degrees. Finally, H was taken when the plant-chamber was warmed. It will be seen how under cooling the spaces between successive dots have become shortened, showing the diminished rate of growth. Warming, on the other hand, caused a lengthening of the spaces between successive dots, thus demonstrating an enhancement of the rate of growth.

Calculating from the data obtained from the figure, we find:—

Absolute value of normal rate	...	...	...	0·457 μ per second.
Diminished rate under cold	...	...	...	0·101 ,, ,,
Enhanced rate under warmth	...	...	...	0·737 ,, ,,

Moving Plate Method.—Another experiment was carried out with a different specimen of *Kysoor*, the record being taken on a moving plate (fig. 2, *b*). The left part of the curve here represents the normal rate of growth. The plant was then subjected to moderate cooling, and the subsequent curve with its diminished slope denotes the depression of growth.

Determination of Latent Period and Time-relations of Response to Stimulus.

In the determination of time-relations of responsive change in growth under external stimulus, I take the typical case of the effect of electrical shock of one second's duration from a secondary coil.

Two electrodes were applied, one above and the other below the growing region of a bud of *Crinum*. The record was taken on a moving plate, magnification employed being 2,000, and successive dots made at intervals of two seconds. It was a matter of surprise to me to find that the growth of the plant was affected by an intensity of stimulus far below the limit of our own perception. For convenience I shall designate the intensity of electric shock that is barely perceptible to us, as the unit shock. When an intensity of 0·25 unit was applied to the growing organ, it responded by a retardation of the rate of growth. As regards the relative sensibility of plant and animal, I may say that the leaf of *Mimosa pudica*, in a favourable condition, responds to an electric stimulus which is one-tenth the minimum intensity that causes perception in a human being.[1]

Inspection of fig. 3, *a*, shows that a flexure is induced in the curve in response to stimulus, the flattening of the curve denoting retardation of growth. The latent period in this case was six seconds. The normal rate was found restored after a rest of five minutes. The intensity of shock was next raised from 0·25 unit to 1 unit. The second record shows that the latent period was reduced to four seconds, and a relatively greater retardation of growth was induced by the action of the stronger stimulus. The recovery of the normal rate was effected after the longer period of ten minutes. I took one more record, the stimulus being 3 units. The latent period was now reduced to one second, and the induced retardation was so great as to effect a temporary arrest of growth, after which there was a slow recovery. Under still stronger stimulation, the retardation culminates into an actual contraction, the responsive contraction persisting for a considerable length of time. What has been said of electric stimulus is equally true of all modes of stimulation, mechanical, photic or chemical.

As a further example of the capability of the crescograph, I will give a record of a single pulse of growth obtained with the peduncle of *Crocus* (fig. 3, *b*). The magnification employed was 10,000, the successive dots being at intervals of one second. It will be seen that the growth-pulse commences with a sudden elongation, the maximum rate being 0·4 μ per second. The pulse exhausts itself in fifteen seconds, after which there is a partial recovery in the course of thirteen seconds. The period of the complete pulse is, therefore, twenty-eight seconds. The resultant growth in each pulse is, therefore, the difference between

[1] Bose, "Irritability of Plants," 1913, p. 50.

elongation and recovery. Had a highly magnifying arrangement not been used, the resulting rate would have appeared continuous. In other specimens, owing probably to greater frequency of pulsation and co-operation of numerous elements in growth, the rate appears to be practically uniform.

TABLE I.—TIME-RELATIONS OF GROWTH VARIATION UNDER ELECTRIC SHOCK.

(*Crinum.*)

Intensity of stimulus	Latent period	Normal rate	Retarded rate
0·25 unit ...	6 seconds ...	0·62 μ per second. ...	0·49 μ per second.
1 ,, ...	4 ,, ...	0·62 ,, ,, ...	0·25 ,, ,,
3 units ...	1 second ...	0·62 ,, ,, ...	Temporary arrest of growth

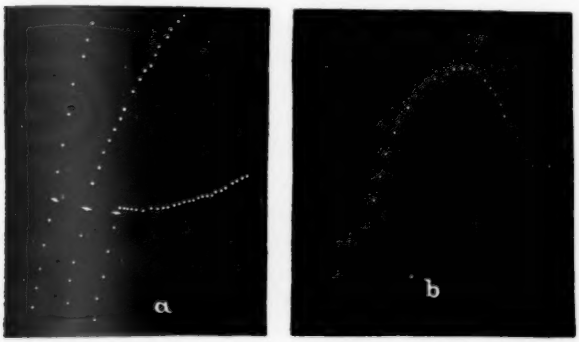


FIG. 3.

a, Time-relations of response of growing bud of *Crinum* to electric stimulus of increasing intensity applied at the short lines; *b*, record of a single growth-pulse of *Crocus*.

It will be seen from the above that growth in plants may be affected by an intensity of stimulus which is below human perception; that with increasing stimulus the latent period is diminished and the period of recovery increased; that the induced retardation of growth increases continuously with the stimulus till at a critical value there is a temporary arrest of growth; with greater intensity of stimulus there is induced an actual contraction.

By the employment of the method just described the effect of manures and chemicals, drugs and poisons, may now each be determined in the course of a few minutes, and with unprecedented accuracy. Here too, as in the preceding cases, we realize the value of this high magnification apparatus: not simply because all the phenomena are

rendered far clearer and more conspicuous, but also because the result of any particular change of conditions can be detected in the course of a few minutes, during which the other conditions may remain constant, or be artificially kept so.

It will be understood that it is only by the discovery of laws of growth that any marked advance in scientific agriculture is possible. We have been using only a few stimulating agents, whereas there are thousands of the actions of which we have no conception. The rule of thumb method hitherto employed in the application of a few chemical stimulants and of electricity has, moreover, not been uniformly successful. The cause of the anomaly is found from the discovery of an important factor—namely, the dose of application, which had hitherto not been taken into account. Thus I find that while a particular intensity of electrical current accelerated growth, any excess above a critical point retarded it. The same is true of chemical stimulants. A striking practical result was obtained with certain poisons which in normal doses killed the plant, but in quantities sufficiently minute acted as an extraordinarily efficient stimulant, the treated plants growing far more vigorously and flowering much earlier. The treated plants, moreover, successfully resisted the insect blights. These facts lead to the inquiry into the critical point at which a depressant passes into a stimulant, or conversely. At this point we see how a fresh line of research has here been opened for pharmacology and medicine. And similarly another for speedily testing the action of manurial agents, and other means of accelerating growth for agriculture. The immediate test needs only a few minutes instead of a season, while the changing conditions of the latter are avoided.

The Balanced Crescograph.

The researches described showed that the growth of plants was affected by changes in the environment which were below the range of human perception. For this new range of investigation I had to turn my attention to a new type of apparatus, the sensitiveness of which had to surpass those which I had already invented. The high magnification crescograph enabled me to measure the most minute rate of growth. For the detection of the effect of impact of external stimulus it was necessary first to measure the normal rate, and afterwards the changed rate induced by the stimulus. The effect of stimulus, whether stimulating or depressing, could be found from calculation of the difference in the two cases. I next wished to eliminate the necessity for calculation

and the consequent loss of time by devising a new method which would *instantly* show by the up or down movement of an indicator the accelerating or retarding effect of the agent on growth.

The desideratum was to compensate the up-movement of growth by some regulating device; this involved the problem of making the plant descend at the exact rate at which the growing plant was rising, whatever that rate might be. Some such regulator has to be introduced as in the compensating movement of an astronomical telescope, by which the effect of earth's movement round her axis once in twenty-four hours is neutralized. But the problem that confronted me was far more difficult, for instead of compensating a definite rate I had to obtain adjustment for widely varying rates of growth in different plants, and even of the same plant under different conditions.

The difficult problem was successfully solved in my balanced crescograph. A train of revolving clockwheels, actuated by the fall of a weight, lowers the plant exactly at the same rate at which it is growing. The exact adjustment is obtained by regulation of a governor; the gradual turning of a screw to the right or to the left causes the rate of compensating fall to be retarded or accelerated. In this way the rate of growth becomes exactly compensated, and the recorder now dots a horizontal line instead of the former curve of ascent. The turning of the adjusting screw of the balanced crescograph also moves an index against a circular scale so graduated that its reading at once gives the rate at which the plant is growing at that instant. When balanced, the recording apparatus is extraordinarily sensitive. Any change, however slight, in the environment is at once indicated by the upset of the balance with up or down movement of the curve. This method is so sensitive that it has enabled me to detect variation of rate of growth as minute as 1,500-millionth of an inch per second.

As an illustration of the delicacy of this method, a record (fig. 4) is given of the effect of carbonic acid gas on growth. A jar is filled with this gas and emptied over the plant; the invisible gas, on account of its heavier weight, falls in a stream and surrounds the plant. The record shows that this gave rise to an immediate acceleration of growth, and this continued for two and a half minutes; this preliminary acceleration was followed by retardation of growth, as shown by the down curve. With diluted carbonic acid, the acceleration may persist for an hour or more. Thus the balanced crescograph not only shows us the beneficial effect of an agent, but also tells us the dose which prolongs the beneficial effect.

Plants are regarded as extremely sluggish: and it is supposed that they are unable to perceive a stimulus unless applied for a considerable length of time. Thus, for the perception of geotropic stimulus, it is supposed that, "even in rapidly reacting organs there is always an interval of about one to one and a half hours before the horizontally placed organ shows a noticeable curvature, and this latent period may in other cases be extended to several hours (Jost)." I find that the latent period of geotropic perception is often as short as a second.

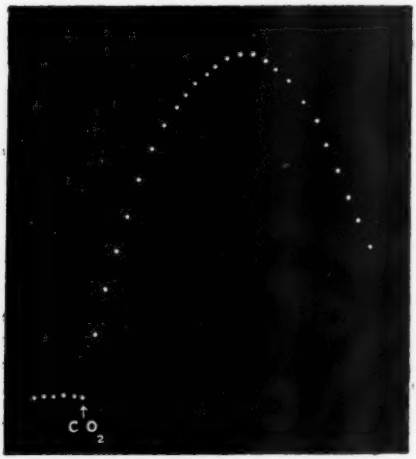


FIG. 4.

Record by Balanced Crescograph of the effect of CO_2 on growth. The horizontal line is the record under exact balance, which is upset by the action of CO_2.

As regards perception of light, it has been supposed that the period of effective exposure must at least be of seven minutes' duration. With my extremely sensitive apparatus I investigated the question of plants' capability to respond to stimulus of light of excessively short duration. We can hardly conceive of anything so fleeting as a single flash of lightning. The growing plant, balanced in the crescograph, was subjected to an artificial flash of lightning—that is to say, to the light emitted by a single electric spark between two metallic balls. The plant perceived this light of incredibly short duration, as was manifest from the upset of the balance, and the resulting automatic script made by the plant.

So much as regards the perception of plants to minimum duration of stimulus. The next question is as regards their range of perception. My discovery of the response of plants to wireless stimulation has recently roused much interest. At first sight the possibility of the plant to perceive and respond to the long ether waves employed for wireless signalling would appear to be very unlikely. For the rays hitherto regarded as most effective on the plant are in the ultra-violet region with wave-length as short as 20×10^{-6} cm.; but with electric waves used in wireless signalling we have to deal with waves 50,000,000 times as long. The perceptive power of our retina is confined within the

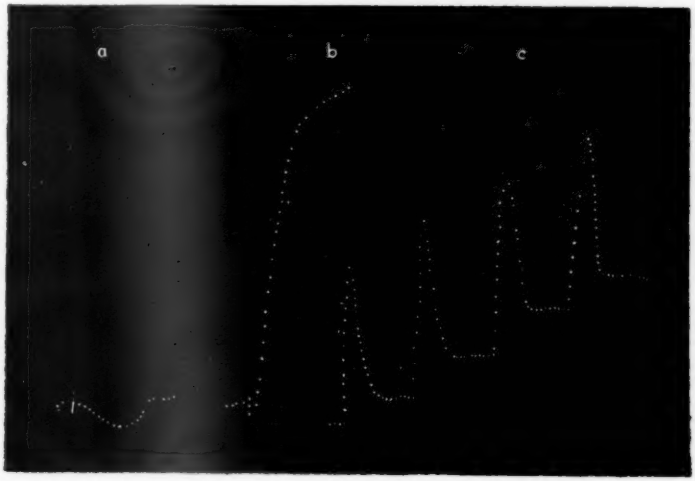


FIG. 5.

Record of responses to wireless stimulation by means of the Balanced Crescograph. (a) Response to feeble stimulus by acceleration of growth; (b) response to strong stimulus by retardation; (c) response to medium stimulation —retardation followed by recovery. Down-curve represents acceleration, and up-curve retardation of growth. (Seedling of wheat.)

very narrow range of a single octave, the wave-lengths of which lie between 70×10^{-6} cm. and 35×10^{-6} cm. It is difficult to imagine that plants could perceive radiations so widely separated from each other as the visible light and the invisible electric waves.

But the experimental investigations which I have carried out prove conclusively that plants perceive and respond to wireless stimulation. This will be seen from the records (fig. 5) here reproduced given by the

balanced crescograph. The perceptive range of the plant is thus inconceivably greater than ours; it not only perceives but also responds to the different rays of the vast æthereal spectrum.

The Magnetic Crescograph.

The magnification obtained with two levers, as stated before is 10,000 times. It may be thought that further magnification would be possible by a compound system of three levers. But the friction at the fulcrum becomes added up by an increase in the number of levers, and this interferes with the uniformity of the movement of the last recording lever. For securing further magnification additional material contact has, therefore, to be abandoned and some mode of linking, as it were, through space to be adopted. I have recently been successful in devising an ideal method of magnification without additional contact. The movement of the single lever of the crescograph upsets a very delicately balanced magnetic system. The indicator is a spot of light reflected from a mirror carried by the deflected magnet (fig. 6). In this way I have been successful in obtaining magnification which may be varied from one to fifty million times.[1]

Our mind cannot grasp magnification so stupendous. We can, however, obtain some concrete idea of it by finding what the speed of the proverbial snail becomes when magnified ten million times by the magnetic crescograph. For this enhanced speed there is no parallel even in modern gunnery. The 15-in. cannon of the *Queen Elizabeth* throws out a shell with a muzzle velocity of 2360 ft. per second or about 8,000,000 ft. per hour; but the crescographic snail would move at a speed of 200,000,000 ft. per hour, or twenty-four times faster than the cannon shot. Let us turn to cosmic movements for a closer parallel. A point on the equator whirls round at the rate of 1037 miles per hour. But the crescographic snail may well look down on the sluggish earth; for, by the time the earth makes one revolution, the snail would have gone round nearly forty times!

[1] "Sir Jagadis Bose's crescograph is so remarkably sensitive that doubt was recently expressed as to the reality of its indications as regards plant growth; and the suggestion was made that the effects shown by it were due to physical changes. A demonstration at University College, London, on April 23 has, however, led Lord Rayleigh and Professors Bayliss, V. H. Blackman, A. J. Clark, W. C. Clinton and F. G. Donnan to state in the *Times* of May 4: 'We are satisfied that the growth of plant tissues is correctly recorded by the instrument and at a magnification of from one to ten million times.' Sir W. H. Bragg and Professor F. W. Oliver, who have seen similar demonstrations elsewhere, give like testimony that the crescograph shows actual response of living plant tissues to stimulus." *Nature*, May 6, 1920, p. 305.—J. C. B.

The magnetic crescograph is before you. If I attach to the apparatus a specimen of plant which had been killed and in which there is therefore no growth, the indicating spot of light remains quiescent. But when I attach the growing plant, see how the spot of light moves with steady speed towards the right, the magnification employed being now two million times. While it is moving to the right I give it an electric shock, which we saw arrests growth : the movement of the spot of light also comes to a stop. Under stronger and long continued shock the spot of light flies to the left, showing the actual contraction of the organ. A very similar effect is produced by the stimulus of light, the ultra-violet light being most effective in retardation of growth.

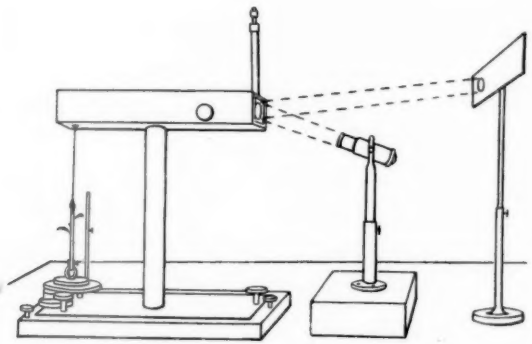


FIG. 6.

The Magnetic Crescograph. A magnetized lever causes by its movement rotation of suspended magnetic needle with attached mirror.

I take another specimen: you note the normal rate of its growth movement. I now apply a stimulating drug : observe how the movement is greatly enhanced ; a depressing drug on the other hand slows down growth. The life of the plant thus becomes subservient to the will of the experimenter, so that he can exalt or depress its activity, bring it near the point of death by application of poison, and when the plant is hovering in an unstable poise between life and death resuscitate it by the timely application of an antidote. Here is played before us the drama of life and death, revealing to us much that had been obscure.

These, our mute companions, silently growing beside our door, have now told us the tale of their life-tremulousness and their death-spasm in script that is as inarticulate as they. May it not be said that their story has a pathos of its own beyond any that we have conceived?

In realizing this unity of life is our final sense of mystery deepened or lessened? Is our sense of wonder diminished when we realize in the infinite expanse of life that is silent and voiceless the foreshadowing of more wonderful complexities? Is it not rather that science evokes in us a deeper sense of awe? Does not each of her new advances gain for us a step in that stairway of rock which all must climb who desire to look from the mountain-tops of the spirit upon the promised land of truth?

DISCUSSION.

The PRESIDENT: You have shown by your applause the intense interest aroused in these demonstrations, revelations which almost bewilder one as to their eventual outcome. These records have been made possible by the extraordinarily skilful adaptation of the graphic method, and as we have with us a master of that method, Dr. Waller, he will perhaps voice our interest and appreciation of Sir Jagadis Bose's demonstration.

Professor A. D. WALLER, F.R.S.: You, Sir, have already given expression to the feeling of gratitude we have to Sir Jagadis Bose for his lecture and for the great trouble he has taken to put before us a demonstration of his beautiful instrument. I have been doing a little sum to enable me to realize what a magnification of ten million times means. It would mean a blood corpuscle 70 metres in width. One μ multiplied by a million would be a metre, multiplied by ten millions would be 10 metres. Errors would be in the same proportion. It is an astounding magnification, and I came here to-day in order that I might satisfy myself whether or not the movement upon the screen truly represented growth. It is at a quite reasonable rate; the spot of light appeared to me to move at the rate of about a metre per second. But the behaviour of the spot of light was, to me, highly suspicious, if I may venture to say so. It moved at the rate it ought to, and at the rate I expected it to move, and the experiment at the end was precisely what I expected it to be. That is to say, not only was there an arrest of the growth, but there was the converse of growth, there was de-growth; to the right as I read it, was the direction of elongation, to the left the direction of contraction. What I would like is, that Sir Jagadis would repeat to us on non-living cells under the less difficult conditions of the laboratory the experiment he has just shown us on living matter under the difficult conditions of the lecture room—you see I am taking free advantage of your permission, Sir, to criticize. Let me add, however, that criticism is a

compliment we Westerns freely pay to each other, though it may seem to an Eastern fellow-subject of the Empire a little out of place if he does not see quite eye to eye with us. But it is, in reality, the greatest compliment I can pay Professor Bose to take his demonstration quite seriously. When the induction shocks were passed there was the reversal of growth, the spot moved in the sense of shortening of the plant; and if we had been in the laboratory I should have said "kill that plant, put it into hot water, then repeat the experiment and see how it acts." [Sir JAGADIS BOSE: I can kill it.] I quite understand that under present conditions it would be impossible to carry that out, but it could easily be done in the laboratory.[1]

The contractility of the nerves of plants as well as of animals was another discovery announced by Sir Jagadis Bose a few years ago, which excited my astonishment. But I think the explanation was entirely to be found in the heating effects of the currents used for excitation. A very good object to take for this kind of demonstration is Engelmann's "E" fiddle string, which is certainly non-living matter; upon that one can make all the experiments Sir Jagadis has shown us upon plant nerve contractility and so on. A weak stimulation causes elongation, a strong stimulation causes shortening. If Professor Bose would repeat his experiments in the laboratory we should arrive at a common understanding and a real augmentation of learning. I am taking advantage of the opportunity in the friendliest spirit possible; I am doing it, recognizing that Sir Jagadis is a British citizen of the Indian Section of our Empire, and he comes with a message from which we can well learn. He pointed, in one of his photographs, to the magnificent establishment which he has set up in Calcutta, and looks to it hopefully as a Renaissance of the ancient Indian learning which might be taken by us as an object lesson. We can repeat a statement contained in his dedication of this magnificent Temple of science to

[1] The misgiving to which I gave expression at the meeting, after witnessing the experimental demonstration by Sir J. C. Bose, F.R.S., so far from having been allayed by subsequent proceedings, has been fully confirmed. I could not be sure at the moment whether the movement of the "crescographic" spot to the right indicated growth or not. I am now satisfied that it did not, since it was made upon the upper half of a hyacinth (or daffodil) leaf of which the growth is exclusively basal. Obviously a repetition of the trial on the killed plant could not be made on the spot, but required time and the resources of a laboratory. I placed the resources of the Physiological Laboratory of the University of London at the disposal of the Society and of Sir J. C. Bose in all good faith, and in the expectation that he (or I) might become convinced of his (or my) mistake. My offer was repeated in the *Times*, of March 18, and by Professor Bayliss in the *Times*, of March 30, and accepted by Sir J. C. Bose in the *Times*, of April 3. A demonstration was arranged for by Professor Bayliss, in consultation with Sir J. C. Bose, at the Physiological Institute of University College on April 23. My reiterated requests to be allowed to assist at that demonstration were refused. A report by Professor Bayliss (and of other distinguished professors of University College and of the Imperial College of Science) was published in the *Times*, of May 4, and in *Nature*, May 6. That report is quoted by Sir J. C. Bose as having established his case.

In view of Sir J. C. Bose's confession of faith that he regards it as his duty "to stand up for truth and for freedom of inquiry," I shall still hope that he will not let the matter rest until the correctness of his theory is demonstrated beyond dispute. See also *Journal of Physiology* (Proc. Physiol. Soc.), June 19, 1920.—A. D. W.

his countrymen, including ourselves. He proclaims that it is open to students of all creeds and all nations throughout the Empire and the world. He extends to all students an invitation to come and, by union of mind and sometimes by clash of mind, help in the elucidation of truth. I would like to give, from this side, a cordial acceptance of that invitation, with the invitation that he should lay his head together with us here in London towards that same elucidation. Every laboratory in London is open to him. He has studied here, and is acquainted with our Western methods. It happens sometimes that an Indian comes to us and does things better than we do them ourselves. It happened in the case of cricket, it happens, perhaps, in the case of the physical application of these beautiful instruments which Professor Bose has been showing us at work. I should like to say, as far as the laboratory for which I am responsible is concerned, that I should be delighted to place it at the service of Professor Bose, and if this Royal Society of Medicine wished to invite other experts to examine further into these matters, the resources of the Physiological Laboratory of the University of London are at its full disposal. Therefore, in conclusion, with your permission, I would express, on my own behalf and on behalf of this audience, the gratitude we all feel to Professor Bose for the powerful impetus he has given to the welfare of Science by his foundation of the Bose Institute of Research.

Professor BICKERTON: It only requires, Sir, that someone should second this cordial vote of thanks which has been proposed to the Lecturer, and I have much pleasure in doing so. Having followed Professor Bose's work for a great number of years, I am absolutely delighted with the examples he has shown us to-night. One reads of these researches with a certain amount of interest, but when one sees the experiments in actual operation, it then suggests a series of phenomena which are wonderful. It seems a pity that these discoveries had been virtually suppressed for many years by the papers being relegated to the archives of the Royal Society. The President informs us that the value of Sir Jagadis Bose's work has now become fully recognized as evidenced by his nomination to the Fellowship of the Royal Society; this honour has been long overdue. But when we bear in mind the revolutionary character of Sir Jagadis Bose's discoveries it is no wonder that some time was necessary before his great work received the fullest honour and appreciation. I heartily support the vote of thanks.

The resolution was carried by acclamation.

Sir JAGADIS CHUNDER BOSE (in reply) said: I thank the audience for the warmth of their welcome and the interest evinced in my demonstrations. Dr. Waller spoke in appreciation of the message brought from the East, but it is not in that capacity that I am here this afternoon. I am here to announce and demonstrate new results which may clash with preconceived ideas, and exhibit new methods of experimentation by which phenomena, hitherto in the

realm of the invisible, are brought clearly to view. Science knows no geographical boundaries, and the test of truth is the same in the East as in the West. I go further and say that when anyone brings forward facts which challenge existing theories, then the burden of proof must lie with him, and his work ought to be subjected to the closest scrutiny; for without such precaution fact and fancy would become inextricably confused with one another. But criticism must be legitimate; instead of suggesting vague doubts, it must be directed to eliciting definite information in respect to the subject under inquiry. One speaker referred to the difficulties repeatedly experienced by me in publication of my results, till their recent acceptance. My special investigations have by their very nature presented extraordinary difficulties, and these have been greatly aggravated by misunderstanding or misrepresentation. The tide, however, turned, when I was able, six years ago, to bring my apparatus from India and give public demonstrations before the different Universities and Scientific Societies, including the Royal Society of Medicine; and it was a matter of much gratification to me that my work on the establishment of unity of physiological reaction in plants and animals received very cordial appreciation from the late Sir Lauder Brunton and from your Society. The success of my present visit makes me forget the painful incidents of the past, which may now be left to oblivion. I shall now meet the objections raised by Dr. Waller:—

(1) That the enormous magnification employed by me would, according to his calculation, enlarge a blood corpuscle to something like 70 metres in diameter. But if magnification is the objection, then all the great advances of knowledge made by the microscope labour under the same disadvantage.

(2) That the movement shown by my crescograph may not be due to growth, but to physical disturbance, such as that of variation of temperature. With reference to this, my paper on "Researches on Growth and Movement in Plants by means of the High Magnification Crescograph" was, after careful scrutiny by the Committee, accepted by the Royal Society and published last year.[1] In that paper full account is given for eliminating physical disturbance. It is there shown that *the physical elongation due to rise of temperature reaches a maximum, after which the indicator remains in a fixed position; whereas acceleration of growth by rise of temperature, as shown by quicker rate of movement of the indicating spot of light, persists indefinitely.* Dr. Waller has seen that paper, and I am at a loss to understand his omission in the present discussion of so important a part of my communication to the Royal Society. As regards the indications of my crescograph exhibiting induced physiological changes, my audience had the fullest opportunity of watching the enhanced rate of movement of spot of light when the plant was treated with a dose of stimulating drug, a slowing down of the movement being brought about by the action of a depressing agent. Nothing could be more grotesque and misleading than the suggestion that "fiddle strings" exhibited these characteristic physiological reactions.

[1] *Proc. Roy. Soc.*, B, xc, 1919.

(3) A further experiment was shown to decide between the question of physical and physiological action. I have shown how growth-movement becomes retarded by any mode of stimulation, by light, by rough handling, or by electric shock. This retardation has also been shown by me to culminate in an actual contraction under sufficiently strong stimulus; *the movement of elongation due to growth may thus be reversed to an opposite movement of contraction.* This important corroborative experiment was demonstrated before the audience by applying electric shock to the plant, when the normal growth-movement of the spot of light was reversed to the opposite movement to the left. This Dr. Waller regards as merely "de-growth," whatever that may mean. Dr. Waller adduces this as a proof of the dubious character of my demonstration; for according to him *the indicating spot of light should have stopped on the cessation of stimulus*, instead of which the spot of light persisted to exhibit contractile effect by continuing to move to the left. Dr. Waller is in error in supposing that the effect of stimulus on growth lasts during the application. Reference to my Royal Society paper, alluded to above, will show that the after-effect of stimulus, be it electrical, photic or mechanical, persists for a long period.

(4) But the most direct and simple test of discriminating physical from physiological movement would lie in repeating the experiments first with the growing plant, and secondly, with the same plant after it had been killed. If, under similar conditions, the growing plant exhibits movement in the crescograph, and the killed plant no movement, then the inference is clear and obvious that the crescograph does indicate the movement due to growth. Indeed, Dr. Waller himself suggested the desirability of such a test, and when I at once expressed my willingness to repeat this particular experiment, he waived his objection.

As regards the suggestion that my demonstration should be repeated before physiologists, I may say that a very large number of plant and animal physiologists have visited my laboratory and witnessed the experiments. Public demonstrations have also been given before the Universities of Oxford and Cambridge. The President kindly referred to the recognition of the value of my work by the Royal Society nominating me for election to its fellowship. This is a matter of gratification to me as an expression of goodwill and appreciation on the part of my numerous scientific colleagues in this country. And the best return that I can make for this confidence is to stand up for truth and for freedom of inquiry. We are at one both in the East and in the West for the spread of knowledge and for the removal of ignorance. We shall pass away, nations will disappear; Truth alone will survive, for it is beyond the reach of time and is eternal.

The Royal Society of Medicine.

President—Sir Humphry Rolleston, K.C.B., M.D.

OCCASIONAL LECTURE.

Toxic Idiopathies: The Relationship between Hay and other Pollen Fevers, Animal Asthmas, Food Idiosyncrasies, Bronchial and Spasmodic Asthmas, &c.[1]

By J. Freeman, M.D., B.Ch.Oxon.

(*Director of the Department of Clinical Bacteriology of St. Mary's Hospital.*)

There are a number of affections, apparently dissimilar, which for their study and treatment should be classed together; I have done so by calling them by the group named "Toxic Idiopathies." I hope to justify this name to you to-day; and I shall be able to make myself clearer if I work through my subject in the order in which the various problems actually occurred to me. This arrangement will give the advantage of proceeding from the better to the less understood parts of the subject.

Hay Fever.

Hay fever is one of these toxic idiopathies. Leonard Noon started work on this ten years ago; after only a few months he fell ill, and I have been working at the subject since then. It will be convenient to take hay fever as our type, but it will not be necessary, and we should not have time enough, to discuss hay fever exhaustively. All we need to do is to tabulate the facts which serve to link it up with the other toxic idiopathies to be discussed later.

[1] At a meeting of the Society, held March 15, 1920.

(1) *Causal Agent.*—Grass pollen stands in much the same causal relationship to hay fever that the tubercle bacillus holds in tuberculosis. In this sense grass pollen—and only grass pollen—causes hay fever; it does so whenever it comes into contact with the tissues of the susceptible person. It can do this at any time of the year but can only do so in nature when the grasses are producing pollen. Therefore the incidence of the disease shown in Diagram I corresponds, as one might expect, to the grass pollen production. Experimentally, however, the disease may be produced at any time of year by pollen which has been kept in a dry state.

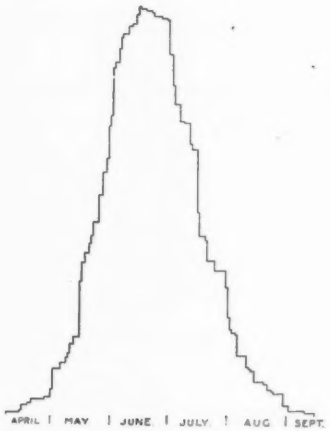


DIAGRAM I.

Curve showing case incidence of hay fever, compiled from some 300-400 cases studied in the last ten years.

(2) *Spontaneous Local Symptoms.*—Symptoms are experienced at every point where this pollen can get to work on the tissues of hay fever sufferers. As grass pollen is adapted for its purpose of cross fertilization by being extremely easily air-borne (it will even float in air from Norway to Yorkshire, as Blackley showed by his ingenious kite experiments), it is always present in the atmosphere during the grass flowering season except when it has recently been precipitated by rain. It will, therefore, come in contact with the external surface of the body; but the skin of most pollen-sensitive persons is too thick, too much covered by the dead horny layer of the epithelium, to permit any biological reaction except at the conjunctiva or the mucous membrane

of the air passages. Here, therefore, the local symptoms of hay fever are produced in greatest intensity: inflammation, irritation, œdema, coryza, lacrymation, and sneezing; but thin-skinned persons (chiefly women and children) may suffer itching, erythema, and even urticarial



DIAGRAM II.

Ophthalmo-reaction for diagnosing hay fever. First very dilute extracts of grass pollen, then progressively stronger extracts, are dropped on the eyeball of the patient till a slight itching and a faint redness develops at the inner canthus of the eye. The eye to the left in this diagram shows the normal condition; the other eye shows a rather strong hay fever reaction. The reaction should take about three minutes to develop.

wheals over the skin of the face and neck, and some very thin-skinned persons experience this over the entire body.

(3) *Artificial Local Symptoms.*—The natural symptoms can be reproduced at any time of the year, either with dry pollen, or more mercifully with a carefully diluted watery extract of the pollen. Diagram II shows the effect on the eye of a hay-fever patient, and this ophthalmo-reaction will either (i) serve as a diagnostic test at any time of the year of the pollen susceptibility which characterizes hay fever, or (ii) if the reaction is conducted with known dilutions of the pollen extract, an estimate can be made of the degree of susceptibility of that

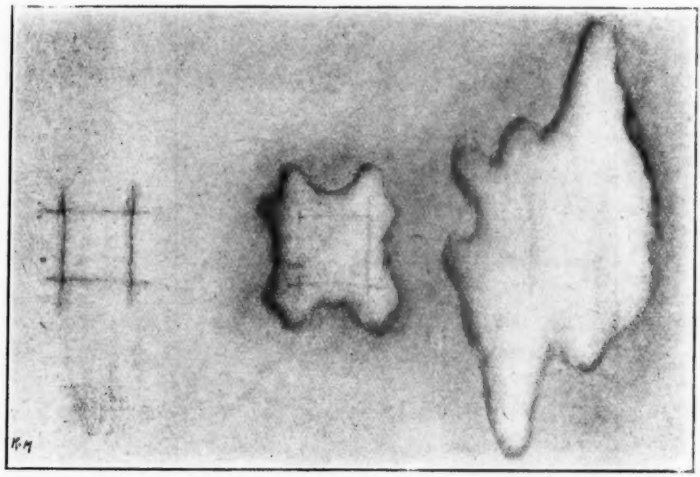


DIAGRAM III.

Cutaneo-reaction for diagnosing hay fever. Some convenient portion of the skin is scratched to remove the dead, horny layers and then a watery extract of grass pollen is placed thereon. The diagram shows to the left no reaction; in the centre a minimal but definite reaction; to the right a severer reaction produced by a stronger extract. This reaction takes from ten to fifteen minutes to develop.

patient. That the whole body is sensitive and not merely the eyes, nose and throat, is shown conclusively by producing reactions in abnormal sites. Dunbar placed pollen in the rectum and got the symptoms. More conveniently, one or two scratches through the horny layer of the epidermis will permit the pollen to act on the living tissues at any point of the body surface. Diagram III shows the effect

produced by pollen or by a watery extract of pollen on the scratched skin; and such a cutaneo-reaction can, like the ophthalmo-reaction be used either for diagnosis or quantitatively for measuring the degree of sensitiveness. Pollen experimentally swallowed with the food gives rise to nausea, vomiting, a feeling of indigestion, and diarrhœa. As this will be of special interest when we consider the toxic idiopathies connected with food sensitiveness, I will give a sample experiment to illustrate the point.

Example.—A young man in my hay fever clinic at St. Mary's Hospital recognized in someone who came to see me a brother in arms from the same battalion in the war; they readily agreed to split a bottle of beer which had been surreptitiously doctored with pollen. The normal man drank fair and experienced no sensations, but the hay fever patient had marked nausea for four hours, especially on exercise, and, what was most unusual for him, distinct looseness of the bowels for a day or so.

(4) *Constitutional* or distant effects are also produced by the irritant: lassitude, depression, irritability, headaches, general urticaria or œdema and asthma. Such general symptoms are produced by the natural disease and also by the experimental administrations of pollen at abnormal sites—e.g., by large hypodermic doses (large, that is, from the point of view of therapeusis or prophylaxis), or by incautiously severe skin reactions, or by the experimental ingestion of pollen in the food.[1]

(5) *The specificity* is very marked. None but hay fever sufferers will ever react to grass pollen, and grass pollen produces no effect on normal persons.

(6) *The commencement of attacks* may seem to the patient to be due to many things. When, but only when, grass pollen is in contact with the tissues (normally, i.e., when it is in the air during the times of incidence shown in Diagram I) the hay fever symptoms may be precipitated by a number of diverse stimuli—e.g., by any mechanical irritant, by any strong smell, "even the smell of my dinner"—any strong light, a change from cold to hot surroundings (and vice versa), by any strong emotion like the shock of bad news, or by suggestion. These statements are admittedly given without proof or explanation; perhaps the last of this somewhat indigestible list may be illustrated by comparison with sea-sickness. Vomiting may often be induced in

[1] Such experimental contact with pollen in abnormal situations may also produce the eye, nose, and throat symptoms of the natural disease. This would certainly seem to denote local sensitization, but this does not concern our argument here.

imaginative subjects—but only on board ship—by people speaking of sea-sickness or by someone imitating a rolling motion; in the same way sniffing at an artificial rose or merely listening to a vivid description of a sunny field on a hot still day, may produce—but only in the hay fever season and with hay-fever patients—fits of sneezing or the eye reaction shown in Diagram II. All these, as it were, "detonations" of hay-fever symptoms may profitably be borne in mind when considering some immediate "causes" of symptoms (e.g., of asthma) in the other toxic idiopathies.

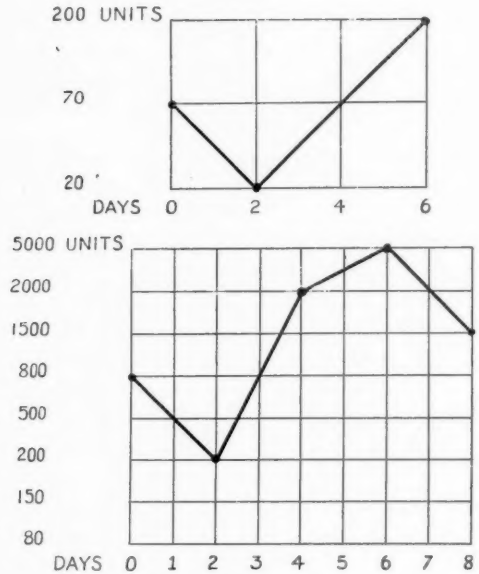


DIAGRAM IV.

The two curves show the effects on the sensitiveness of a hay-fever patient to grass pollen after hypodermic inoculation of a grass pollen vaccine. The upper curve is measured by the ophthalmo-reaction, the lower curve by the cutaneo-reaction. The vertical lines show days after the dose.

(7) *Passive immunization* may be produced against hay fever; that is to say, one may immunize a horse or other animal with grass pollen, and the serum of such an animal will give some degree of alleviation from the symptoms. This work was carried out by Dunbar of Hamburg, the serum being known as pollantine: and it is important to note that pollantine will give definite relief, though this is so slight and transient that the effect is negligible if the case is at all severe.

(8) *Active immunization* with a vaccine made from the grass pollen works well, both for prophylaxis and for therapeusis. Noon selected his doses by tracing with the ophthalmo-reaction their effect on his patients; one can also use the cutaneo-reaction. The two curves in Diagram IV show the changes in sensitiveness which follow the administration of a pollen vaccine. One is measured by the ophthalmo- and the other by the cutaneo-reaction method. Though there are, of course, failures to deplore, yet from 80 to 90 per cent. of hay-fever cases inoculated during the last ten years have been well pleased to have been so treated, and with many of these the results are really striking.

DIAGRAM V.

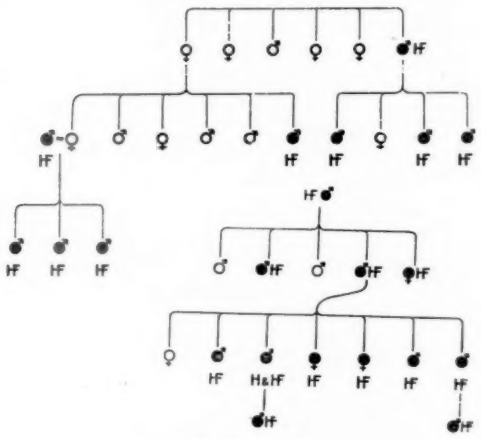


DIAGRAM VI.

Diagrams V and VI show a toxic idiopathy where the conventional sign for male and female is blackened in, and hay fever is denoted by HF. The letter H in Diagram VI denotes horse asthma.

Example.—A man had hay-fever so badly that for three months in every year he had to go to sea. One year, through pressure of work, he risked staying in London and, as a result, spent two months in a dark room, with his body packed with hot-water bottles to stop the shivering fits, and his head packed with ice-bags, the conjunctiva skinning off his eyeballs and his lungs gasping for breath. Since his inoculation treatment began nine years ago, that man has been able to work through each summer, and has been getting progressively better; last year he told me that for half-an-hour one afternoon he "felt a slight annoyance from the old enemy."

(9) *Heredity* is strongly marked in hay fever, as Diagrams V and VI show clearly enough: the monogram made by compounding the letters H and F indicates in these and in succeeding genealogical tables the incidence of hay fever. (In Chart VI, where H is added the individual had horse asthma as well as hay fever—the significance of this is mentioned later.) In going over my records last summer I was able to pick out some two hundred well-marked examples of heredity, any one of which could have served my argument. Heredity, we shall find subsequently, is a link which closely binds together the different toxic idiopathies.

For portability we may summarize the foregoing characteristics of our typical toxic idiopathy as: (*a*) the *symptom complex* both of the natural and also of the artificial disease; (*b*) the *specificity*, both of the irritant for that particular patient and of the patient for that particular irritant; and (*c*) *heredity*.

Other Pollens.

Other pollens than grass pollen have this toxic power—they will react in the eye or on the skin of those sensitive to that particular pollen, thereby causing reactions precisely like those shown in Diagrams II and III. Also these pollen sensitivenesses tend to run in hay-fever tainted families—that is to say, they will either affect sufferers from hay fever or their close relatives. Diagram VII shows in tabular form the response which a number of hay-fever patients and two relatives of hay-fever patients show when tested with grass pollen and a random collection of twenty-five other pollens. From this we may judge that practically every pollen one cares to test will react on some person or another with the hay-fever taint; but note how capricious is the selectiveness of the different pollens, reacting with some hay-fever tainted persons strongly, with other persons reacting only weakly and with others again not at all. One must regard each as being the irritant of a separate toxic idiopathy: each is a potential cause of a fever parallel to hay fever.

How, then, can one say that, in England at any rate, grass pollen causes the only pollen fever which counts? Because in this country grass pollen alone fulfils all the three essentials for a considerable disease; these are: (1) production in sufficient amount; (2) capacity for producing a strong reaction; and (3) dissemination by light wind. For example, many varieties of daisy pollen are frequently strongly toxic and produced in

considerable quantities, but this pollen only flies yards where the grass pollen flies miles. Again, the pollen of the firs and pines is produced in large quantities and is fairly easily borne on light airs, but proves to be only slightly toxic for most persons with the pollen-sensitive taint

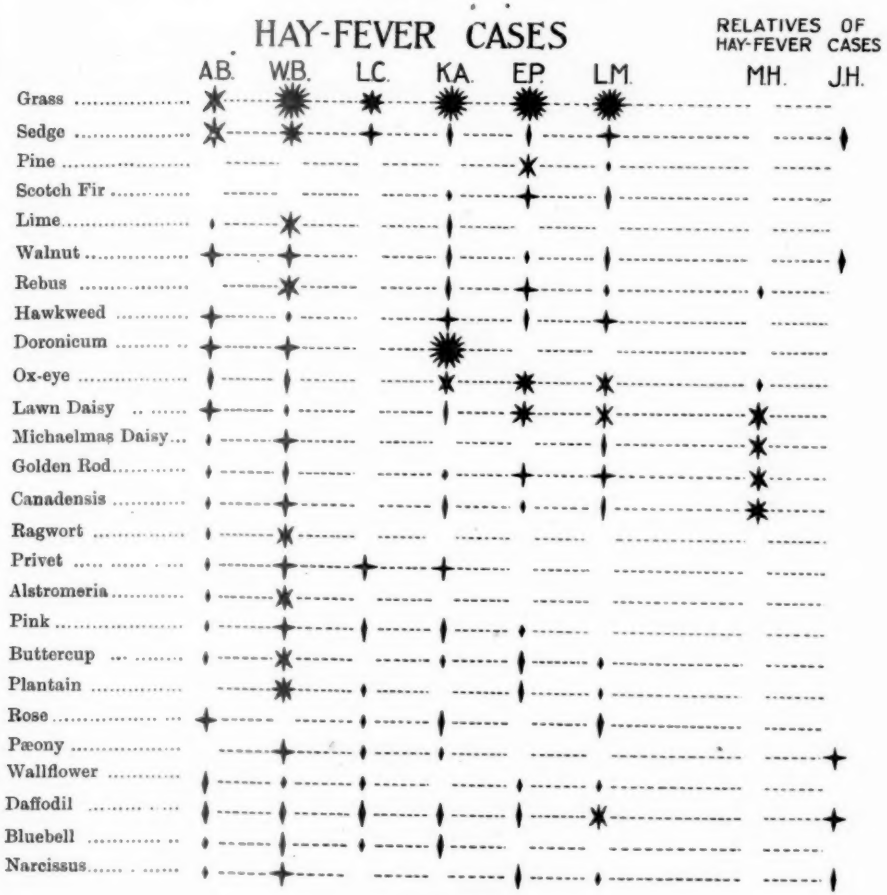


DIAGRAM VII.

Here the extent to which the patient reacts to the pollen in question is denoted by the number of points to a star. The names of plants to the left of the diagram give the type of pollen employed for the particular reaction, and the initials along the top indicate the individuals upon whom these tests were carried out. The first six are hay-fever sufferers, the two individuals to the right are relatives of hay fever sufferers and appear in Diagram XIII, marked with an asterisk.

(Diagram V chances to over-represent the toxicity of the coniferæ). In America, however there is an indigenous race of composites which provoke an intense reaction in those who happen to be susceptible, and, in addition, the pollen is produced in large quantities and is very easily wind-borne. As a result, in America there are two pollen fevers, one occurring at the time of the flowering of these composites—viz., in August, September and October—which is known as the American autumn fever, and another at the time of the flowering of the grass, which happens about the same time as in Europe, and is, therefore, known as the June fever. It is this latter alone which is rightly called hay fever.

There is much talk in America and elsewhere of pollen fevers caused by other trees and plants, but time will show if any of them are of much importance. As we have seen, every one of the pollens may produce toxic effects on susceptible people, and it is merely a botanical accident as to whether the particular toxic idiopathy in question makes a serious trouble for humanity or not.

Animal Asthmas.

Eight or nine years ago hay fever seemed to stand alone, but Dunbar told me that the horse-sensitiveness known as "horse asthma" was an occasional symptom, or at least was never found except associated with hay fever; this, it now appears, is not always true, but why it should frequently be so will be shown when discussing heredity presently. The irritant is usually air-borne and so the local symptoms are similar to those of hay fever: the victims get red eyes, a running nose and much sneezing, and a tickling throat when near horses, also constitutional symptoms such as are found in hay fever. These symptoms are most marked if the animals are lathering freely or in an ill-ventilated stable. With "horse substance" in place of grass pollen one can carry out parallel reactions to those used for diagnosing or estimating the sensitiveness to hay fever, and Diagrams II and III would serve equally well for horse-asthma reactions. Normally it is the horse-dandruff—i.e., the debris which will come off on a curry-comb—which picks out for us the horse asthmatic; but almost any part of the horse—the flesh, the serum, the fæces, or the mucous discharge from the nose—will provoke the reaction, though to different degrees. Furthermore, other members of the horse tribe share with horses this power to react with a horse-sensitive person, as Diagram VIII shows.

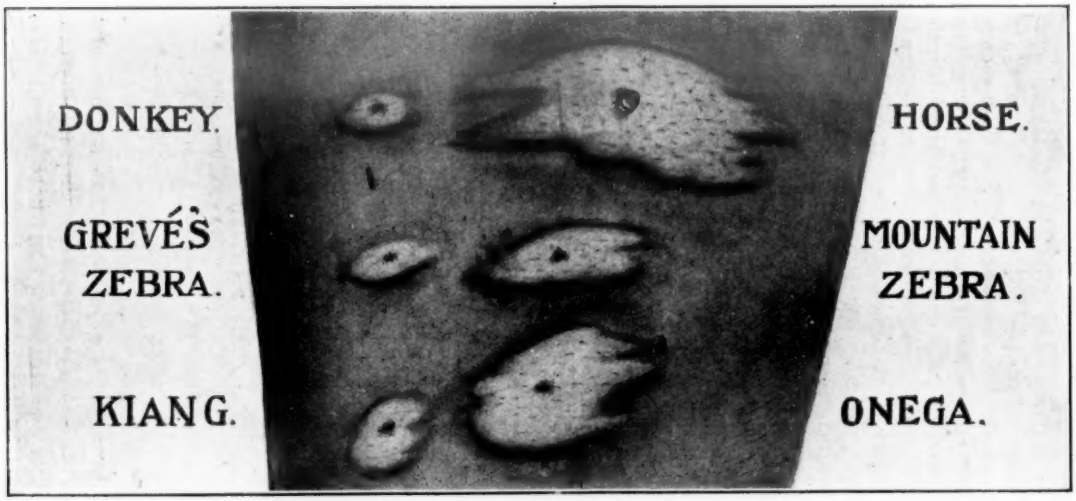


DIAGRAM VIII.

[Shows the typical response of a horse asthma patient to various members of the Equidæ. As is usual, the reaction is strongest to horse dandruff and least to donkey dandruff.

A couple of examples will bring out most of the characteristics and make it a definite disease-entity for us.

Example.—An officer in the Horse Guards developed horse asthma and so had to resign his commission; also he had to give up hunting and avoid hunting men until they had changed their clothes and washed after riding. He quickly learned how to avoid the trouble; but once, when shooting in Scotland, put on his macintosh because it was raining. Almost immediately he began to have bad asthma, could not put a gun to his shoulder all day, and on returning to the house found he had put on the macintosh belonging to a "horsey" man, who always wore it when on horseback or kept it rolled up on the horse's withers. The same patient, when stopping in a house where a case of diphtheria had occurred, received from the local practitioner a dose of anti-diphtheritic serum, which, of course, is obtained from horses. As a consequence they sat up with him all night, and were very relieved that he survived. He finds London nowadays very much more comfortable because of the motors, but if there is any wind on a dry day he has to bolt for home because his eyes get affected by the horse dung which constitutes the bulk of London street dust.

Another Example.[1]—A lady had for years been noted as a horse-asthma patient. Anybody who touched a horse's skin and then rubbed it on the skin of this patient would produce a great urticarial wheal, or if on the face would, probably, make the eyes bloodshot. Her serum given to a guinea-pig would passively sensitize it to horse serum given forty-eight hours later—human serum and horse serum being, of course, in themselves quite innocuous to guinea-pigs. This same lady was out at a luncheon party one day, and developed terrible asthma in the middle of the meal; suspicion fell on some sausages, which, by serological tests were proved to contain horse meat. On another occasion horse-serum made her very ill, but relieved her horse-asthma for four months.

The ramifications of this strange illness are many. The necessity of avoiding curative sera in such cases is obvious; not so obvious is the necessity for avoiding horsehair mattresses or upholstery if the patient is very sensitive.

Cat, and other Animal Asthmas.

Parallel with horse-asthma you have cat asthma, in which various substances from the cat (particles from the fur, the serum, the urine, &c.) provoke skin or eye reactions, like those shown for hay fever in Diagrams II and III.

[1] De Besche, of Christiania, in *Berl. klin. Wochenschr.*, 1918.

Example.—A patient, who had some post at Court, told me that he could not bear the presence of cats. They made him gasp for breath, so that he could not stay in the room with them and would subsequently give him asthma. When you are in the presence of royalty I understand, you are, so to speak, on parade; you must not go until dismissed. But on one occasion a cat strolled into the room where the King and this patient were talking. The patient clenched his hand and tried "to stick it out," but, almost immediately, incontinently bolted and had to make his explanations later.

Similarly we have a sensitiveness to sheep, cows, pigs, rabbits, goats, and all the rest of them.

Example.—A man who has always hated the presence of rabbits or rabbit meat or rabbit skin. He is devoted to shooting, but has to get the keepers to put him where there have not been rabbits about; otherwise he knows that he will not hit a bird.

Among these animal asthmas should be included a sensitiveness to the emanations from feathers of birds. This would seem at first sight more trivial than, say, a cat asthma, but is really much more important when it exists; a little tumbling about of pillows, bolsters, quilts, sofa cushions, and soft upholstery in general, will send the feather particles into the air. In the words of a patient, "You must put your head on the pillow as if on a wasp's nest." Even the swan's-down powder puff may cause trouble.

We summed up hay fever, our type of a toxic idiopathy, under the three headings of symptom complex, specificity and heredity; now let us see how the animal asthmas correspond under these headings.

Symptoms.—As far as circumstances permit these are identical both in local and constitutional effects. The experimental symptoms also correspond. The respective ophthalmo- and cutaneo-reactions are indistinguishable; an injection of an extract of curry-combings (or, as we have seen, of antidiphtheritic serum) causes the same symptoms as those which follow an over bold dose of grass pollen extract in the hay-fever subject. Similarly eating horse or rabbit produces much the same effect on those sensitive to that particular animal as the experimental ingestion of grass pollen by a hay-fever subject.

Specificity.—Here again they correspond. The horse asthmatic is sensitive to horse substance and not necessarily to anything else. Each animal substance is sharply specific for its own type of asthmatic, but a patient may suffer from two or more animal asthmas at a time, just as the hay-fever patient may be sensitive to other pollens—e.g., to ox-

eye daisies or peonies. Again, as I have said, the horse asthmatic may also have hay fever (this occurs in perhaps 40 per cent. of my horse asthma cases); or he may be quite non-sensitive to pollen; or he may have hay fever early in life, grow out of it, and then develop some animal asthma later in life. The different animal asthmas are separate or, at any rate, separable, toxic idiopathies.

Heredity.—In the hereditary nature of the diseases the animal asthmas correspond to our type, but as they are each of them much rarer than hay fever, it is difficult to demonstrate many, say, of horse asthmatics in one family. On reviewing many family histories, we find

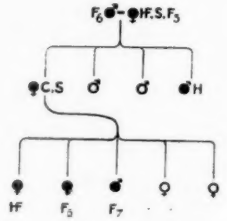


DIAGRAM IX.

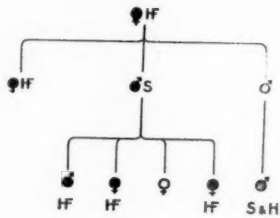


DIAGRAM X.

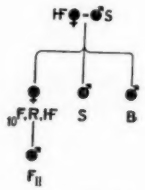


DIAGRAM XI.

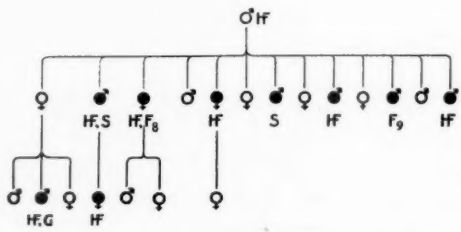


DIAGRAM XII.

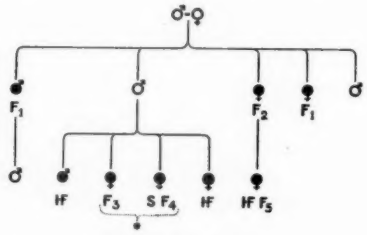


DIAGRAM XIII.

Diagrams IX, X, XI, XII, and XIII.—As in Diagrams V and VI, a blackened male or female sign denotes a toxic idiopathy, while the nature of that idiopathy is denoted by the letters attached: HF, Hay fever; H, Horse asthma; C, Cat asthma; G, Goat asthma; R, Rabbit asthma; B, Bacterial asthma; S, "Spasmodic" asthma; F, Food idiosyncrasy, and the number attached denotes the particular food which reacts: 1, Lobster; 2, Lobster, anchovy and duck's egg; 3, Oysters; 4, Mackerel; 5, Honey; 6, Apples; 7, Chicken; 8, Oysters and lobsters; 9, Milk; 10, Onion; 11, Fish.

good evidence of heredity, and also that different animal sensitivenesses tend to run in the same family (suggested here by Diagram IX, where a sister is sensitive to cat and a brother to horse). It is quite clear, even from the few diagrams of heredity here published, that the

animal asthmas tend to run in the same families as the hay fever cases; we have here Horse in Diagrams VI and X, Horse and Cat in IX, Rabbit in XI, and Goat in XII—all related to, and half of them coinciding with, hay-fever cases (besides coincidence with and relation to the toxic idiopathies to be mentioned next). We have seen that sensitiveness to any particular pollen other than grass tends to run in hay-fever tainted families; so that all our toxic idiopathies tend to be inherited together—there is some common taint.

Bacterial and Spasmodic Asthma.

From my first experience, hay-fever patients insisted on the occurrence of bronchial and spasmodic asthma in their families; but to begin with these complaints were discarded as having no possible connexion with a sensitiveness to pollen. After some time, however, I had to acknowledge that these diseases occurred with significant frequency. Here, again, it is necessary to work through very many family histories to reach full conviction, but Diagrams IX to XIII show eleven cases of spasmodic asthma between them and one of bronchial asthma. Of these the bronchial asthma and four cases of spasmodic asthma stand alone, though closely related to hay fever and other toxic idiopathies; two cases of spasmodic asthma coincide with hay fever; one with cat asthma; one with horse; and two with toxic idiopathies to be described later.

For many years before the war we of the Inoculation Department of St. Mary's Hospital (and doubtless many others elsewhere) had been inoculating against bronchial asthma by immunizing against the bacterial cause of the bronchitis, and frequently with strikingly successful results; and we were driven to the conclusion that the asthma was the remote effect of the bacterial infection. I believe now the asthma is a toxic idiopathy to the endotoxins of the infecting microbe.

The specific ophthalmo- and cutaneo-reactions of the pollen-sensitive or animal-sensitive persons (Diagrams II and III) are exactly paralleled by the ophthalmo- and cutaneo-reactions, produced by the extracted endotoxins on the bronchial asthmatic,[1] and such reactions are

[1] One may get this bacterial cutaneo-reaction in cases of bacterial intoxication (e.g., a bad case of pyorrhœa) which do not have symptoms of asthma urticaria, &c. It is believed that such symptoms would have been produced if the patient had had the toxic idiopathy taint, but may get rheumatism or gout instead, if the diatheses happen to be in this direction.

bacterially specific—e.g., a man infected with Pfeiffer's *Bacillus influenzæ* will react to that particular microbe on the scratched skin. Again, just as violent asthma, œdema, and urticaria can be produced by injudicious injections of the specific irritants in hay-fever and animal-asthma subjects, so will the same symptoms be produced by too large an injection of the appropriate bacterial vaccine in a case of bronchial asthma, especially by the first dose or two of any series of inoculations.

It is important to note that for bacteria to produce asthma, they need not be infecting the bronchi; they may, for instance, be infecting the gut wall; thus, perhaps, paralleling the effect of pollen in the food of a hay-fever subject, or the specific meat in the food of an animal asthmatic.

Example.—A bad case of asthma of several years' standing came to be treated by vaccines, but was refused, because no infection of the bronchi could be found and the case was, apparently, one of "spasmodic asthma." At a second visit a history was obtained of chronic bowel infection, slight pain and recurrent diarrhœa. A bacteriological examination showed an infection of pyocyaneus.[1] A vaccine was used; the bowel disturbances stopped and the asthma symptoms promptly stopped also. (Personally communicated: Dr. John Matthews.)

This case suggests: (i) That bronchial asthma is a regional subdivision of bacterial asthma, and that spasmodic asthma had better be classed as asthma of unknown origin. It may be an undetected case of any one or more of the toxic idiopathies described or to be described here, but it may, no doubt, be due to some unsuspected air-borne irritant such as the emanations or the spores from moulds—an idea which some cases suggest.

With an unknown irritant one cannot make experiments to prove specificity but from the symptoms and, above all, from the heredity, we must class spasmodic asthma as one of the toxic idiopathies; it remains to make it an asthma of known origin.

This was very much the stage the subject had reached before the war, when the work had to be dropped. But in the United States of America much valuable work continued to be done on bronchitis, hay fever and, in particular, on food idiosyncrasies; these are frequently

[1] This organism is much more pathogenic than the text-books allow.

grouped together under the name anaphylaxies.[1] After the war I was anxious to see if my collections of hay fever and other toxic idiopathy cases suffered from any particular food idiosyncrasies. They did, giving quite surprising tales of which a few examples will suffice to give the clinical picture and show that these disabilities are by no means trivial, or "just fads and imagination."

Example.—"F.6." The male ancestor in Diagram IX was a remarkably healthy man, but apples always upset him. On his honeymoon his wife, after the manner of Eve, persuaded him to eat one; he spent the next twelve hours with violent vomiting and diarrhœa.

Example.—"HF. F.5." One of the granddaughters in Diagram XIII could never since childhood tolerate a trace of honey. After many years' abstinence she took half a teaspoonful as an experiment. Within seven minutes she "came over all queer," and made her way as best she could to her bedroom where she was found unconscious on the floor. She, too, spent the rest of the day with vomiting and diarrhœa. She gives the cutaneo-reaction as in Diagram III, either with direct honey or with the trace of proteid precipitable from honey solution.

Example.—"F.2," the mother of the last example in Diagram XIII used to be sensitive to eggs; urticarial rashes came out whenever they were eaten. She has gradually accustomed herself to them, but remains sensitive to the rarer duck's egg. This is corroborated by a strong positive reaction with duck's egg proteid and no reaction with that of hen's egg.

Example.—"F.9," in Diagram XII, vomited so uncontrollably when put on cow's milk that, to save his life, he had to be given ass's milk. If he had chanced to be horse sensitive, Diagram VIII suggests that the change of dietary would have been equal to "jumping out of the frying pan into the fire."

Example.—A girl, aged 9, had all her life suffered from very bad "spasmodic asthma" accompanying the slight bronchitis which she occasionally had; a streptococcus vaccine was made, but gave little or no relief. There was a confused story of delicacy of digestion and tendency to dysentery (when one has had it all one's life one cannot describe nausea) consequently skin reaction tests were made, which disclosing a large number of sensitivenesses, amongst them reactions to horse and to hen feather; but most pronounced of all were the reactions to the various flours; strongly so to wheat and barley and very strongly to pea, bean and lentil flour.

The *symptoms* fall into line with those of toxic idiopathies in general, closely resembling what we have seen to follow from pollen,

[1] The use of this term would seem to be unfortunate, since the phenomena of the toxic idiopathies do not fall into line with those of anaphylaxy as demonstrable on a guinea-pig. "Anaphylaxy" has grown so protean nowadays as to cover almost any untoward biological reaction—and therefore, when used so loosely, explains nothing.

horse meat, or a bacterial infection in the intestines of persons with those particular sensitivenesses. The constitutional troubles, depression, asthma, urticaria, &c., follow along the same lines too, and one gets ophthalmo- and cutaneo-reactions precisely like those shown in Diagrams II and III.

The *specificity* of the particular irritant for the patient and of the patient for that particular irritant, is as exact as with, say, hay fever or cat asthma.

Heredity is frequently found to the same food sensitiveness, but more often the food sensitivenesses vary in a family. Common inheritance, together with all the other toxic idiopathies, is seen from the diagrams, where "F" denotes some food idiosyncrasy and the number following it denotes the particular food.

Before leaving these food idiosyncrasies some general considerations are worth while. They vary in intensity, from a shock producing coma down to a hardly noticeable malaise, and when slight they are usually not detected. Frequently one gets without leading questions the histories of "delicate digestion," or of being "faddy about food," from hay fever patients, which turn out to be quite well marked but unsuspected food idiosyncrasies. Another difficulty, both in the general diagnosis of a food idiosyncrasy and in specific diagnosis of the particular irritant, is that the reactions are frequently delayed, both in the natural disorder and in the diagnostic cutaneo-reaction; the delay may sometimes extend from, say, ten minutes up to as long as fourteen hours or longer (this delay also sometimes takes place with the bacterial cutaneo-reactions). So food idiosyncrasies often are unsuspected. Therefore in an anomalous case of dyspepsia, it would be well to look for a family history of toxic idiopathies of any type. If the patient comes of a family so tainted then the so-called "dyspepsia" may well chance to be a food idiosyncrasy.

Another point worth comment is that food idiosyncrasies are most frequent in childhood. From the day the child is weaned he is constantly meeting new proteid substances in the form of food; and as a rule children have an instinctive dislike to new foods or foods prepared in unrecognized ways. Possibly any new type of food requires an immunizing effort on the part of the recipient to make that food wholesome and any failure to so immunize would produce a food idiosyncrasy. Under the eye of a stern parent, intent on seeing that the "children are not faddy" and that "good food is not wasted," the dislike and difficulty is soon got over with normal children.

One point of special interest is suggested by one of our examples given above; it must be well worth while to test the cutaneo-reactions of persistently vomiting babies for a milk sensitiveness. Some of the patent foods may work well in exceptional cases for this reason (a case of a child who could tolerate egg substitutes but not egg itself is instructive). In a thesis by L. De May for the University of Lyons, thirty-two cases of uncontrollable vomiting and diarrhœa of infants were successfully treated by subcutaneous injections of cow's milk to produce "anti-anaphylaxis after the method of Besredka."

Worms.

Many other curious sensitivenesses to foreign proteids must apparently be included amongst the toxic idiopathies, but I must be brief. There is, for example, the fact that worms which, when handled or dissected for some time, give all the nasal and eye symptoms of an air-borne toxic idiopathy; and occasionally one finds that children with intestinal worms, who happen also to possess the toxic idiopathy taint, apparently have asthma as a consequence and lose the asthma after the removal of the worms.

Insect Bites.

The bites and stings of animals are another occasion when small quantities of foreign proteid come into intimate contact with our tissues; and it does seem as if people of the toxic idiopathy taint sometimes give much stronger reactions to the bites of fleas or gnats than the normal person does. I will give a case in point:—

Example.—Clewes, of Buffalo, U.S.A, who first introduced the use of a pollen vaccine for hay fever into America, told me when recounting his success, that the most successful inoculation he had ever given was to a lady who had a house on the borders of Lake Erie. She was being inoculated at the time for hay fever, but was so sensitive to the bites of small gnats from the lake that one single bite, which would hardly have been felt by the normal individual, would cause such swelling as to send her to bed for days. Clewes got a number of these gnats collected, brayed them in a mortar, and made a vaccine therefrom. After several inoculations the patient could lie out in a hammock at the edge of the lake and be bitten with pleasure.

This sounds a tall story, but she was hay-fever tainted, and if the bites and stings of insects can give rise to toxic idiopathies with people of the correct sensitiveness, then it is quite understandable.

Conclusion.

I hope I have made it clear that all these apparently diverse affections are linked together in an extraordinary way by (i) their selectiveness; (ii) by the symptom complexes; and (iii) by heredity. This is worth doing for several reasons. First, the very fact of the connexion, and still more so the connecting link if only we could discover it, must have great significance in immunology. Secondly, the detection of one definite toxic idiopathy in a patient should put us on the alert to discover others. And, lastly, anything we learn about one will help us in dealing with another toxic idiopathy; this refers chiefly to treatment, for what is sauce for the goose is likely to be sauce for the gander. Excluding the palliatives, we have choice of two treatments for all of them: (1) avoiding the particular irritant, or (2) immunizing against it.

I think it might be exaggeration to say that the study of these toxic idiopathies will open a new field of medicine, but I feel confident that they throw light from a new angle across a very large field of the old medicine.

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Section of Anæsthetics.

President—Dr. Llewelyn Powell.

Intratracheal Insufflation of Ether in Operations which involve Bleeding into the Air Passages.[1]

By F. E. Shipway, M.D.

It is some ten years since Elsberg first used intratracheal insufflation of ether in the human subject, and seven and a half years since it was demonstrated at Liverpool by R. E. Kelly. Sufficient time has therefore elapsed to enable the statement to be advanced with some confidence that the value of this method has been established. It has, however, for various reasons, made somewhat slow progress in this country. The method has not, I think, suffered any set-back as the result of misguided enthusiasm on the part of its friends. It has only had to meet the opposition of its critics, who have either resisted its advance from an excessive caution, or from a mistaken conception of its difficulties. Its failure to secure more general adoption may also be due to the disturbing effect of the war on civil practice. On the other hand, injuries caused by the war were the means of bringing the method more prominently before the notice of those whose previous experience of it was slight. Its merits and demerits have long been known to some, and it is, I think, their duty to render an account of the knowledge they have gained in order that others may be induced to readjust their views and to abandon their attitude of hesitation. My own experience is limited to some 930 cases; in 407 of these the method was used for operations on the face, mouth and pharynx, which were accompanied by bleeding into the air-passages. Since the claim has been advanced that intratracheal ether finds its most useful oppor-

[1] At a meeting of the Section, held November 7, 1919.

tunity in intra-oral surgery, I have chosen these cases, to the exclusion of others, as the subject of my paper. The cases have been arranged in three groups:—

(I) Gunshot wounds of the face, mouth and pharynx.
(II) Non-malignant disease of the mouth and pharynx.
(III) Malignant disease of the mouth and pharynx.

(I) This group contains 142 cases. It is marked off from (II) and (III) by the greater uniformity of age and a higher level of general physical condition. On the other hand, the average length of anæsthesia was greater; many of the operations lasted over one and a half hours, twenty lasted two hours or more, the duration of the longest being three hours and seven minutes.

Plastic operations	117
Fixing wires or splints	22
Taking impressions	3
	142

(II) There were 89 cases, the ages ranging from 8 to 70:—

Enucleation of tonsils	11[1]
Operations on frontal and nasal sinuses	40
Operations on dental tumours and cysts	17
Multiple extractions, &c.	6
Various	15
	89

Intratracheal ether was used on a few occasions for extraction of buried wisdom teeth when a long anæsthetic was required; it is an ideal method for these cases. There were six children in all in this group under 12; average duration of anæsthesia forty minutes.

One of the most interesting cases in this group was that of removal of polypi in a man aged 58; he was a fisherman, weighing 18 st., who had long been subject to attacks of urgent dyspnœa due to extreme nasal obstruction. Examination showed the presence of a mass of polypi bulging down the soft palate to a considerable extent. The choice of anæsthetic was not easy, but, for various reasons, I decided to give him intratracheal ether. Induction was unusually difficult owing to obstruction to breathing caused by a large tongue and the bulging of the palate, and it was not until the surgeon, Mr. Steward, held up the palate with his finger, whilst I held forward the tongue, that an airway could be obtained and induction completed. Five per

[1] In one case, a boy, aged 11, cleft palate was operated on at the same time.

cent. cocaine was applied to the epiglottis and the catheter passed. Anæsthesia was completely satisfactory. Some weeks later intratracheal ether was again given to him for the removal of more polypi.

(III) This group is subdivided into (III*a*) cases in which gland operation and operation on primary growth were done at the same time: (III*b*) cases in which primary growth alone was removed.

The classification of these cases has not been easy, and I have decided, after much consideration, to follow that which was adopted by Mr. Wilfred Trotter in his Hunterian lectures;[1] it is based upon the anatomical distribution of the growth. I have, however, not excluded cases of cancer of the lip, and have placed under a separate heading cases of cancer of antrum, ethmoid, &c., which have necessitated removal of the superior maxilla; the latter do not appear in Mr. Trotter's figures.

(III*a*) 43 cases, in 9 of which glands on both sides were removed.

Mouth	(1) Lip	6
	(2) Tongue (oral part)	19
	(3) Floor	10
	(4) Gum	4
Pharynx	(1) Oropharyngeal	2
	(2) Epilaryngeal	1
	Removal of superior maxilla	1
		43

The youngest patient was aged 30, the oldest 73. There were seventeen cases between ages 60-70, and four over 70. The duration of anæsthesia was necessarily long, rarely being under one hour. In one case it lasted one hour fifty-three minutes, in another two hours five minutes. The average duration of the lip cases was fifty-five minutes, of tongue seventy-three, of floor seventy-five, of gum eighty.

(III*b*) 133 cases:—

Mouth	(1) Lip	8
	(2) Tongue (oral part)	47
	(3) Floor	8
	(4) Gum	11
	(5) Cheek	10
Pharynx	(1) Oropharyngeal	15
	(2) Epilaryngeal	2
	(3) Pyriform sinus	1
	(4) Hypopharyngeal	2
	Laryngeal	2
	Removal of superior maxilla	25
	Removal of growth of nose	1
	Lateral rhinotomy	1
		133

[1] *Lancet*, 1913, i, p. 1075.

The youngest patient was a girl aged 12, the operation being the removal of superior maxilla; she made an excellent recovery. Two patients were aged 80; and two operations for cancer of the tongue were performed on a man aged 79. Of the two octogenarians, one had a small recurrence of cancer of the tongue; operation lasted twenty-five minutes; recovery was perfect. The other had a growth of antrum; this case is referred to later. There were fifty-six cases between ages 50-60, twenty-nine between 60-70, and, excluding those mentioned above, eleven over 70. The average age of the superior maxilla class was considerably less than that of the rest of the group. Of the two laryngeal operations, one patient, aged 69, was suffering from a growth of the anterior commissure which was causing some distress in breathing; laryngo-fissure was done but the growth was inoperable and tracheotomy was performed. Anæsthesia was in every way satisfactory. In the other, tracheotomy was also performed, and the catheter was placed in the tube. This procedure has been found quite satisfactory on two other occasions. In the oropharyngeal group operation was limited in one case to enucleation of a sarcomatous tonsil. Five operations in this sub-group (III*b*) lasted over two hours; the longest took two hours forty-eight minutes; recovery was good and uneventful. Average duration of anæsthesia in the lip cases was thirty minutes, tongue thirty-seven, floor fifty-nine, gum fifty-five, cheek forty-four, oro-pharyngeal sixty-five, superior maxilla seventy-six.

Intratracheal ether is a great asset to both surgeon and anæsthetist in this difficult field of surgery. It makes for the peace of mind of both. The careful dissection and deliberate technique of these operations are not hindered by anxieties about the anæsthetic, nor hampered by the close presence of the anæsthetist. The catheter presents no obstacle to the surgeon, the area of operation can be kept sterile, and the quiet easy character of the respiratory movements much facilitates dissection. For the anæsthetist the administration is extremely simple, the breathing is unobstructed and the colour remains good. The presence of blood in the mouth and pharynx is without danger to the patient.

Before proceeding to discuss the incidence of complications, the mortality, &c., I wish to comment on two points, one of which is connected with the technique, making it clear at the same time that it is not my intention to describe either the principles or the technique of this method; they are sufficiently well known. The first point—of supreme importance in the operations of group (I)—is the difficulty

which may sometimes present itself of passing the catheter; the other is the advisability of giving a preliminary narcotic. With regard to the former, it is certain that the anæsthetist, however skilful he may be in the use of the direct laryngoscope, is at a considerable disadvantage compared with the laryngologist. He has to induce an anæsthesia of sufficient depth to abolish (almost completely) the sensitive pharyngeal reflexes and to pass the catheter before these reflexes return, or partially return. The laryngologist has the help of an anæsthetist in keeping these reflexes in abeyance. It is, then, wise for the tyro to recognize that he must approach the laryngologist for instruction, and practise under his eye the use of the direct laryngoscope under the most favourable conditions of posture, relaxation and quiet. It is, of course, obvious that with increasing experience the difficulty of exposing the cords decreases. Until this dexterity has been acquired, advantage should be taken of the anæsthetic action of cocaine. I advocated this plan some years ago,[1] used it for a considerable time, and am convinced that it is a safe and very helpful procedure. It is particularly helpful in conditions in which light anæsthesia is indicated, and meets the criticism that has been made to the effect that the depth of anæsthesia necessary for passing the catheter prohibits the application of the method in cases in which its use might be specially advantageous. Such cases are, for example, those of Graves' disease, or of certain types of intestinal obstruction with vomiting, in which intratracheal ether is highly valuable. Now deep anæsthesia is not necessary. But if the anæsthetist is doubtful of success in any given case in which light anæsthesia is indicated, he should, I submit, cocainize the pharynx and the epiglottis, either before or during induction, with a 5 per cent. solution. As a matter of fact, I abandoned intratracheal ether for cases of Graves' disease shortly after oil-ether anæsthesia was introduced, for I believe that by no other method of general anæsthesia can the technique of Crile be so well carried out. At the same time the direct laryngoscope and catheter are always kept at hand during these operations, in case any difficulty of breathing should arise. Such an occasion did occur recently, and although anæsthesia was of the type so frequently associated with oil-ether and it had been necessary to give the patient a little C.E. at intervals, there was no difficulty in passing the catheter. Nor has any difficulty been met with in cases of intestinal obstruction,

[1] *Brit. Journ. Surg.*, i, 1913, p. 105.

in which deep anæsthesia is dangerous, although it is only fair to admit that here the conditions are somewhat different; these patients are ill, and toxæmia has dulled their reflexes.

There were, however, some conditions during the war which were abnormal and presented at first very real difficulties. I refer to cases of gunshot wounds of the face and jaw in which there had been much loss of tissue, with subsequent scarring, contraction and deformity. These were especially common in the early days of the war when treatment was non-existent or inadequate, and the type of neglected case which I had to anæsthetize at the Maxillo-Facial Hospital again taught me that something more than a knowledge of the usual is demanded of our profession. Complete failure awaited me on six occasions, and on two or three of them not even a partial view of the larynx could be obtained. Failure occurred in the case of the same man on two different occasions. This was a serious matter, for the ordinary methods of anæsthesia gave distinctly poor results, and there was some hesitation in opening the windpipe, particularly as this procedure might have to be repeated on subsequent occasions. Kühn's tube was not available, and as I felt strongly that intratracheal ether was the best method for these patients, it became necessary to look at the difficulties afresh and from every point of view, and to try to find a solution. Reflection showed me that proper account had not been taken of the altered relations of parts, and that the head-down position used by the laryngologists is the worst possible for these cases of contracted mandible. All difficulties usually disappeared when the patient was raised to the semi-recumbent posture and the head somewhat flexed, and on the third occasion of my anæsthetizing the man on whom I had practised unsuccessfully twice before, the catheter was passed with ease. Probably by a scarred and adherent tongue was presented the most serious obstacle in these war injuries, but by a careful examination of the altered anatomy beforehand and by attention to posture, intubation should be successful. I do not mean to imply that I met with no difficulties: some of these patients were very difficult, particularly if the mandible had been splinted, but if success was not attained at the first attempt sufficient knowledge of the special obstacles had been gained to enable one to succeed at the second. Above all things good illumination is essential. I have gone somewhat fully into this question, in the hope that my experience may be of use to others.

The second point—viz., the advisability of giving a preliminary narcotic, is of almost equal importance, and in coming to a decision

several factors must be taken into account. Two which at once present themselves are the predilections of the surgeon and of the anæsthetist—their views may be totally opposed,—and idiosyncrasy on the part of the patient. The latter is rare, and occurs only when comparatively large doses are given. Other factors are the age and condition of the patient, the nature of the operation and the probable length of anæsthesia. As regards the very young, I feel myself to be on pretty safe grounds in stating that morphine is contra-indicated. To no patient, young or old, should morphine-scopolamine be given. As regards old age, the decision is not so easy, although it might appear at first sight that in this particular type of operation the anæsthetist should use no agent, in addition to the anæsthetic, which would have the effect of delaying the return of the reflexes. This objection is valid if the patient is anæmic or toxic or asthenic, and may be upheld in the epilaryngeal and hypopharyngeal groups. Anæsthesia is usually prolonged in them, and a post-operative tracheotomy may be necessary in order to prevent any difficulty in breathing when the catheter is removed. There would seem to be a considerable body of feeling in favour of the withholding of morphine from patients who have been tracheotomized. On the other hand, if we exclude these groups, morphine offers such great advantages to the patient and to the anæsthetist who is using intratracheal ether, that he should hesitate long before deciding against its exhibition. One of these advantages is the absence of excitement or struggling, or the lessening of struggling during induction. It is notorious that the victims of cancer of the mouth are frequently elderly men who have indulged freely in alcohol or tobacco, men, that is, in whom great excitement during induction is to be expected. Now struggling represents a serious loss of energy and throws an excessive strain upon an impaired cardio-vascular system. It is, unhappily, such a comparatively common event that it is looked upon as of no material import, and there is danger that the fact may be lost sight of, that over-exertion or severe effort sustained over even a short period of time on the part of the conscious subject is sometimes followed by acute cardiac distress or syncope. Violent struggling is, therefore, I believe, not infrequently the cause of sudden death following operation, or of gradual cardiac failure during the first forty-eight hours of the post-operative period. In the latter case the waste of energy during induction and the strain of operation, added to the inability to partake of solid nourishment, constitute a tax upon the resources of the patient which may easily turn the scale

against him. For these reasons, I advocate, whenever possible, a small dose of morphine, preferably $\frac{1}{6}$ gr. for the elderly patient.

Morphine also enables the anæsthetist to maintain a much lighter anæsthesia; this is particularly true of intratracheal ether. The administration is constant and the control extremely delicate. What exactly constitutes light anæsthesia is almost a matter of personal opinion and practice. I believe that, in general, anæsthesia is maintained at too great a depth. But whatever difficulty there may be in maintaining light anæsthesia with the ordinary methods, there can be none with intratracheal insufflation, a system that delivers the anæsthetic with an even precision below all possible sources of obstruction or spasm. The zone of anæsthesia which I endeavour to reach—and it can usually be reached—is one in which the eyeballs are constantly moving and the corneal reflex is extremely brisk; it is sometimes possible to demonstrate the accuracy of Elsberg's[1] statement that a condition can be produced in which the patient will open and close his eyes and give no evidence of pain sensation. The explanation of this phenomenon probably lies in the fact that artificial respiration is in itself an anæsthetic measure; it may also be that there has been some diminution in the CO_2 content of the blood. It is certain that the minimum amount of anæsthetic can be used and the fullest advantage taken of the narcotizing power of morphine. The anæsthetic, too, can be blown out before the end of the operation and the patient sent off the table with full control of his reflexes. It has happened occasionally that following a difficult catheterization, blood has found its way into the trachea. No harm has resulted. The danger of this occurrence has been exaggerated. Provided that the material which invades the lungs is non-infective, and is small in amount, there need be little fear of septic pneumonia. We have here an additional argument in favour of appropriate doses of morphine combined with intratracheal ether. I have never seen any untoward effects, and the ward sister, who has had many of these patients under her charge, has not found it a disadvantage from the nursing point of view. Each case, of course, must be judged on its merits, and there should be hesitation in its use before full control of the intratracheal method has been acquired.

The claim that intratracheal ether abolishes the risk of aspiration or septic pneumonia has been made good. No case has occurred in this series. Mr. Trotter has objected to the use of intratracheal ether for

[1] *Ann. Surg.*, 1911, liii, pp. 166, 167.

cancer of the tongue on the ground that septic material from the growth may be conveyed by the direct laryngoscope to the upper opening of the larynx and thence aspirated into the lungs. This, if upheld, would be a fatal objection. But apart from the fact that this has not occurred on one single occasion, although many of these growths were large and ulcerating, it must be remembered that direct laryngoscopy gives a clear view of the whole field, the inner surface of the spatula is sterile, and the catheter can be inserted without contamination. Further, contact of the catheter with the tracheal mucous membrane usually brings into play its strong protective reflexes, or these reflexes can be quickly elicited. I think, then, that this objection falls to the ground but it emphasizes the importance of attention to detail. Whether the impossibility of aspiration of blood, septic material, &c., is due entirely to the force of the outgoing stream of air, is not clear to me; it is certainly true in the case of animals, where the pressure used is probably higher than that which I have used in the human subject. But, I believe, of almost equal importance with the pressure of the air-stream is the great decrease of inspiratory efforts; no air is drawn into the trachea during inspiration. The point is of practical as well as theoretical interest, for upon the answer depends the safety of the method for the semi-recumbent and upright postures.

Another point arises here, viz., whether the interruptions in the air-stream at regular intervals are necessary or advantageous. Theoretically the result of the interruptions is that the lungs collapse slightly, drive out the CO_2-laden air, and at the moment of the resumption of insufflation receive a stream of fresh air. The gaseous interchange in the lungs is thereby promoted. In fact, the interruptions act as a sort of artificial respiration and thus add a factor of safety. Now a high pressure maintains the lungs in a state of distension, which impedes the venous flow to the heart. Interruptions, therefore, are necessary under these conditions but there is, I believe, less need for them when a moderate pressure is maintained, such as exists when the safety-valve blows off at 20 to 25 mm. Hg. In intra-oral operations, again, it is probably wiser, if the field is septic or the hæmorrhage free, to reduce the number of these interruptions; the same remark applies to cases of intestinal obstruction with vomiting, although, strictly speaking, they do not come within the scope of this paper.

The apparatus in common use in this country allow only of a partial reduction of the volume of the ingoing stream; whether this calls for modification is uncertain. Some light is thrown upon this question by

the experience gained in using this method on the human subject for purposes of artificial respiration. It is related of cases of this sort that the insufflation and the interruptions have had to be supplemented at intervals—whether long or short, regular or irregular, I do not know—by compression of the chest in order to overcome a gradually increasing cyanosis. Obviously a thorough interchange of gases could not be maintained. Samples of alveolar air taken from these cases would have proved of great interest.

Preference is expressed by some anæsthetists for the foot bellows in place of the motor-blower, on the ground that they are in more intimate touch with the patient. Fischer[1] uses a hand-pump as the pressure is not constant, but there is always a slight rhythmical fall of pressure at every movement of the lever, which favours a thorough interchange of gases. My own preference is for the motor-blower, for the very reason that the pressure of air and ether is constant and the control of the depth of anæsthesia is more delicate. An objection to the foot-bellows in prolonged operations is that it becomes very wearisome; interest in the patient wanes. The longest time for which I have used it was three hours twenty minutes; this was too long.

A few cases have developed acute bronchitis, six in all, and these chiefly amongst the emphysematous and chronic bronchitic. This freedom from a somewhat serious complication is remarkable when the advanced age of many of the patients and the length of operation are taken into account. It is to be attributed to the use of atropine, the weakness of the vapour and its warmth. One patient developed bronchitis following lateral pharyngotomy for the removal of a carcinoma of the sinus pyriformis; the operation lasted two hours six minutes. Later the sputum became foul, the lungs having become infected from a septic laryngeal wound. It is the opinion of the surgeon that infection was post-operative. No other case was severe or gave rise to any anxiety.

One of the great merits of this method is that shock is reduced to a minimum. Intratracheal insufflation is essentially a method of artificial respiration. All the needs of respiration are met, and intermittent asphyxia is eliminated. It is, indeed, a luxurious method and calls for no effort on the part of the patient. There is, in consequence, a great saving of energy, which enables the most extensive operations to be undertaken with a high degree of confidence. There is, indeed, a danger that more may be attempted than is wise. The one-stage

[1] *Surg., Gyn., Obst.*, 1911, xiii, p. 566.

operation for cancer of the mouth may be a serious risk in a patient who has passed his sixtieth year, or even fiftieth, bearing in mind the habits and average condition of this group. It is of interest to note that this operation has been in my experience much less common in the last three years, and there has been a corresponding reduction in the mortality.

I have seen a severe degree of shock develop towards the end of the gland operation and pass off completely as soon as the skin flaps were brought together. Shock was here due to exposure and cooling. It is probable that the temperature of the operating room was too low. It has occurred usually in the small thin type of patient, and at first caused me some anxiety in those cases in which it had been decided to remove the primary growth at the same sitting. Experience, however, has taught me that a very considerable improvement can be looked for when the neck has been covered up, sufficient to allow of a safe completion of the operation.

No deaths occurred in Groups (I) or (II).

The mortality in Group (III*a*) was small, viz., two deaths out of forty-three cases. In one of these death was due, I believe, partly to mechanical difficulties but chiefly to the depressing effects of a large dose of morphine and scopolamine. These drugs had been given in error by a house-surgeon, and the breathing was with difficulty kept free when the catheter was removed. The patient died suddenly next day; post-mortem examination was negative except that the heart was small and fibroid. Death in the other case followed an extensive one-stage operation for a growth of the floor of the mouth; the man was an extremely unfavourable subject, flabby and a very heavy drinker, but his condition was so miserable that he was anxious to run any risk and preferred the small chance of a complete cure rather than a palliative operation.

In Group (III*b*) there were five deaths, one being due to pulmonary embolism the same evening. A boy aged 16 died the evening of operation from hæmorrhage combined with shock after removal of the superior maxilla and the eye. Death occurred in two cases on the third day. In the first, an extensive operation had been performed for a growth of the tonsil, following removal of the glands a week previously; this had left the patient anæmic. In the other, a feeble spare man aged 71, one-half of the mandible had been removed; the breathing was not satisfactory unless the epiglottis and tongue were held forward with a finger, some blood was aspirated and tracheotomy was necessary.

This case, and the case in Group (III*a*), emphasize the truth of Mr. Trotter's statement—a statement of the utmost importance to surgeon and anæsthetist—that "the shock of an attack of even partial asphyxia is very apt to be fatal in an enfeebled patient." The benefits of intratracheal anæsthesia can be thrown away in a few minutes. These fatalities were partly attributable to error of judgment and not to the method of administration: in the first, the operation should have been postponed; in the second, anæsthesia was deeper than necessary when the catheter was withdrawn. Death occurred after removal of the superior maxilla in an old lady aged 80. She was a very healthy woman and was anxious to have the operation as she was suffering much from a rapidly growing tumour of the antrum. In order to lessen hæmorrhage the external carotid was ligatured, but the vessel was tortuous and difficult to define. This half of the operation was done under C.E. and oxygen and lasted forty minutes. The second half, under intratracheal ether, proved also longer than anticipated, part of the soft palate being involved. The duration was forty-seven minutes. The condition of the patient at the end was good, the forehead warm, pulse 84, reflexes very brisk, but she gradually became weaker and died on the fourth day of exhaustion and basal pneumonia.

Disadvantages urged against the intratracheal method, such as cost and bulk of apparatus, should carry no weight. A minor objection is the bubbling of blood and mucus in certain operations: much can be done to remedy this by efficient sponging, holding the jaw forward, the use of nasal tubes and other manœuvres. But no one of these disadvantages is of sufficient importance to justify the refusal to adopt a method which has been proved, both in America and in this country, to have so much real value. In war surgery of the face and jaw it was indispensable, and it is a matter of regret that the neglect to use it more frequently led to the result that wounded soldiers lost their lives on the operating table. It is not as if the method, or the conditions, were insuperably difficult. I fear the explanation must be that there were lacking a clear appreciation of the value of intratracheal ether and a close acquaintance with the technique. The taking of impressions, fitting and fixing of splints, wiring of the jaw—these are operations which are attended with much depression of the mandible and pushing back of the tongue, frequently in the presence of suppuration. Here are conditions which favour aspiration-pneumonia and cardiac failure if the ordinary methods of anæsthesia are used.

These dangers were, of course, recognized early and substitute-

methods adopted where possible. But none of them is as efficient or of such wide application as the intratracheal method. Crile's tubes have a limited field. Kühn's tube is bulky and impedes the operator, and as Mr. P. P. Cole[1] has pointed out, "the effort of breathing through it is a physical strain that cannot be neglected." The reason is physiological. The tube is long and of small bore; oxygenation is deficient and rebreathing excessive. Chloroform and oxygen in the upright position is not a safe method except in the hands of the expert, and even then may give cause for considerable anxiety. Opening the windpipe is seldom if ever indicated in military surgery, if, indeed, it is always possible. I remember a case, in the early days of the war, of an officer suffering from a fractured mandible, much neglected and very septic, with a septic condition of the skin of the neck. Laryngotomy or tracheotomy was out of the question, and it was doubtful whether intratracheal ether could be used in such a dirty condition of the mouth. It was, however, quite successful, and similar cases were anæsthetized throughout the war without any complication arising. If due care be taken in passing the catheter no septic material can be carried down with it.

Puncture-laryngotomy was used on a few occasions by Mr. P. P. Cole, at King George Hospital, and was abandoned at the request of the patients, who complained that the wound was painful and prolonged their stay in bed; their neighbours, who had had intratracheal ether, were able to get up sooner.

The question arises whether the administration of the anæsthetic through a laryngotomy or tracheotomy tube is a better method in cancer of the mouth than intratracheal ether. It has been said that cancer cells may be implanted further down the air passages by the introduction of the direct laryngoscope. This is a difficult matter to prove, and hardly comes within the province of the anæsthetist. The danger, however, cannot be overlooked, and it would seem that if contact with the growth cannot be avoided—e.g., in cases where the growth is large, and is situated in the mid-line, the advisability of opening the windpipe should be seriously considered. My experience of this type of growth is small and information on the point is welcomed. Information is also wanted on the relative mortality of the two methods. It may be taken for granted that aspiration pneumonia is not a cause of death in either. It will be seen from the results in Group (III) that the death-rate

[1] *Proc. Roy. Soc. Med.*, 1917, xi (Sect. Anæsth.), p. 10.

from shock is low, but here again the figures may be misleading, for the epilaryngeal and hypopharyngeal cases are relatively few in number. From the point of view of the anæsthetist, chloroform is not such a good anæsthetic as ether for extensive and prolonged operations. It is too depressing. Ether, on the other hand, especially when given by modern methods, is known to be capable of sustaining the blood-pressure over long periods and to have rid itself of its former reputation of setting up irritation of the lungs. The results that have been obtained in other fields of surgery favour the use of ether, and I believe that the figures given in this paper show that chloroform has received another set-back. I would submit, then, that the intratracheal insufflation of ether is a very valuable method in intra-oral surgery, can be used with confidence in the young and the old, has abolished the risk of aspiration-pneumonia, and has much reduced the mortality.

DISCUSSION.

The PRESIDENT (Dr. Llewelyn Powell): The results obtained by Dr. Shipway are excellent. I have not his skill or experience, but I feel that in many of the cancer cases a preliminary laryngotomy is a more convenient method. In the epilaryngeal cases is not his catheter rather in the surgeon's way?

Mr. F. F. BURGHARD: I am very much indebted to the intratracheal method of anæsthesia for operations upon the tongue and mouth. Those who have operated upon extensive cancers of the tongue by this method must have been as surprised as I was at the perfect smoothness of the anæsthesia, the total absence of leakage of blood into the air passages and the freedom from interference with the surgeon's work by continuous sponging. There is no need to sponge more often than is necessary to get a good view of the field of operation; it is quite unnecessary to sponge the upper airway. This method is greatly superior to preliminary laryngotomy; in the latter, not only is the patient exposed to such risks as the operation entails, but he is not rendered immune to the danger of blood getting into the air passages, for it is very common for an assiduous assistant to pull out the sponge plugging the pharynx in the efforts to keep the field of operation clear. The risk of carrying infection to the lungs directly via the catheter appears to be imaginary. Experience shows that the septic pneumonia does not follow the method, and no case of implantation infection of malignant disease has been so far charged to it. When the cancer reaches close down to the upper laryngeal orifice it may be a little difficult to keep out of the way of the tube, but this is a difficulty easily got over with a little care and is counterbalanced by the security against entry of blood into the air passages.

Dr. CLARENCE H. MOTT: Does Dr. Shipway still pass the pharyngoscope by the angle of the mouth, instead of the mid-line? Also, does he think that the employment of a larger pharyngoscope (for adults) facilitates the finding of the glottis, because I have found it much easier to locate the glottis in children with this? Surgeons sometimes object to the strong ether vapour proceeding from the mouth, and that is one reason why some prefer a preliminary laryngotomy. Does not the selection of a rather smaller catheter than that usually recommended often seem to give a better anæsthetic? I have found that the stomach (and even the intestines) blow up not only from an intra-œsophageally placed catheter, but also from intrapharyngeally placed (nasal) tubes. Dr. Gwathmey states in his book that in such a case (intra-œsophageal) the air would probably regurgitate as fast as it entered.

Dr. HUGH PHILLIPS: Owing to the difficulty I found in passing the catheter, I have used Kühn's tube with a certain amount of success.

Dr. F. ROOD: Dr. Shipway recommends in his paper the introduction of the catheter into the trachea under light anæsthesia. Is not this proceeding more likely to be followed by trauma of the larynx with consequent laryngitis and hoarseness on account of the spasmodic gripping of the catheter by the larynx if the reflexes are not abolished, than if deep anæsthesia is used when the cords are naturally widely abducted and the catheter can be introduced without even touching the cords? We are indebted to Dr. Shipway for his hint upon the posture of the patient in certain difficult cases. I had a case the other day in which, on account of a well developed upper jaw and a strong set of teeth with at the same time an underhung lower jaw, I found the greatest difficulty in even getting a view of the epiglottis, much less lifting it up in order to introduce the catheter into the larynx without exercising a force which would have in all probability broken the patient's upper teeth. By using the manœuvre suggested by Dr. Shipway and flexing the patient's neck until the chin rested almost upon the sternum I was able easily to lift the epiglottis forwards, get a good view of the cords and pass the catheter downwards between them in an almost vertical position.

Dr. FREDERIC LONGHURST: I have had but small experience in the intratracheal method for mouth cases, and such experience has not taught me whether there is, or is not, a risk of blood entering the trachea. I understand Dr. Shipway to say that in some cases, some blood may have got into the trachea, while Mr. Burghard in praise of the method, says that he is never afraid of that happening. I should like to be made clear on this point. Dr. Shipway says he would never employ any other method for exophthalmic goitres; I suggest that the anæsthesia deep enough to allow of a sufficient interval for laryngoscopy and intubation is unadvisable and unnecessary, as I never failed, so far, in maintaining a pure ether anæsthesia for these cases, by means of my combined tongue tractor and tube attached to the rest of the intratracheal apparatus—the ether being delivered over the base of the

tongue, and the hand holding the tractor being no nearer the site of operation than the patient's forehead.

Dr. SHIPWAY (in reply): I want to make it clear that a small quantity of blood may find its way into the trachea following a difficult introduction of the direct laryngoscope, such as happened occasionally at King George Hospital, when a granulating wound or a recent wound was pressed upon by the instrument. Blood cannot enter during insufflation. I have long abandoned intratracheal ether for exophthalmic goitre. Technique was purposely excluded from my paper, but I would say in reply to Dr. Mott that a strong ether vapour shows that anæsthesia is too deep. I think Dr. Rood has misunderstood my remarks on the depth of anæsthesia to be used in introducing the catheter. Deep anæsthesia is not necessary, but is used in all cases in which it is not dangerous, the point being that skill should be acquired in introducing the catheter under light anæsthesia. I do not think that laryngitis is likely to follow this practice.

Two Types of Portable Gas-Oxygen Apparatus.

By GEOFFREY MARSHALL, M.B.

I HAVE here two types of portable gas-oxygen apparatus. I will not take up your time by discussing general principles. Gas-oxygen, which is the most physiological of general anæsthetics, will, I believe, in time become the routine method; it can be administered by a facepiece or by intrapharyngeal or intratracheal tubes. Before the method can come into general use we must have apparatus that is light, simple and compact. These two models represent my attempts at solving the problem. In the first (fig. 1), the carrying-case itself forms the stand, and after raising the hinged lid, it is only necessary to attach the sight-feed to the top, and slip on the rubber connexions and light the spirit lamp, to make the apparatus ready for use. The general principles of this outfit are similar to that of Dr. Gwathmey and others. The new details are:—

(1) Fine adjustment and reducing valves have been dispensed with; regulation is effected by means of long lever keys working on the ordinary cylinder taps. The control thus obtained has proved perfectly adequate and satisfactory, and this system not only eliminates weight and expense but has also proved surprisingly economical in expenditure

of the gases. The latter point indicates that leakage is very apt to occur when fine adjustment valves are used. The oxygen and N_2O are led by flexible tubes to the sight-feed, and here is to be seen the second new feature :—

(2) The two metal tubes which pass down into the water in the sight-feed bottle curve outwards so that the five side holes of each tube do not come vertically beneath each other, and when I turn the gas full on you will see that the streams of bubbles from the various holes rise separately to the surface making it more easy to see how many holes are delivering, and for still greater clearness there is a metal boss marking the first and third holes of each tube. This arrangement has the additional advantage that there is less agitation in the water in the sight-feed bottle. The diameter of the N_2O holes is twice that of the O_2 holes. The gases mix over the water and pass through a Y-connexion which acts as a water trap. The latter is fitted with a clip so that any water collecting in it may be emptied out at will. Having passed through this Y-connexion the gases are led through the graduated cock on the ether bottle so that varying proportions of the gases can be bubbled through the ether as required. Hence a long rubber tube delivers the gases to the three-way Hewitt valve.

(3) This is the third new detail. You will see that the gases are delivered in front of the rebreathing bag and as near as possible to the mask, with the advantage that newly delivered mixture continually displaces expired gases within the facepiece, reducing dead space to a minimum. Experiments with Dr. G. R. Phillips in France convinced me of the value of such an arrangement for avoiding the effects of excessive CO_2 concentration when rebreathing is employed.

You will see that the stand holds two 50-gallon cylinders of N_2O and one 15-gallon cylinder of O_2 ready for use and this will be sufficient for about ninety minutes' anæsthesia; in addition there is room for a spare cylinder in the lid of the case.

The second apparatus (fig. 2) is similar in principle, but is designed for carrying in an ordinary anæsthetist's leather bag. The stand is composed of steel tubing, the sections of which bolt together. Its disadvantage is that it takes five to ten minutes to unpack and prepare for use, but, on the other hand, it is very easy to carry; when resting on the floor it is sufficiently tall to raise the feed bottles to a convenient height, and, lastly, it is "mobile," so that there is no difficulty in anæsthetizing the patient outside the theatre and then

following up the wheel trolley with the apparatus when induction is completed.

With regard to technique I turn on one hole oxygen and four of N_2O, set the Hewitt valve half way between "valves" and "no valves," and when the bag is half full apply the mask to face. After two minutes I fasten the mask in position by means of a double strand of fine rubber

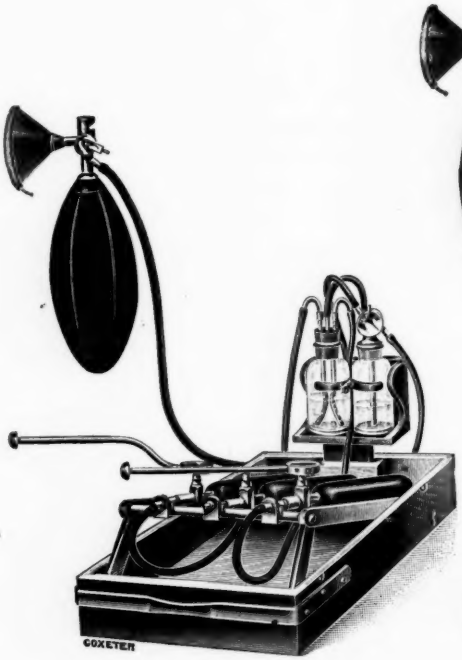


FIG. 1.

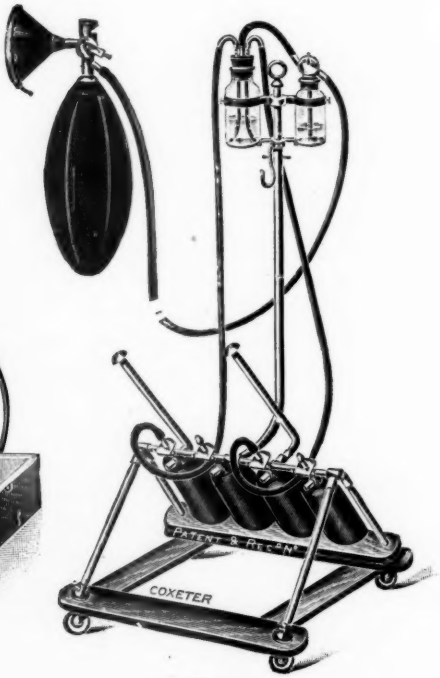


FIG. 2.

Fig. 1.—First apparatus ("portable set").

Fig. 2.—Second apparatus ("combined set"). Above illustration shows wooden frame with castors, an accessory I have discarded. It does not show the water-trap between sight-feed and ether-bottle.

tubing, one end being passed round the nape of the neck and the other round the occiput. If there be any tendency to struggle ether should be turned on during the period of a single breath, and this process repeated every few breaths until induction is complete. Cyanosis

should be avoided throughout the administration. It will probably be found necessary to increase the O_2 flow to two holes after about ten minutes and to three or four holes after half an hour. If there be any inspiratory obstruction this will probably be due *not* to falling back of the tongue but to sucking together of the lips, and it can be remedied by inserting a short rubber tube between the lips; there is no necessity to pass it between the teeth.

I have found gas-oxygen an admirable anæsthetic even for infants under two years of age. For these cases I dispense with bag and face-piece and insert the end of my delivery tube into a Schimmelbusch mask, covering the frame with an impervious material such as jaconet or rubber sheeting; the material should be cut wide so that it falls as a curtain round the edges of the mask. Induction is peaceful and a quiet anæsthesia with good pink colour is easily maintained.

Apparatus for Nitrous Oxide, Oxygen, and Ether.

Shown by F. E. SHIPWAY, M.D.

AN attempt is made in this apparatus, which is a modification of that devised by Dr. G. Marshall, to combine easy control with portability. It can be packed with two 50-gallon N_2O cylinders and one 15-gallon O_2 cylinder into the bag which is made to hold Hewitt's gas-oxygen apparatus, and is quickly assembled.

The stand consists of (1) a wooden base and an upright rod (made up of three lengths which slip into one another) which carries a steel three-ringed holder for the gas cylinders, and (2) a unit consisting of the mixing chamber and ether bottle. The bottles in the latter are interchangeable, and the unit carries a hook which can be slipped over the back of a chair, or hung on the rail of a bed. The height of the apparatus can be altered at will, by removing one or more of the upright sections; it can be used with the base standing on the floor, or on a table or chair. It takes three cylinders in the upright position, two of N_2O, and one of O_2; either vertical or horizontal cylinders can be used of 25, 50, or 100-gallon capacity, so that the anæsthetist can use any combination he thinks desirable. If a vertical bottle is used in the centre, the bayonet-shaped section of the upright allows of the use of a

hand-key. Long levers are used for regulating the flow of the gases; should the valves be tight at first opening, the cylinders should be steadied with the hand. A spirit lamp, to keep the valves of the N_2O cylinders from freezing, can be used with either the vertical or horizontal pattern.

For hospital use, the construction of the stand is slightly modified.



A, wooden base; **B**, upright rod; **C**, holder for cylinders; **D**, unit; **E**, mixing chamber; **F**, ether bottle; **G, G**, levers; **H**, spirit lamp; **K**, water-trap.

Apparatus for Nitrous Oxide and Oxygen.

Shown by W. J. McCardie, M.B.

I have found that the admirable gas-oxygen apparatus of Mr. Boyle which I possess, and with which I have had excellent results, is rather large and heavy for transportation. I have therefore had made by Messrs. Coxeter one which is divided into three units and have added some of the distinctive features of Mr. Marshall's modification, such as curved sight-feed tubes, long levers, and no reducing valves. An extra bottle has also been fitted. The three units comprise:

(1) Two 100-gallon cylinders of nitrous oxide contained in a special wooden case like that of Mr. Marshall. This unit can be separately used with a foot-key for dental work.

(2) Two oxygen cylinders of similar size in a similar case. This unit also can be separately carried and used in surgical work, and contains enough oxygen if it be delivered close to the epiglottis to suffice for any ordinary surgical emergency.

(3) Three bottles coupled up instead of the usual two and contained in a small close fitting case, half of which is lifted off before use. The bottle stand is supported during administration by detachable metal rods which fit into the sides of one of the cylinder cases. By attaching the bellows this unit can be used like a Junker's inhaler for administering ether or chloroform.

These three units can be easily transported and packed in corners of a vehicle and can be quickly put together and placed on a table. Consequently one does not hesitate to employ 100-gallon cylinders rather than smaller ones. The third bottle is intended to contain anesthol, or a mixture of chloroform and ether if preferred, in cases of lung trouble or for special work where ether is undesirable. Anesthol is a mixture of ethyl-chloride, ether and chloroform, used years ago in America and lately recommended by Gwathmey in certain cases. Finally, the objects I have aimed at in this apparatus are:

(1) Portability.

(2) Units which can be separately used in other methods of anæsthesia.

(3) Greater scope by addition of an extra bottle for other anæsthetics than ether.

DISCUSSION.

The PRESIDENT: Mr. Geoffrey Marshall has told us that gas and oxygen is to be the only anæsthetic of the future, but I notice on each of these machines there is an ether bottle.

Dr. HUGH R. PHILLIPS: I have obtained better results with using chloroform either by itself or in a mixture, especially in obtaining good relaxation, than with pure ether.

Dr. ROOD: Most of the criticisms directed against gas and oxygen are due to the title of this form of anæsthesia which is surely a misnomer. Why not call this method a modified and much improved method of giving gas, oxygen and ether?

Mr. BOYLE: I agree with everything Mr. Marshall has said about gas and oxygen. I am quite convinced that gas and oxygen is the best anæsthetic I have ever used, and am certain that it will be the anæsthetic of the future. It is not perhaps strictly correct to speak of the method as gas and oxygen, for in a certain proportion of cases it is advisable to give a small amount of ether, and in some cases C.E. mixture: but the amount of ether or C.E. is so small as to be almost negligible. Under these circumstances, and to satisfy the purists, it would be more correct to speak of the method as gas-oxygen-ether anæsthesia. I find that the addition of a little ether or C.E. during the latter part of the induction period gives a much more even and quiet anæsthesia later on, even though no more ether or mixture be given. I have now had some considerable experience with the method, and the more I see of it the more I like it. I am delighted to find that other men are now turning their attention to devising machines, and as far as I can see all of the machines exhibited are excellent. They all aim at a really good portable machine, and for this lightness is very important. In the machines exhibited, however, there is rather a risk of exploiting lightness at the expense of cubic content, and I do not think that the small amount of oxygen that these machines appear to take will be found enough for a busy anæsthetist. After all it is only a matter of personal opinion and for my part I prefer to err on the side of safety, and although my machine can be worked with cylinders of 25, 50, or 100 gallons capacity, I prefer to work with the 100-gallon size. It means a little more weight, but that is easily compensated by the comfortable feeling that one always has enough and to spare of both gases. It is unpleasant to be uncertain whether the supply of one or both gases will be enough to finish a case. The problem of cylinders has been exercising my mind for some time, and I am now in communication with the manager of a firm who makes very light steel cylinders—cylinders which, as Mr. Marshall has said, were used for carrying oxygen for the flying men during the War. I have had hopes that we might be able to use these cylinders, but unfortunately they do not quite conform to the Board of Trade regulations with regard to safety. However,

I intend to follow up the matter and it may be possible to look forward to a time when the portability of cylinders for gas and oxygen will be a very easy matter.

Mr. F. F. BURGHARD: I speak as an unbiased observer who at first regarded the method very critically and only became convinced of its great merit after careful and prolonged trial. My conversion is due to Mr. Boyle, who has shown me in practice how the method can be made perfectly satisfactory for a very large majority of the cases with which the surgeon is called upon to deal, and am inclined to agree with Mr. Marshall that if any anæsthetic method can be called the method of the future it is this. At the same time, I would like to see the term "gas-oxygen-ether" applied to it instead of that of "gas-oxygen," as the latter is not always strictly descriptive of it. This anæsthetic method is largely employed in my cases by Dr. Ernest Playfair and Mr. Cecil Hughes in almost every kind of case, and our experience has been that the most satisfactory surgical anæsthesia can be obtained almost invariably: in a fair proportion of cases it may be necessary to use a small quantity of ether, but this is so trifling as not to rob the method of its valuable advantages. Difficult gall-bladder, abdominal and rectal cases are constantly being done under it with the most satisfying results.

Mr. MARSHALL (in reply): I agree with the President that as the ether attachment is an essential part of the apparatus the latter ought to be called a gas-oxygen-*ether* apparatus. In over 50 per cent. of cases it is advantageous to use some ether in the early stages of the administration—i.e., to complete the induction. It is seldom necessary to continue it for more than two minutes. With regard to preliminary medication, small doses of morphia with atropine give the best results, hyoscine seems to make the administration more difficult. The amount of morphia should not exceed $\frac{1}{6}$ gr. for a woman, or $\frac{1}{4}$ gr. for a man; patients under 17 years of age are better without any. As to vomiting, this is less common after gas-oxygen than after other general anæsthetics, if it does occur it is usually due to one of the following causes: (1) Faulty administration: cyanosis from giving insufficient oxygen; or carbon dioxide poisoning from excessive rebreathing. (2) Faulty preparation of the patient before operation, especially the giving of excessive doses of morphia, or the administration of strong purgatives such as castor oil, a practice which I consider pernicious.

Section of Anæsthetics.

President—Dr. Llewelyn Powell.

Intratracheal Insufflation of Chloroform: A Report on 357 Cases.[1]

By Clarence H. Mott, M.R.C.S., L.R.C.P.

I was asked if I was willing to read a paper on "Intratracheal Insufflation of Chloroform," and though I should have liked to postpone it until I had used the apparatus with its present modifications, I could hardly decline the honour.

The tendency of late years has been in the direction of improving the methods of ether anæsthesia; the drop, and the vapour methods are seldom followed by serious after-effects, and the introduction of intratracheal insufflation of ether has rendered the use of chloroform even less often necessary; and chloroform has largely come to be used merely as an adjunct to ether; as shown by Professor Gwathmey's and Dr. Shipway's three-bottle apparatus, and Dr. Silk's and Dr. McCardie's mitigated ether.

Is there any reason why chloroform should not be allied to intratracheal insufflation? Ether is given with success by either closed or open methods; chloroform, as far as I know, only by the open method—I leave out the Roth-Drager, not having seen it used. The more open the method, the safer one might assume it to be for chloroform. Again, chloroform is said to throw less strain on the heart and lungs. Dr. Peck, of New York, refers to intratracheal insufflation as giving "absolute relief from all strain on the respiratory system, and consequent relief of much strain and harmful effect of the anæsthesia on the cardio-vascular and the central nervous systems." He was, of course, referring to ether.

Chloroform depresses both the circulatory and respiratory systems; is not this an additional reason for employing this method? Chloro-

[1] At a meeting of the Section, held December 5, 1919.

form is given intratracheally by Kühn's tube, and Dr. Dakin Mart described in the *Lancet*, November, 1914, his method of pumping chloroform vapour by means of a Junker's inhaler into the trachea through a catheter, for nasal operations. Meltzer wrote, "even for the use of chloroform the method (i.e., intratracheal insufflation) seems to offer a good deal of safety, but I have succeeded in killing one animal when a great deal of chloroform was used." Elsberg wrote, "careful experimental investigation will have to show whether chloroform can be safely given by intratracheal insufflation." Gwathmey, in the course of his remarks on intratracheal ether said "the dose of chloroform has not been worked out yet." Dr. Ehrenfried, of New York, refers to a case in which a sarcoma of the chest wall was successfully removed under "intermittent intralaryngeal insufflation of chloroform and oxygen" by a modified O'Dwyer's apparatus.

I first saw ether given intratracheally at the British Medical Association's annual meeting at Liverpool, in 1912, by Mr. Kelly, and have used his apparatus for all this work; he is, I know, opposed to chloroform being given in this way, but his apparatus is admirably suited to giving both.

In June, 1917, when giving intratracheal and intrapharyngeal ether, I began adding small quantities of chloroform to the stream. For this I employed a Vernon Harcourt inhaler fitted up on the plenum system, placed between the motor and the Kelly; I used the intrapharyngeal method to familiarize myself with this combined method without the added distractions of catheter, pharyngoscope, battery, &c. As will be seen later I gave a good many intrapharyngeal mixtures, and chloroforms; often it was simply because I did not wish to delay the operation if the surgeon was ready, and he was operating on the head or neck; the passing of the catheter would have entailed some delay.

A very gradual increase of the chloroform was a simple matter with the Vernon Harcourt, and I thus used increasing strengths of chloroform, and decreasing amounts of ether, until I arrived at pure chloroform. I found that to maintain anæsthesia with chloroform alone it was necessary to suspend the Vernon Harcourt bottle in warm water, in fact up to about 110° F. for adults. Fitted up in this way I gave 104 cases of pure chloroform intratracheally, with chloroform in the Vernon Harcourt bottle, and ether always in reserve in the Kelly.

As I evaporated 4 oz. and more of chloroform an hour, I naturally found it an inconvenience to add more chloroform during the operation to the Vernon Harcourt bottle, which held only 2 oz., so I had a copper

bottle made similar to the Vernon Harcourt one to hold 4 oz. Later I obtained a 4 oz. glass bottle from Griffin Bros.

The maintenance of the proper temperature of the chloroform in the Vernon Harcourt bottle proved irksome, so I transferred the chloroform to the Kelly, and ascertained the position of the tap at which 4 oz. of chloroform per hour would evaporate; this proved to be with the tap *at half*, and as I used up more than this amount of chloroform in the case of adults, I knew it was safe. I then proceeded to give mixtures, both intrapharyngeally and intratracheally, with ether in the large 4 oz. glass bottle, also suspended in warm water, attached to the Vernon Harcourt, and 6 oz. of chloroform, which I previously ascertained evaporated at the same rate as 4 oz. in the Kelly.

During this series of cases I substituted a Wolff's bottle holding 8 oz. of ether for the 4 oz. glass bottle in the Vernon Harcourt, and very gradually increased the amount of chloroform in the Kelly to 16 oz.; due regard being paid to this important fact in my manipulations of the tap; to my surprise I found that only quite slight alteration of the tap was required. As before, by working with varying strengths of mixtures of chloroform and ether, I again came to the employment of pure chloroform, both intrapharyngeally and intratracheally, but in all my intratracheal chloroform work ether was in reserve.

The employment of the Vernon Harcourt in this manner entailed a considerable waste of time in setting up, and in dismantling, and the lack of symmetry caused by the tall, slender, top-heavy looking Vernon Harcourt with its outspreading branches at the top on the one hand, and the squat, compact, low, much longer Kelly on the other, was hardly a recommendation to this combined method of giving chloroform and ether, and was an artistic absurdity. I therefore asked Mr. Drew, of Down Bros., to add a second chamber, and how well he has done it you all see to-night. I look back upon my first conglomeration of tubes and connexions with amazement.

Since February of this year I have altogether used this double chambered apparatus in 354 cases, made up as follows:—

Intratracheal chloroform	68	cases
,, mixture	134	,,
,, ether	4	,,
Intrapharyngeal chloroform	2	,,
,, mixture	141	,,
,, ether	5	,,

Therefore of my 357 cases of intratracheal chloroform, 289 were given before I received the new apparatus.

Description of Apparatus.

The motor-board has a rheostat 12 in. in length, and it is possible to put on it two bottles containing gauze, to intercept any grease from the pump.

A Cheauveau valve has been fitted at the beginning of the apparatus. I got used to seeing this valve when using the Vernon Harcourt in circuit, and at least it gave me some idea of the volume of air (the flow-meter shows that accurately now), and also that the air was passing at that point. There are several connexions, glass, metal and rubber, as well as rubber corks, where leakage can occur, also the belt can come off the fly-wheel of the pump, and this valve shows at once if the trouble is on the proximal side; it is especially useful because the motor is rather out of sight, being on the lower deck of the anæsthetic table; this valve was too light, but Down Bros. have weighted it. Below this valve is seen a projecting tube for attachment of a length of rubber tubing, to be held in the hand, and so a much more convenient and unobtrusive method of reducing the pressure to zero is afforded, also less anæsthetic is blown into the air, a factor of importance to all present, besides being a slight economy. If not required a cork can be fitted in the rubber tube.

Flow-meter.—I have been in correspondence for seven months (not with Down Bros.) trying to get this flow-meter, for with it the tap can supply any desired percentage of chloroform at any rate of flow. The dial as marked on a piece of vulcanite, shows percentages for a delivery of 10, 20, 30, and 40 litres of air per minute in increments of 0·5 per cent. of chloroform vapour; the flow-meter is placed on the far side of the chloroform tap, so that supposing the rheostat is set to deliver 40 litres, that volume will be recorded on the flow-meter with the chloroform tap closed. If the chloroform tap is opened so that only 30 litres of air are recorded on the flow-meter, that means that 10 litres are passing over the chloroform.

I am greatly indebted to Dr. Waller for teaching me his gravimetric system, and for allowing me the use of his laboratory and assistant for two whole days. When I knew I was to read this paper, I wrote to Dr. Waller appealing for his help, not only for myself but also for the benefit of this Section. While there Mr. Drew and I obtained valuable data regarding the evaporation of chloroform with different amounts in the chamber with a delivery of 10·6 litres of air per minute. By a mistake I had not taken my motor with me, and

was obliged to rely on Dr. Waller's which, as recorded on my flow-meter, delivered 10·6 litres of air per minute.

I propose now to throw on the screen a few charts.

TABLE I.

Six ounces of chloroform in chamber. Motor delivering (?) 25 litres of air per minute.

	Tap at half		Tap at full	
First quarter of an hour ...	8 dr.	= 2·2 per cent.	... 12½ dr.	= 3·6 per cent.
Second ,, ,, ,, ...	9 ,,	= 2·4 ,,	... 11 ,,	= 3·0 ,,
Third ,, ,, ,, ...	8 ,,	= 2·2 ,,	... 10½ ,,	= 2·9 ,,
Fourth ,, ,, ,, ...	9 ,,	= 2·4 ,,	... 10½ ,,	= 2·9 ,,
Fifth ,, ,, ,, ...	7 ,,	= 1·9 ,,		

The first one relates to the experiment I have mentioned when I desired to transfer the chloroform from the Vernon Harcourt to the Kelly. You will see that with a roughly estimated delivery of 25 litres (I had not my flow-meter then and had to employ a revolution counter to arrive at this figure) and 6 oz. of chloroform in the Kelly, about 1 oz. was evaporated for every quarter of an hour with the *tap at half*. Any discrepancy must be due to fluctuations in delivery (owing to more or less grease reaching the motor or pump), or to a little cooling of the chloroform, or to faulty measuring, or a combination of all three. The percentages are not to be taken as strictly accurate. The delivery with the *tap at full* is also shown.

The next table can be taken, I think, as absolutely correct, because these data were all obtained at Dr. Waller's laboratory:—

TABLE II.

10·6 *litres of air per minute.*

Tap	6 oz.	No water 12 oz.	Water ? 10 oz.	Water 16 oz.	Water 20 oz.
6	—	0·05	—	0·13	0·27
6½	—	—	—	0·14	0·32
7	0·15	0·2	—	0·3	0·45
7½	—	0·3	—	0·45	0·6
8	0·28	0·55	0·66	0·92	1·18
8½	—	1·1	0·95	1·4	1·55
9	1·05	1·45	1·45	1·84	2·05
9½	—	1·8	1·8	2·27	2·4
10	1·7	2·2	2·25	2·25	2·73
10½	—	2·5	2·5	2·85	3·0
11	2·17	2·68	2·8	3·08	3·12
11½	—	2·8	2·9	3·23	3·3
12	2·4	3·0	3·1	3·4	3·42
12½	—	3·19	3·3	3·52	3·5
13	2·6	3·2	3·35	3·7	3·7
13½	—	3·27	3·5	3·83	3·82
14	2·33	2·3	3·5	3·83	3·84
	Several hours' duration; few drachms left	1¾ hours; 3½ oz. used, 8½ oz. left		1 hour; 2 oz. used, 14 oz. left	1½ hours; 2·6 oz. used, 17·4 oz. left

We had to use a delivery of 10·6 litres of air per minute, and we successively placed in the chamber 6, 12, 16 and 20 oz. of chloroform, and went straight through any one series without the addition of more chloroform. The temperature of the laboratory was 70° F. The first column shows different positions of the tap; the second shows the rise in percentage as the tap is opened. These calculations took some hours; of course the motor would not be running all that time, it was frequently stopped for several reasons. At the end of this test only 1 dr. or 2 dr. of chloroform were left in the chamber, as ascertained by measuring the next morning; the temperature of the room was 70° F.; perhaps not much chloroform would have evaporated during the night from a more or less closed chamber. The drop in the percentage at the 14 mark is probably due to the small amount of chloroform left. The third column shows the same test with 12 oz., but in this case we took readings of the percentages when the tap was between the marks as well. The last reading is obviously a mistake. There was, by error, water only up to the base of the chloroform chamber. The fourth column shows readings when the water entirely surrounded the chloroform chamber. The difference is but slight and only when the tap reaches the 11 mark and beyond. The estimation of the percentages shown in these two columns took an hour and three-quarters and used up 3½ oz. of chloroform, leaving 8½ oz. of chloroform at the end. We did not fill up with chloroform so probably began the tests for the fourth column with about 10 oz. of chloroform instead of 12 oz., yet the percentages are almost identical. The last column but one refers to 16 oz.; there is obviously an error when the tap was at 10. This test lasted one hour, and used up 2 oz. of chloroform, leaving 14 oz. at the finish. The last column refers to 20 oz. of chloroform; this test lasted one hour twenty minutes, used up 2·6 oz. of chloroform, leaving 17·4 oz. in the chamber at the end. There is less rise in percentage when passing from 16 oz. to 20 oz. than from 12 oz. to 16 oz. Chart I shows these results in dial form (*see* next page).

To come back to the apparatus: two other tubes which project backwards are for connexion to Waller's balance for experimental purposes. I place a small measure glass full of water in the moistening chamber, and it lasts some days; I think it supplies sufficient moisture. A wash bottle was added for attachment of rubber tube from an oxygen cylinder: this has now been replaced by Dr. Marshall's modification of Dr. Boyle's sight feed for gas and oxygen. The mercury safety valve

has been brought to the front. The next point I mention with a good deal of reserve; a second manometer has been added in which I place coloured water. This can be connected by a piece of rubber tubing to a catheter placed in the œsophagus, and indication thus afforded of the respiratory movements. I hope this may prove of value in head and neck cases in which sterile towels so often impede a full view of the patient's face. I suppose we have all, as Dr. Silk so caustically described, passed the catheter into the œsophagus. I have more than once blown up the stomach, the air even entering the duodenum, as shown when the abdomen was open, and the air in my cases did not regurgitate as fast as it entered the stomach.

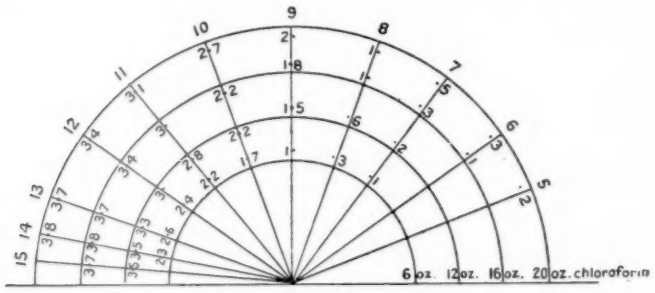


CHART I.

Scheme for motor delivering 10½ litres per minute.

Dr. Gwathmey points out this danger of blowing up the stomach, but suggests that the air would probably regurgitate as fast as it entered. I have on not a few occasions heard, of course very faintly, the air coming out of the mouth of a catheter placed in the œsophagus, and a fluff of cotton wool has proved that air is entering and leaving—i.e., that there is an air current down the œsophagus when the latter is patent from the presence of a catheter, especially a Coudé; I have, therefore, on several occasions, whether using insufflation or face-mask methods, passed a Coudé catheter into the œsophagus, and observed the reading on the water-manometer. An ingenious catchpit that Mr. Drew has added obviates any danger of inhaling the water, should the catheter be in the trachea. The following happened to me once: My second catheter followed the first into the trachea, instead of, as intended, going into the œsophagus, so I passed a third into the œsophagus, connecting this last one to the water-manometer, and using either of the intratracheal catheters for the insufflation. The œsophageal catheter,

perhaps, even facilitates the return of air through the pharynx. A rather fine metal tube with tap pierces the stopper of each chamber, reaching to the bottom; to empty the chamber, open this small tap, run the motor fast, clamp delivery tube, and the anæsthetic can then be collected into a bottle; this saves dismantling apparatus to empty the chambers, a tedious process, and at times the cause of breaking a glass vessel; I particularly have to thank Mr. Drew for this idea.

The Anæsthesia.

The patient usually has morphia and atropine, and I generally induce with C_1E_2, often finishing up with open ether. The taps are set as desired, generally at mixture, the motor started; nasal tubes attached to Connell's adapter are then passed into the pharynx, connexion being made with delivery tube, and intrapharyngeal anæsthesia is instituted; the operation can then begin. If not going on with intrapharyngeal insufflation I can pass the catheter deliberately and without haste, as there is no fear of the patient coming round, and the anæsthesia need not be unduly deep. The one drawback is, that it is necessary to get accustomed to the blast of strong anæsthetic vapour, which sometimes makes the eyes water; but this is a small drawback compared with the ease of passing the catheter in this manner. If intending to confine myself to chloroform, the ether is turned off when the catheter is in place.

As in intratracheal ether the depth of anæsthesia is controlled by movement of the taps, and *ether is in reserve all the time.* Should any untoward symptom appear, some error in technique has been allowed to intervene; increase or decrease of the chloroform, more frequent, or longer reductions of pressure to zero, increase or decrease in volume of air, attention to depth of glass tube in mercury safety valve, or alteration of position of catheter in trachea will generally effect the required improvement; if still not perfectly satisfactory the addition of ether, for perhaps only a short period, or even oxygen, may enable the pure chloroform to be resumed. But so seldom do any difficulties occur, that I have convinced myself of the safety of this method.

I now come to a consideration of the operations performed under intratracheal chloroform.

Amongst the 357 cases were the following: 135 intraperitoneal (11 gastro-enterostomy, 10 gall-bladder, 10 hysterectomy), 15 limbs, 12 hæmorrhoids, 10 kidney, 10 breast amputations, 7 glands of neck,

7 thyroid, 3 bladder, 3 mouth. There are only three operations inside the mouth; here for various reasons ether is preferable.

My best thanks are due to the honorary surgical staff of the North Staffordshire Infirmary, not only for allowing me to use this method but also for permission to publish statistics of their cases; they have also given me their very cordial co-operation at all times.

This method has been used in the service of seven surgeons, and I am pleased to say that four of them are here to-night; they will probably be listened to with greater attention than myself in any discussion that I hope this paper may evoke; I not only expect criticism, but look forward to its being of the greatest assistance in carrying on this work.

It was more immediately my duty to compile statistics of the after effects, and the following table gives them from a respiratory point of view:—

TABLE III.

After results; Respiratory.

In 307 cases	No cough.
In 28 ,, ...	Very slight.
In 12 ,, ...	Decided cough.
In 4 ,, ...	Somewhat serious.
In 6 ,, ...	Quite serious.
In 1 case	Pneumonia developed.

Details of the four "somewhat serious cases."

Case	Age	Length of anæsthesia
Prostatectomy	68	35 minutes
Gall-stones	49	50 ,,
Hysterectomy	42	30 ,,
Hernia	36	20 ,,

Details of the six "quite serious cases."

Case	Age	Length of anæsthesia
Gastro-enterostomy	34 ...	1 hour, 20 minutes
Carcinoma mammæ	56 ...	1 hour, 40 minutes
Gall-stones	49 ...	50 minutes
Ovarian tumour	45 ...	45 minutes
,, ,, ...	45 ...	1 hour
,, ,, ...	58 ...	1 hour, 40 minutes

Hysterectomy, age 49. Operation, 1½ hours. Had a cough before operation, developed pneumonia, and died six days later.

I must apologize for the lack of definite clinical diagnosis, as to whether bronchitis, or broncho-pneumonia were present; but this work was done during the absence of my fellow anæsthetist, whose work I was

doing as well, and the information was derived from the sister or staff nurse, without troubling my already overworked colleagues. In only one of the four "somewhat serious" cases did the operation last more than three-quarters of an hour, they were all abdominal cases. In the six "quite serious" cases, none were over 60 years of age, but with one exception they were intraperitoneal operations, and four lasted an hour and over. One death occurred from pneumonia six days after hysterectomy; she had a cough before operation.

Vomiting.—Of the 357 cases, 114 did not vomit, 129 vomited one to three times, 60 vomited four to six times, 23 vomited during two days, and 5 vomited during three days. I do not mean the vomiting was continuous for two or three days. The following table shows the average number of vomits per case for 123 major operations :—

TABLE IV.

Cases	Number	Number of vomits per case	
Gall-bladder ...	10	5·9	One case vomited 30 times; one case vomited 12 times
Gastro-enterostomy	11	5·5	One case vomited 27 times, ? tabetic
Ovarian tumour ...	10	5·2	One case vomited 15 times; two cases vomited 10 times
Breast amputation...	9	4·7	
Kidney	12	4·6	One case vomited 10 times
Hysterectomy ...	11	3·6	
Hysteropexy ...	10	3·6	One case vomited 16 times
Appendix	40	3·3	Four cases vomited 18, 17, 14, 10 times respectively
Colporrhaphy and perineorrhaphy	13	2·6	
Prostatectomy ...	2	0·5	
Bladder	4	—	No vomits

The personal idiosyncrasy to chloroform is perhaps shown by the great number of times that twelve of these vomited: patients with operations on the upper part of the abdomen vomited most. I was rather surprised to find how high up in the list breast amputations came. Perhaps one day, this personal idiosyncrasy to chloroform may be determined before operation. In many cases the Trendelenburg position was used. Children appear to take this method as well as adults, and the table of after-results shows particulars (*see* next page).

Of the 357 cases, eight died; of these Table VI gives particulars. The second case on the list suddenly developed serious symptoms on the fourth day after operation, whilst having his bed jacket changed, and died within a quarter of an hour. At the post mortem, which was not made by a pathologist, dense pleural adhesions

were found on both sides, and ecchymoses on the omentum; death was attributed to pulmonary embolism. The fifth case, also tubercular, died of syncope thirty-six hours later, as also the last case, a very fat woman, three days after an operation for hysterectomy.

TABLE V.

After-results of Cases of 12 Years of Age and under.

Cases	Ages	Coughs	Number of vomits
Hernia	12	1	—
,, ...	12	—	—
Ventral hernia	10	—	13
Appendix	11	—	1
,, ...	11	—	4
,, ...	9	—	3
,, ...	11	—	1
Tuberculous glands of neck ...	12	—	5
,, ,, of axilla ...	10	—	3
Sarcoma of kidney	12	—	2
Tuberculous hip	12	—	2
Osteo-myelitis, chronic	12	—	12
Fracture dislocation of elbow ...	9	—	—
Orthopædic	12	—	2
,, ...	9	—	—
,, ...	5	—	—
,, ...	8	—	—
,, ...	11	—	—
,, ...	8	—	4

The case of tuberculous glands of axilla developed symptoms of pulmonary embolism, but made a complete recovery.

TABLE VI.

Case	Age	Length of operation	Cough	Number of vomits	Cause of death
Perforated duodenum	33	30 minutes	—	2	Peritonitis
Tuberculous glands of neck	12	1¾ hours	—	5	Pulmonary embolism four days later
Colostomy	45	45 minutes	—	—	Intestinal obstruction five days later
Carcinoma of cervix ...	31	15 minutes	—	3	Hæmorrhage fourteen days later
Laparotomy, tuberculous cæcum	19	45 minutes	—	7	Syncope thirty-six hours later
Hysterectomy ...	49	1½ hours	Cough before operation	2	Pneumonia six days later
Carcinoma mammæ ...	56	1 hour 40 min.	—	3	Hæmorrhage six days later
Hysterectomy ...	54	1 hour	—	6	Syncope three days later

A comparison must be made with other methods as regards after-effects.

TABLE VII.

Anæsthetic	Number of cases	No respiratory complications Number	Percentage
Intratracheal chloroform ...	357	307	85
Intrapharyngeal ,, ...	75	72	96
Vernon Harcourt	126	104	82
Intratracheal mixture	193	174	90
Intrapharyngeal mixture ...	76	69	90
Intratracheal ether	135	124	91

This shows that from a respiratory point of view intrapharyngeal chloroform gave the best results, followed by intratracheal ether, intratracheal mixture, and intrapharyngeal mixture; then comes intratracheal chloroform and last the Vernon Harcourt. In this connexion I will only make two remarks; the Vernon Harcourt must throw more work on the respiratory system, and, if the patient had a cough before operation, I selected intratracheal chloroform. In 291 cases when Dr. Shipway's three-bottle apparatus was used in 95 per cent. of them no respiratory complications developed.

TABLE VIII.

Anæsthetic	Number of cases	No vomiting Number	Percentage
Intratracheal chloroform ...	357	114	31
Intrapharyngeal ,, ...	75	26	34
Vernon Harcourt	126	48	38
Intratracheal mixture	193	54	27
Intrapharyngeal mixture ...	76	28	36
Intratracheal ether	135	67	49

Intratracheal ether gave the best results as regards vomiting; intratracheal chloroform coming last but one on the list.

TABLE IX.

Operations lasting Three-quarters of an Hour and Over.

Anæsthetics	Number of cases	Hours of anæsthesia	Number of vomits per case
Intratracheal chloroform ...	123	126	4·5
Vernon Harcourt ...	45	45	3·1
Intrapharyngeal chloroform ...	16	15	4·9
Intratracheal mixture	79	80	3·3
	263	267	3·9

The above table shows the number of vomits per case of operations lasting three-quarters of an hour and over, for four different methods. This is probably a more reliable estimate of the after results as regards

vomiting. The Vernon Harcourt comes out best here, probably because I used it less often in abdominal cases, which require a deep anæsthesia.

Of the 123 cases of intratracheal chloroform lasting three-quarters of an hour and over, 95, that is, 77 per cent., did not develop a cough. Again I must remark that chloroform was particularly used if the patient had a cough before operation.

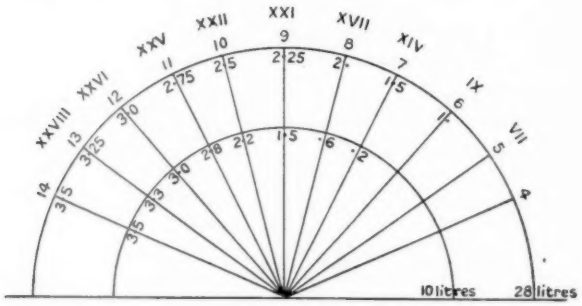


CHART II.

Scheme to compare 10 and 28 litres of air per minute with approximately the same amount (i.e. 12 oz.) in chloroform chamber. The Roman figures show actual numbers of litres of air per minute passing into chloroform chamber at various positions of tap when 28 litres is being used.

On the above chart I have shown on the same dial two sets of figures, one showing the effect of passing 10 litres, and the other 28 litres of air per minute to the apparatus, and you will note when nearly the whole of the air stream is deflected by movement of the tap into the chloroform chamber, the percentages are almost identical whether one is using 10 litres of air, or nearly three times that amount. The greater volume of air of course evaporates more chloroform, but it only appears to supply much the same percentage.

TABLE X.

	5	6	7	8	9	10	11	12	13	14	15	Tap
												Temperature
40 litres ...	0·1	0·6	0·9	1·4	2·1	2·5	2·7	3·0	3·2	3·3	3·4	? 65° F.
30 ,, ...	—	0·6	0·9	1·5	2·1	2·4	2·8	3·2	3·1	3·1	3·0	? 63° F.
20 ,, ...	0·5	0·7	0·9	1·3	1·9	2·4	2·5	2·8	3·0	3·1	3·1	? 61° F.
10 ,, ...	0·5	0·6	0·7	1·0	1·8	2·2	2·8	2·8	3·1	3·1	3·5	61° F
10·6 ,, ...	—	—	—	0·9	1·8	2·2	3·0	3·4	3·7	3·8	3·7	70° F.

In these experiments we placed 16 oz. of chloroform in the chamber, ran the motor at four different speeds—i.e., 40, 30, 20 and 10 litres of air per minute, and estimated the percentages delivered when the tap was gradually opened through eleven different positions; at the beginning of each experiment we filled up the chloroform to 16 oz. When using the 40 litres the temperature of the laboratory was about 65° F. and the maximum percentage obtained was 3·4; when using 30 and 20 litres the temperature of the laboratory dropped a few degrees, and the maximum percentage reached was only 3·1. There are two readings each with a delivery of 10 litres of air per minute; in the one case the temperature of the laboratory was 61° F., and in the other 70° F., the latter naturally gives the higher percentages.

DISCUSSION.

Mr. REGINALD ALCOCK (Hanley): As many as 115 cases recorded are those of patients operated upon by me. It will be observed from the charts that not all the cases belonged to general surgery but that many of them were gynæcological operations. There are certain conditions which a surgeon desires of his anæsthetist, one of the most important of which is that of complete relaxation; this is not found to obtain so uniformly with any anæsthetic as with intratracheal chloroform. In such cases as gastro-jejunostomy, it is especially valuable since the excursions of the abdomen are practically non-existent. Another condition required of the anæsthetist is that the anæsthetic should not increase the hæmorrhagic oozing, which is often so marked in operations about the head and neck, when ether is given, but with intratracheal chloroform there is no increase in hæmorrhage, nor the slightest evidence of congestion of the parts operated upon. It also adds very much to the comfort of the operation if the anæsthetist and his apparatus do not encroach upon the field of operation, and this is fully met by Mr. Mott, as the whole face and neck can be completely covered by a towel. It is only right that reference should be made to the deaths which have been recorded upon the charts, because they all took place many hours after the operation and are in no way connected with the anæsthetic administered. Mr. Mott says in his address that his thanks are due to the surgeons for their permission to use his method in their cases, but in my opinion it is the surgeons who have reason to be thankful to Mr. Mott for his introduction of an absolutely reliable and safe method of giving intratracheal chloroform.

Mr. W. ALLARDICE (Newcastle-under-Lyme): I have operated on close upon 100 cases under this anæsthetic; the list includes emergencies such as ruptured duodenal ulcer and set operations such as radical cure of hernia.

The youngest patient was aged 9 and there are several over 65 years of age, while the time occupied has varied from twenty minutes to well over an hour in different cases. A surgeon is entitled to ask that there should be no interference from the anæsthetist with his apparatus in operations on the head and neck, that there should be full relaxation when operating on the abdomen, and that in all cases the anæsthetic is safe. This method has fulfilled those points. The choice of the anæsthetic and method of its administration should be left to the anæsthetist as he is primarily responsible and has to bear the chief brunt of any inquiry. Under Mr. Mott's present method there has been no fatality so far, nor has there been any cause for anxiety during its administration. Artificial respiration is, however, provided for in a unique manner, as by switching off the chloroform the lungs are freely ventilated by a current of pure air pumped into them, which is an enormous safeguard to the patient. No time is wasted in introducing the gag or pulling forward the tongue, and the difficulty of trying to compress the rigid chest wall of an emphysematous patient does not arise. This is a method for experts only as it not only involves preliminary anæsthesia, but in addition the passage of a sterile catheter into the trachea. The possibility of introducing germs into the trachea is obvious, but the series of cases is free from any record of pneumonia. To one trained in the Scotch method of chloroform given on a towel with the constant close supervision of the patient, this method exhibits a striking contrast. There is the hum of the electric motor and the hiss of the returning air and in abdominal cases the cessation of all movement.

Mr. F. ROOD quoted the Registrar-General's statistics to show the far greater danger of chloroform as compared with ether.

Dr. SILK pointed out that deaths from anæsthetics have very frequently been attributed to other causes.

Mr. CHARLES T. W. HIRSCH: Mr. Mott's contribution is a tribute to the value of chloroform as an anæsthetic. I admit that in septic cases its administration is not desirable on account of its possible after-effects, but the testimony of the two surgeons, from Staffordshire, who have come here to-night, show its appreciation from their point of view, and Mr. Mott's statistics its safety when given by a dosimetric method. Mr. Rood has quoted from the Registrar-General, and there is no question that there are more fatalities from chloroform than ether, but I submit that this can, anyway to a certain extent, be accounted for by the fact that many anæsthetics are given by doctors who are not professional anæsthetists and are not given by a dosimetric method. My own experience, is that when it is so administered in suitable cases it is absolutely safe, a fact which is confirmed by the paper we have heard with so much pleasure this evening.

Mr. NORMAN PATTERSON: Mr. Mott has done an enormous amount of work in connexion with the intratracheal administration of chloroform. I can

see one very definite direction in which this method will prove useful and that is in the treatment of malignant disease of the mouth and pharynx by diathermy; ether cannot be used on account of its inflammatory nature.

Dr. BLOMFIELD: Mr. Mott's work seems to be of the greatest scientific value. He has practically carried out on the human subject experiments analogous to those made upon animals in the physiological laboratory in order to arrive at safe percentage strength of chloroform vapour. Mr. Mott's results are in strong confirmation of the physiologists' teachings as to what that percentage should be. In order to advocate the preference of chloroform over ether insufflation there should be shown, I think, definite advantages in the former such as Mr. Patterson has shown to exist in the case of employment of diathermy. Generally speaking Mr. Mott's cases must not lead us to any alteration in opinion as to the well-established superior safety of ether over chloroform.

Dr. F. E. SHIPWAY: Mr. Mott prefers intratracheal chloroform to intratracheal ether, because he obtains better relaxation and slighter respiratory movements. There are two comments to be made upon this. Firstly, chloroform is not such a safe anæsthetic as ether, and is being gradually abandoned for this reason. Secondly, Mr. Mott's figures show that intratracheal ether gives better after-results. He can obtain adequate relaxation with ether, but I would go a point further to say that it is absolutely unscientific to try to obtain relaxation by pushing the anæsthetic. It is not the duty of the anæsthetist to relax the abdominal muscles; this should be done by the surgeon using an injection of novocain. The severity of after-results is in direct proportion to the depth of anæsthesia. Why does Mr. Mott prefer intratracheal ether for mouth cases? And does he find that the combination of morphine and insufflation of chloroform gives him any anxiety? I found in a few cases that the pulse became suddenly irregular, even when anæsthesia was fairly light and the condition of the patient good.

Mr. H. C. NANCE (Norwich): As a Staffordshire man, attending the meeting as a casual visitor, I am very pleased to think that this work of Mr. Mott's has emanated from the North Stafford Infirmary, where I attended the the operations during my early student days. Speaking as a strong advocate of ether I may mention a fatal case from chloroform. It was the first I had seen and it occurred at the North Staffordshire Infirmary—that of a boy upon whom lateral lithotomy was being performed. The chloroform was administered by the house physician. Mr. W. H. Folker, the senior surgeon, had just extracted the stone, and noticed that the child had suddenly stopped breathing. Long continued attempts at resuscitation were tried. Mr. Folker felt this fatality very keenly, and said that he would not allow chloroform to be given again, and I believe ether was used for a long time afterwards.[1]

[1] I published a letter in the *Lancet* for 1890, ii, p. 44, pointing out the safety of "Ether over Chloroform," as shown by the fact that in three years the deaths from anæsthetics in the United Kingdom were forty-one from chloroform and only three from ether.

Mr. C. MOTT (in reply): I have advocated to-night the association of intratracheal insufflation with chloroform, as the safest method of giving chloroform, ether always being in reserve. I did not feel particularly called upon at the moment to indicate for what special occasions intratracheal chloroform would be useful; the work of others more competent than myself might prove that; I was chiefly concerned with recording my experience of the method. In operations within the mouth in which bleeding occurs I prefer ether, because one has not undivided control in such cases, of the patient's air-way. In my opinion intratracheal chloroform is far safer than intrapharyngeal chloroform, which Dr. Knock has given thousands of times since 1910.

Joseph Clover: An Appreciation.

By DUDLEY W. BUXTON, M.D.

As in the Bidding Prayer we are taught to love our benefactors in remembrance, so I think on occasion, we anæsthetists may well recall the work of our benefactors, the pioneers and makers of anæsthesia.

What John Snow was to the science of anæsthesia, Clover was to its art. It is not easy for us to-day to envisage anæsthetics as Snow and Clover knew them. We may and do recognize the clear cut results of research, the conclusions arrived at after years of patient experiment, and we are justly proud of the great strides and the degree of precision arrived at in our methods of practical anæsthesia. For Clover the outlook was otherwise; indeed, in no small measure do we owe to him the advances in methods alone rendered possible by his spade-work of over forty years ago. As a student Clover, together with Lister, saw the first demonstration of the use of ether in hospital practice in England. Think of the method—a glass bottle containing morsels of sponge soaked with ether, the vapour being conveyed to the patient through a length of tubing terminating in a crude mouthpiece. Clover, whose delicate health precluded him from a surgical career, adopted anæsthesia as his main line of practice. It is not too much to say when he commenced to study the subject it was crude to the degree of chaos, and when "finis" was written over his work, order had emerged out of chaos and the germ of progress had grown into a healthy sapling.

The appointment of the Anæsthetics Committee by the then Royal Medical and Chirurgical Society gave Clover his chance. Although he

was only named as an assessor while physicians and surgeons sat upon the Committee, yet Clover's acumen with his technical knowledge made the Report at once notable and valuable. To us of to-day it seems almost incredible that this Report decided that although chloroform was dangerous, as indeed it had been shown to be, yet ether, although admitted to be safe, was by itself an impracticable anæsthetic *because it was too slow in its action.* The terms of the problem were thus stated and Clover at once set about removing this drawback and designed his "small regulating ether inhaler." Thus did Clover teach the world how ether should be given. One of the factors making for his success was the meticulous care he took alike in the design and the making of the various anæsthetic appliances which stand in his name. A skilled limner and painstaking mechanic, he brought to bear upon the smallest detail his personal supervision and inventive genius.

Perhaps the greatest advance he made was the introduction of the nitrous oxide-and-ether method. Remember at this time the former had hardly become acclimatized and the latter had comparatively few friends. Clover designed the large gas and ether inhaler, which although it has been modified, yet remains in principle the best apparatus. It was more elaborate than the smaller inhaler, and a perusal of Clover's letters indicate the great pains he took in its production as well as the heart searching it cost him. One point which has perhaps been overlooked is the immense amount of work Clover performed to arrive at the facepiece as we know it. We no longer put a mouth-cap on our patient nor do we pinch his nose with a clip; this departure from a barbarous practice we owe to Clover's facepiece. The question of the bore of the tube fitting the facepiece in the two apparatus was carefully investigated by Clover. All air-ways were made as large as the average trachea, for as he recognized the patient does not aspirate from the tube but from the dead space of the facepiece, hence Clover's insistence upon using an oro-nasal facepiece of adequate size.

To pursue his work with regard to nitrous oxide or chloroform would occupy too much time. Suffice it to say that little has been altered, if we exclude the oxygen-nitrous oxide method, since Clover's day; he even suggested and employed a nasal method, inserting little rubber nipples into the nostrils. In the matter of chloroform he followed the teaching of that astute thinker John Snow, and even if his cumbersome chloroform bag has fallen into desuetude, its invention marked an advance in our art.

Clover's excursions into the field of untried anæsthetics are less

generally known and perhaps less notable. He was so convinced of the general applicability of his gas and ether method that dichloride of ethidene and other agents held but a subsidiary place in his mind. Clover's published writings were comparatively few, his mind being practical rather than literary, but the articles which he contributed to the journals and the papers published in "Quain's Dictionary" were models of conciseness and perspicacity. To appreciate what Clover did for anæsthesia we must compare that science as it was in 1864, when the Royal Medical and Chirurgical Society's Report was issued, with what it had become in the last quarter of the nineteenth century, and we must study his personal letters and sketches. This done we may well be amazed at the revolution his master mind created, nor shall we grudge an appreciation of his toil, his perseverance and his genius.

Apparatus and Documents belonging to the late Mr. Clover were exhibited at the Meeting.

Section of Anæsthetics.

President—Dr. Llewelyn Powell.

DISCUSSION ON ANÆSTHESIA IN OPERATIONS ON THE THYROID GLAND.[1]

(Dr. Harold Low, Vice-President, in the Chair.)

Dr. May Dickinson Berry.

In opening the discussion on this subject I propose to confine myself chiefly to my own experiences and to describe in detail the procedure of light anæsthesia I always use in operations for goitre. I beg the audience to excuse me if I dwell on points which to many, if not to all, seem elementary and well known, because, from remarks which I have heard made both here and elsewhere, I believe that the phenomena and possibilities of very light anæsthesia are often little recognized, even by some of those versed in the administration of anæsthetics.

The Anæsthetic.—My remarks are based upon a total of nearly 700 cases of operation for the removal of goitre. In the first sixty cases $CHCl_3$ was the only anæsthetic used; in the next 260, dating from 1908 to 1912, induction was carried out with $CHCl_3$, or in a few cases C.E., ether by the open method being mainly used for the rest of the administration. During this period I had one death on the table; it occurred in 1909 and has already been published. The patient, a healthy looking young woman, succumbed suddenly to heart failure one minute after the first incision; $CHCl_3$ had been changed to ether before the operation began, and there had been no previous trouble. Persistent

[1] At a meeting of the Section, held April 9, 1920.

thymus and enlarged spleen were found post mortem and the case was reported as one of status lymphaticus. I also find notes of a few instances of pallor and faintness occurring at the end of induction or early in the operation, all in cases in which $CHCl_3$ or ether preceded by $CHCl_3$ had been used. Since1 912—after a visit to the Mayo Clinic—I have used nothing but open ether for goitre operations, and it has given me a sense of security and of control of the patient I did not feel before. Of these I have had about 360 cases.

Method and Nature of the Anæsthesia.—In operations on the thyroid, especially in bad cases, I always use a light anæsthesia, my aim being to maintain an anæsthesia *as light as is compatible with the requirements of the operator.* As a preliminary I like to have $\frac{1}{100}$ gr. atropine injected one hour before the operation in order to diminish the likelihood of any trouble from mucus. Morphia I do not like and am not in the habit of using. For a facepiece I use indifferently an ordinary Skinner's mask, or the somewhat larger circular frame employed in the Mayo Clinic, covered, in both cases, with either a single layer of lint and one of gauze, or a double layer of stockingette. The method of dripping ether by means of a strip of gauze laid in a groove in the cork of the ether bottle I much prefer to the rubber cork perforated by two tubes, which is often employed, as giving more latitude in varying the flow from isolated drops to a steady stream. I believe in slow induction, as being pleasanter to the patient and more likely to lead to a smooth anæsthesia with no trouble from mucus. I think it is especially desirable that the patient should not be in any way disturbed during the first five minutes, as, for instance, by arranging the towels, &c., but I talk to him and encourage him to talk as long as he can do so. I find induction takes from six to ten minutes as a rule. I allow the operator to begin as soon as deep regular breathing is well established and do not necessarily wait till the conjunctival reflex is abolished. But though there may be, and generally are, no movements or other reflex signs at the first incision, I generally deepen the anæsthesia somewhat while the deeper structures are being divided, for I do not at all find, as is so often stated, that it is the skin which needs the deeper anæsthesia. When the gland is reached I withhold the ether, if the patient is absolutely quiet, until alteration in the breathing or other signs tell me I have reached the furthest limit of satisfactory anæsthesia. Administering a very light anæsthesia resembles, in my opinion, riding a bicycle along the edge of a narrow path with a drop beside it. This path, to continue the simile,

varies in width in different patients, on one side it merges into the region of "surgical anæsthesia," on the other is a sharp descent to normal consciousness. This constitutes the area of light anæsthesia and this area varies in width in different patients. As long as the patient is upon it he is under complete control, a few drops of ether readily bringing him to any desired level; the moment he gets over the "edge" the ether must be given freely and rapidly to get him up again. And to know just when he is at the edge and when he is over are crucial points in successful administration. In the maintenance of this kind of anæsthesia the customary guides are often of little value. The eye reflexes, for instance, vary greatly, and, to me at least, are of little practical use. Often an active lid reflex is present while the patient remains flaccid and motionless, and on the other hand, it may be nearly or entirely absent when one knows the patient is very lightly under. The first signs of what one may call "coming round" vary greatly in different patients, and upon the character of these depends how wide the available "bicycle path"—to return to my first simile—will be. If they are of a nature which does not interfere in any way with the operation I disregard them and wait for others before I feel that the "edge" which must not be passed has been reached. Sometimes the eyes may open spontaneously or talking may occur, the patient remaining quite motionless. These I allow to continue, but coughing, swallowing, the beginning of general movements have to be restrained. The breathing is undoubtedly the main practical guide as a general rule. When one has ascertained the available limit of the anæsthesia one continues administration by using the smallest amount of anæsthetic possible to keep the breathing regular. The *depth* of the respirations I find very variable. It is quite common to get deep respirations, suggestive of deep ether anæsthesia, when the patient is really very lightly under, and this is quite irrespective of whether there is dyspnœa or no.

Stages of the Operation.—Certain stages in the operation require special notice from the anæsthetist's point of view. The *dislocation of the tumour* is a dangerous process in cases of severe dyspnœa, and many instances of death at this period have been recorded in the literature. It is important that the anæsthesia should be specially light at this time. In bad cases I withhold the anæsthetic altogether until the dislocation is completed, even though this means that the patients get over the "edge" and down the hill on the other side. It is always easy to re-establish anæsthesia by giving the ether freely when the dangerous time has passed.

While the goitre is being dissected out there is often a good deal of pulling on the trachea, which may interfere considerably with an already narrowed airway. Since this, however, is a condition which can be remedied at will by the surgeon, it merely calls for care and watchfulness on the part of the anæsthetist, who must prevent the deprivation of air being carried too far, by demanding free breaths at necessary intervals. After the goitre has been removed, the vessels, which have hitherto been secured by forceps, are tied. During this process it will be found that a lower level of anæsthesia can be used to maintain the same quiescent condition. I withhold the anæsthetic till the limit is again reached, and keep the patient just inside this until about three ligatures remain to be tied; then I remove the facepiece, insert a gag, and the patient is generally ready to strain as soon as the last ligature has been secured. This straining, which is held to be very important in preventing any possible after-hæmorrhage, is often a prolonged proceeding. The patient often appears to be nearly round during this process, but I have never found that there has been any recollection of it afterwards. If, however, the "coming round" seems in danger of going too far, it is easy to maintain a *statu quo* by a little ether in the intervals of straining.

The notes of a few recent cases will illustrate some of the points touched upon above; all were operated upon within the last ten days:—

Case I.—A man, aged over 50, with a large fixed goitre, believed to be malignant. There was a good deal of stridor, and the operation was a difficult one, lasting nearly one and a half hours. The patient was easily kept sufficiently under with but little ether. In this case the first sign of coming round was always slight coughing.

Case II.—A middle-aged lady with a substernal goitre, long standing dyspnœa and considerable stridor. An active lid reflex was present when the first incision was made, but there was no movement. This patient frequently required no anæsthetic for long intervals, remaining completely motionless and flaccid. While the ligatures were being tied I said, in order to test her, "Do you know where you are?" "Yes, in the theatre," was the immediate reply. "Do you know who I am?" asked the surgeon. "Yes," she said, giving his name. "Do you feel any pain?" "No," she replied. The operation was not an easy one, and prolonged straining was required to ensure that the hæmostasis was reliable. But neither of this nor of anything else connected with the operation had she any recollection whatever. The condition illustrated in this case is not very uncommon. It is a very curious one. The patient can see, hear, speak, and appears quite conscious, but does not move, and certainly

no stimuli from the seat of operation appear to reach the higher nerve centres. This condition can be maintained indefinitely by the use of the right amount of ether. The patient just described was a woman, but I think I have found the condition appearing more frequently in men, proportionately to the much smaller number of male subjects requiring operations for goitre.

Case III.—A lady, aged 59, with a large, nearly fixed goitre, and a history of many years' dyspnœa. The operation was a double resection extirpation; the trachea was found much compressed, being barely ¼ in. in width by external measurement. In this patient the first signs of any "coming round" were crying out and a tendency to general movement. In an ordinary case this would have given no trouble, one would simply have used a slightly deeper anæsthesia than usual. But the severe dyspnœa in this case made a specially light anæsthesia essential. This was very difficult to maintain. The "path" alluded to above seemed reduced to a line, the patient passing directly the anæsthetic was at all relaxed, from deep regular breathing to unruly movements and crying out, so that at one moment one sometimes received the warning: "Isn't she rather deeply under?" and the next the request, "a little more anæsthetic, please." The behaviour described in this case is very unusual, especially in patients with chronic dyspnœa.

Case IV.—A girl, aged about 20, with adenoma of the thyroid and no dyspnœa. In this case swallowing and slight movements of the head were the first "coming round" signals.

Case V.—A lady, aged 58, stout and short necked, with a large nearly fixed tumour and history of prolonged dyspnœa. The operation was difficult, especially the dislocation of the tumour. In this patient movements of the legs were the first signs. Movements of the legs were very marked also during the straining at the end of operation, so that, although there was, as usual, a strap across the knees, a nurse had to hold the patient's legs as well.

These cases, which were consecutive ones, show how differently patients behave under this light anæsthesia.

Advantages.—The advantages of light anæsthesia are I consider *diminution of disagreeable after-effects* and the far more important one of *increased safety*.

After-effects.—Many patients after a goitre operation feel no after-effects from the anæsthetic and are indeed surprisingly little affected by it. One patient, directly he reached his room after leaving the theatre, remarked that he felt inclined for a beef-steak, and I have seen many patients an hour or two after the operation, sitting up, saying they were feeling quite well and had had neither sickness nor nausea. In a good many cases there is slight sickness on coming out of the anæsthesia, which passes off rapidly. Out of 150 hospital

cases in which students had made careful notes of the after-effects, there were sixty (40 per cent.) who had no sickness or nausea at all, seventy-two (50 per cent.) who were sick just once or twice immediately after coming round, and eighteen (12 per cent.) who had vomited more than this, in a few cases the sickness lasting through the following night.

Safety.—Among the subjects of goitre operations there are two groups of cases of special danger, those with marked tracheal obstruction and those with cardiac trouble. Among the former I have had many severe cases: patients with tracheas narrowed and much displaced; goitres partially within the thorax or extending behind the trachea. It was taught formerly that patients with obstructed air passages should not have ether. I have never found any difficulty in administering it in goitre cases; given slowly it does not cause irritation or produce cyanosis. Many of these patients have a dusky complexion to start with, but I do not as a rule find this becomes worse under the ether, if it does I generally give a little oxygen with the ether. The patients with chronic dyspnœa are often particularly easy to anæsthetize in the manner I have described. As regards inductions, I find little difference either in the time or in the amount of ether used. These patients have to be got to the same point as the others for a smooth anæsthesia to be carried on. But once inside the line curiously little ether is often required to keep them there. The ether may often be withheld for long periods, the patient remaining motionless and without rigidity. It is almost as if he were keeping himself anæsthetized—by his own carbonic acid one is inclined to say—but there is no marked increase of cyanosis. There are many cases recorded in literature of deaths on the table in cases of severe dyspnœa and the anæsthetic has been looked on as at least a contributory cause. I find it difficult to believe that a conscious patient struggling against suffocative proceedings is not more likely to succumb than one under the light anæsthesia described. I except cases of dangerous acute dyspnœa; many of these, however, are in a semi-conscious condition which does not need an anæsthetic.

Cardiac Cases.—The group of cardiac cases includes cases in which the heart has become affected by long-standing dyspnœa, true exophthalmics, and those fairly common cases of goitres not typically exophthalmic associated with cardiac symptoms. In all these I believe the lighter the anæsthesia the better. The exophthalmic cases have all done well anæsthetized in this manner, the pulse being well maintained

and often slightly diminished in frequency. During the straining at the end the pulse-rate generally rises, but neither then nor at any other period of the operation have I noticed any ill-effects from the patient being insufficiently anæsthetized. In many of these heart cases a little oxygen may advantageously be given with the ether.

Ether by the intratracheal method is often advocated in cases of operations for goitre. The main disadvantage of this method lies in the difficulty and attendant risk of the passage of the tube in cases of severe obstruction, especially with displaced and narrowed trachea, even in the hands of experts; and the depth of anæsthesia required for this proceeding is in itself an added danger. Otherwise it has distinct advantages and produces a light anæsthesia easily maintained. In conclusion it may be said that no method is without drawbacks, and that every anæsthetist will probably get the best results from a method in which he is experienced and in which he believes.

Dr. Silk.

Mrs. Dickinson Berry's account of her experience is excellent. Her experience in these cases is so large that criticism becomes almost impossible, but there are one or two points upon which I should like to say a few words.

In the first place I think that these cases emphasize the importance of "team work," by which I mean that the surgeon and the anæsthetist should be accustomed to work with one another, and thoroughly used to one another's ways and methods. I do not know that Mrs. Dickinson Berry said this in so many words, but I think that she implied it.

The next important point is that all these cases should be but lightly anæsthetized from the very beginning, so that the depth of anæsthesia can always be fully under control. Mrs. Dickinson Berry has shown us how admirably she can do this, in fact I think that I may say that "light anæsthesia and complete control" were the predominating notes of her paper.

I was rather surprised not to hear any reference made to the use of rectal oil-ether. In my opinion this method is very useful in nearly all these cases, and particularly so in those of the exophthalmic type. It is not necessary or even desirable to administer full doses so as to deeply

anæsthetize the patient; half doses, or even less are quite sufficient; this forms a basis which renders the control of the oral administrations much more certain and easy. In exophthalmic cases too, it is quite possible, by means of giving very small doses two or three days beforehand, to educate these patients up to operation pitch, and so avoid a good deal of psychic shock. This plan is in fact one of the best examples of the method of "anoci-association" as introduced by Professor Crile.

Mr. James Berry.

Over thirty years have elapsed since I held the post of anæsthetist at any hospital, so that in responding to your kind invitation to take part in this discussion, I must do so from the point of view, not of the anæsthetist, but of the operating surgeon. There are, however, a few points on which I may venture to offer a little advice to any anæsthetists who may not be very familiar with the class of operation now under discussion. The first is that the anæsthetist should not begin the administration until he sees that the surgeon is quite ready for the operation. He should wait till the surgeon has put on his gown, gloves, &c., and is standing near the patient, ready to begin. For occasionally, especially in cases of urgent dyspnœa, it may be necessary to perform the earlier stages of the operation very suddenly and rapidly. On rare occasions I have had to do this and to dislocate the tumour rapidly to obviate a threatened attack of suffocation. It may be very important that no time should be wasted by the surgeon not being ready to intervene promptly. For the same reason, I never allow the anæsthetic to be given anywhere but in the actual operating theatre in which the operation is to be performed.

I may say here that in speaking of thyroid operations from the point of view of anæsthesia, there is all the difference in the world between the removal, say of a small thyroid tumour high up in the neck, a case in which no respiratory or cardiac complication is likely to occur, and the very common but far more serious cases in which there is urgent respiratory distress, or dilatation of the heart, or in which the operation is being done for Graves' disease. In the first class of case the administration of the anæsthetic is little if at all more serious than it is for any other operation. I would merely say that for *all* goitre opera-

tions I have entirely abandoned the use of chloroform, and consider that ether administered by the open method, and sparingly, is by far the best anæsthetic. It is in the second class of case, in which respiratory or cardiac complications exist, that the anæsthetist should be specially on his guard.

Before the administration is begun in a case of dyspnœa it is well to ascertain to what extent the head can be extended without serious interference with breathing, and during the subsequent operation never to allow the head to be extended beyond this. The operator who is endeavouring to extricate a substernal goitre will naturally wish to have the head extended as fully as possible; the anæsthetist must see that this is not done to a dangerous extent, or the trachea may become seriously kinked and perhaps completely occluded. It is my custom to employ an assistant, usually a dresser or a nurse, to hold the head firmly throughout the operation, and not to turn it to the right or the left, unless specially directed to do so. Some of my anæsthetists, however, prefer to hold the head themselves and to accept the responsibility for doing so.

This leads me to say that in any given case of dyspnœa the anæsthetist should acquaint himself as far as possible, *before* the operation is begun, with the exact position and shape of the trachea, generally a fairly easy matter, since the condition of the trachea can usually be inferred with much accuracy from external examination of the goitre.

A purely unilateral goitre, for example, tends to displace the trachea to the opposite side, to curve it and to flatten it on the side of the tumour. A purely parenchymatous goitre never flattens the trachea from before backwards, but always from side to side, and the point of maximum compression is nearly always about 1½ in. below the cricoid. An irregularly bilateral adeno-parenchymatous goitre may twist and displace the trachea in various ways. Goitres springing from one or other inferior horn of the gland and descending into the thorax usually produce oblique lateral flattening of the trachea.

It should be borne in mind that the dyspnœa produced by innocent goitre is always due to direct pressure upon the trachea, and has little or nothing to do with irritation of the recurrent laryngeal nerves. If the exact position and shape of the trachea be known to the anæsthetist, he can often relieve the embarrassed breathing by some slight alteration in the position of the head and neck. I may also add that throughout the operation, the patient's shoulders should be

well depressed, since elevation of the clavicles tends mechanically to compress a bilateral goitre, and thus to narrow still further the flattened trachea.

I come now to those cases in which the heart is seriously affected. These are the exophthalmic cases, and those numerous cases in which chronic dyspnœa, due to long continued pressure, has existed for years, and has led to dilatation of the heart and often to myocardial degeneration. Mere murmurs are in themselves of but little importance in goitre operations, but it is quite otherwise with dilatation and degeneration of the heart muscle, which add greatly to the anxiety and responsibility of both anæsthetist and operator. The condition of the heart may be estimated roughly by the anæsthetist by palpation, by observation of the position of the apex beat and of the pulse, but in all these serious cases it is far better, whenever possible, to obtain, before the operation is undertaken, the expert opinion of a cardiologist, based upon a complete X-ray and electro-cardiograph examination. I desire to take this opportunity of acknowledging very warmly the great assistance I have derived for many years from Dr. Strickland Goodall in this type of case.

With regard to exophthalmic cases it seems to be thought by some that all danger can be avoided by the use of local anæsthesia, but I venture to think that this is a most dangerous doctrine. No doubt there are some cases of exophthalmic goitre that are best dealt with by local anæsthesia, but for the majority of cases I am coming more and more to the conclusion that light ether anæsthesia is usually preferable.

Recently, for a forthcoming paper for another Section, I have been looking up the statistics of all my operations for the removal of goitre performed since my last consecutive series was published, in the Lettsomian Lectures for 1913.[1] It may interest the Section if I give some of the figures that have a bearing on the question of anæsthesia.

In the period from January 1, 1913, to January 1, 1920, there were exactly 500 such operations, 275 of which were performed on account of dyspnœa and seventy-nine for typical exophthalmic goitre. But whereas in 1913, out of 119 operations, local anæsthesia alone was employed twelve times, and local anæsthesia combined with a little general anæsthetic once, in 1914, of 136 operations, local anæsthesia alone was

[1] *Trans. Med. Soc.*, 1913, xxxvi, pp. 135-205, and *Lancet*, 1913, i. pp. 583, 668, 737.

employed only eleven times, and combined local and general nine times. The three years 1915, 1916, 1917 were spent largely in military work in Serbia, Rumania and Russia (where I naturally did no goitre operations), and in this period the total number of goitre operations was only fifty-nine, all under general anæsthesia. In 1918, out of seventy-five operations, only two were done under local anæsthesia, and in 1919, of 111 operations, only three. In none of these cases was there any anæsthetic fatality. These statistics do not include a large number of ligations of the superior thyroid artery for exophthalmic goitre during the same period, some with, and some without, general anæsthesia, in none of which has any fatality or serious complication occurred, anæsthetic or otherwise.

The above figures tend to show that I am using local anæsthesia less and less for all my goitre operations, as my anæsthetists become more and more expert in the use of light ether anæsthesia. And no one, I think, can deny that an operation for the removal of a goitre under a local anæsthesia is, at best, generally an extremely disagreeable performance for the patient. For the small and short operation of ligation of the superior thyroid artery it is however often sufficient and very suitable.

Dr. J. Strickland Goodall.

The performance of any thyroid operation (except possibly simple ligation) under any local anæsthetic is in my opinion undesirable on psychic grounds. The administration of any local anæsthetic containing adrenalin is probably positively dangerous, as it is liable to induce auricular fibrillation and possibly ventricular fibrillation, ventricular fibrillation being I believe, the cause of sudden death in thyroid operations. In every case of Graves' disease the heart is affected little or much. Owing to the kindness of Mr. James Berry I have had the opportunity of examining the hearts in a very large number of these cases, before, during, and after, operation. The chief points to which I pay attention in determining the suitability, or otherwise, of any given case for operation under an anæsthetic are:—

(1) The degree of myocardial exhaustion present.
(2) The amount of dilatation.
(3) The presence or absence of definite myocardial degeneration.
(4) The height of the systolic blood-pressure.

In all cases there is a certain amount of myocardial exhaustion. After any operation on the thyroid the blood-pressure tends to go up, and may be doubled. An exhausted, degenerated, or excessively dilated myocardium, may, when confronted by this increased pressure become acutely dilated, or fibrillate.

The best cases for an anæsthetic and operative treatment are in my experience those in which there are:—

(1) No definite myocardial degeneration.

(2) Little myocardial exhaustion or dilatation.

(3) A low systolic blood-pressure.

Electrocardiographic and X-ray examinations, together with the mapping out of the field of cardiac response are essential in estimating the condition of the myocardium.

Dr. J. Blomfield.

My practice is similar to that of Mrs. Berry, but I use preliminary injections of omnopon, scopolamine and atropine, and I find that a "conversational anæsthesia" makes most surgeons uncomfortable. I do not therefore commonly maintain a narcosis as light as that described by Mrs. Berry. I have had the experience of two cases which illustrate the importance of Mr. Berry's contention that the surgeon should be in readiness to operate at once from the moment when induction of anæsthesia has begun.

Dr. F. E. Shipway.

In all goitre operations, except those for exophthalmic goitre, I prefer to use intratracheal ether, preceded in the great majority of cases by morphine and atropine. Intratracheal ether removes all the difficulties of anæsthesia, so vividly described by Mrs. Berry, especially those caused by the pressure of the tumour upon the trachea. At the same time it gives complete control over the depth of anæsthesia, so that a very light anæsthesia can be easily maintained, and straining or coughing can be produced when the surgeon desires. For exophthalmic goitre I think there is no method so good as oil-ether combined with large doses of morphine and scopolamine. Even in these cases it is a

good plan to have the direct laryngoscope and catheter at hand in case any obstruction to breathing should arise during the manipulations. I have known this to occur in two cases. The combination of cyanosis and increased bleeding is extremely dangerous for these patients. Morphine and scopolamine should certainly be given to eliminate the element of fear.

Dr. F. S. Rood.

It appears to me that goitre cases are of three kinds :—

(1) Simple tumours or cysts where there is no respiratory obstruction and no toxæmia.

(2) Cases in which there is stridor, possibly definite respiratory obstruction and secondary heart failure.

(3) Cases of exophthalmic goitre.

In the first group there appears to be nothing special about the anæsthetic; in the second group chloroform appears to me to be a better anæsthetic than ether because the slight congestion which is sometimes caused by the ether during induction may entirely obstruct an already narrow air-way.

From some experience of exophthalmic goitre cases I think it is most important to guard against the element of fear and that these patients should not be brought to the operating theatre and anæsthetized on the table. A preliminary injection of morphia and scopolamine is most useful.

Dr. Harold Low (Chairman).

I am sure the thanks of the Section are due to Mrs. Dickinson Berry for the excellent paper opening this discussion. In her hands open ether has proved a very useful general anæsthetic for all forms of goitre. As Mrs. Berry has pointed out the only cases which require special treatment from an anæsthetic point of view are those in which the trachea is pressed upon or constitutional symptoms such as heart trouble or exophthalmos are present. The question whether a general or a local anæsthetic should be given is still undecided, some surgeons being strongly attached to the local and others preferring a general anæsthetic.

For my own part I think the ideal anæsthetic is either intratracheal ether or oil-ether *per rectum;* open ether on a mask is the next best method, but if used, light anæsthesia during the whole operation is imperative. Every precaution is necessary, and it should be recognized that a dangerous condition is present and none but a skilled anæsthetist should attempt the administration. Local anæsthesia came into use because of the disasters following chloroform or the closed ether method, both of which are dangerous.

Sections of Anæsthetics and Laryngology.

COMBINED MEETING.

Dr. LLEWELYN POWELL, President of the Section of Anæsthetics, in the Chair.

Regulating Dropper for the Supply of Ether.[1]

Shown by A. L. FLEMMING (Bristol).

THIS is a small arrangement that I have employed a good deal lately as a substitute for the ordinary ether drop bottle. It consists of an ether container (which may be attached to the administrator's gown by means of a safety-pin, or to the anæsthetic table by means of a small metal arm and clamp), three feet of special rubber tubing, and a nozzle with a control tap, which is held by the administrator. The delivery is very even, and can be immediately regulated to yield a continuous flow or a very slow dropping.

The apparatus is convenient in cases where the head of the patient is covered by sterile cloths, and by its means a perfect form of light anæsthesia may be maintained, and the total quantity of ether employed can be greatly reduced.

DISCUSSION ON ANÆSTHESIA IN THROAT AND NOSE OPERATIONS.

Dr. F. S. ROOD.

IN all branches of surgery the anæsthetic not only spares the patient pain, but an appropriate type and degree of anæsthesia materially contributes to the success of the operation. More especially is this the case in the special branch of surgery which is under consideration, in

[1] At a meeting of the combined Sections, held February 6, 1920.

which it seems to me that an active co-operation and partnership between the surgeon and anæsthetist, and an appreciation and sympathy with the "steps" of each other's work, are not only essential to the success of the operation, but are vital to the safety of the patient.

Operations upon the nose and throat are not generally serious in themselves, and therefore it is of primary importance that an operative technique be employed that is as free from risk as possible. At the same time it is obvious that an anæsthetic is given for a certain operation to be performed, and no method of anæsthesia can be recommended, however safe, which leaves the operation incomplete or exposes the patient to increased surgical risks, immediate or remote. Unfortunately deaths during operation have not been infrequent in this branch of surgery, and they have been either pure anæsthetic deaths or have been complicated by the entry of blood into the air-passages.

Now it is a fact that death during operation under ether is very rare—that death due to uncomplicated ether anæsthesia is practically unknown; the status lymphaticus does not appear in association with ether anæsthesia. Whoever heard of a patient dying of ether before the operation started? In this country during the last five years nearly 1,500 deaths have been reported during operation. Of course these statistics are of very little value; they give no indication of the condition of the patient before the operation, but it is a significant fact in a very great proportion of these cases the anæsthetic was chloroform or a mixture containing it.

I am indebted to the kindness of Dr. Silk for the reports of deaths under anæsthetics in the military hospitals. In two years and three months 121 patients died; 110 of the fatalities occurred when chloroform or a mixture containing it was being used; eleven were under ether. Out of these eleven ether cases nine were apparently dying of secondary hæmorrhage before the operation started. Each of the various Commands reported fatalities. The only Command with no death upon the table was the Irish Command, where—I quote Dr. Silk's own words—they would not even look at $CH.Cl_3$.

How many of us who spend our lives giving anæsthetics have not at some period had a death with chloroform. I do not mean a case in which the patient was moribund or died of shock; I mean a pure anæsthetic death, where the operation was probably of a trivial nature. I remember some years ago giving $CH.Cl_3$ to an adult for the removal of adenoids. Suddenly during the induction—the patient was moving about at the time—the respiration became exaggerated and sighing in

character, the face intensely pale and the pupils widely dilated: all signs of sudden cerebral anæmia. The pulse was absent at the wrist, and respiration continued for a short time in this peculiar sighing way, then ceased. We lowered his head, we raised his legs, we gave oxygen and did artificial respiration. For some minutes there was no response; then the colour returned suddenly, the pupils contracted, the radial pulse returned. Shortly afterwards respiration began again. After the first sign of danger, shown by the exaggerated, sighing respiration—the patient had no more anæsthetic. After breathing quietly for some time and being apparently out of all danger, the same thing happened again, with the same startling suddenness. No more anæsthetic had been given. Again we got him round, and again he appeared to be all right, and again for the third time he collapsed, and this time he did not recover, well over an hour after the first danger signal and the cessation of the $CH.Cl_3$, although in the meantime he had recovered sufficiently to move his arms and legs about. This case was a mystery to us, although I think Dr. Levy has since pointed out the mechanism. But this is the tragic thing about chloroform deaths: they are all mysterious. You cannot say a mistake has been made, or that there was any technical error. It comes to this: that the patient is dead, and nothing has been learnt from the catastrophe. Were it possible to have the case over again, the same methods and the same care would be employed.

Or again, I once gave a little child, aged four, $CH.Cl_3$ to have an X-ray photograph taken of a misshapen foot. No operation of any kind was performed, and only enough $CH.Cl_3$ given to keep the child still for the few moments necessary for the exposure. There was no difficulty of any kind; the child recovered and was quite well. Forty-eight hours later vomiting began, increasing in severity until it became incessant. The urine was loaded with acetone and β-oxybutyric acid, the child became comatose, and died.

The case with ether is so different. We do not get these mysterious deaths during the induction. If death occurs under ether, I think we may venture to say that the patient was either *in extremis* or some technical difficulty arose which may be foreseen and provided against next time. Furthermore, people do not die of acidosis after ether.

But I do not think that it is necessary for me to labour this point any more, as the evidence that ether is the safer drug is so overwhelming that I think there are few surgeons or anæsthetists nowadays who would differ from this view. Since the days of Clover great improvements have been made in the method of giving ether, so much

so that every type or degree of anæsthesia that can be obtained with $CH.Cl_3$ can equally well be obtained with ether, so that, apart altogether from the consideration whether deep or light anæsthesia is preferred for an operation, it seems to me that to administer $CH.Cl_3$ when a safer alternative is at hand is to incur a heavy responsibility.

My first contention is that ether is the proper anæsthetic for operations upon the nose and throat.

It will be necessary to speak of two degrees of anæsthesia, deep and light. I should like to define what I mean by this. In deep anæsthesia the respiration is regular, automatic and shallow, the larynx and pharynx are absolutely paralysed and immobile, and respond in no way to stimuli; the recurrent laryngeal nerve is paralysed, and the cords stand in wide abduction. The pupil is generally dilated, and does not react to light. All reflex activity of the pharynx and larynx is abolished; a Brünings's tube can be passed into the larynx, and there is no interference with the quiet rhythm of respiration and no spasm of the cords. The patient in this condition can take no active part in his own operation; he cannot cough. On the other hand, he cannot obstruct his own respiration by laryngeal spasm. If this degree of anæsthesia is induced with ether, the patient is pink and rosy, with a full, regular pulse of about 80 to 91.

By light anæsthesia I do not, of course, mean a struggling patient. There is general muscular relaxation; the respiration is regular, but certain reflexes persist; the pharynx and larynx respond with a muscular contraction if stimulated. The patient can cough, and so can in this way protect himself from blood entering the air passages; on the other hand, he can obstruct his own airway by spasm of the cords.

Of course I assume that before the administration of ether, atropine sulphate has been injected hypodermically. The contrast in the clinical picture of anæsthesia with and without this drug is too well known to need discussion.

There are many ways of inducing anæsthesia with ether, but in order to obtain this deep type a method must be used which is capable of giving a high concentration of ether vapour. So a modification of the so-called open ether method has been devised, which simply consists in extemporizing a bag from a towel as the anæsthesia progresses, and in converting the open ether into a semi-closed method, practically into a Clover inhaler, only capable of giving more ether, and without the disadvantage of the absolutely air-tight and possibly septic rubber bag.

This method is extravagant both of gauze and ether, a whole six-

yard roll of gauze being used for each case, and from seven to eight ounces of ether, according to the physique of the patient; but it produces in from six to eight minutes a type of anæsthesia which I believe can be obtained in no other way. It requires, of course, some little practice to get the patient under quickly and quietly, and entirely depends upon a regular increase in the strength of the vapour, so that the patient is always inhaling the maximum percentage of ether that will pass through the larynx without producing spasm.

In order to save time in description, I have had three photographs taken from actual cases. The first shows the commencement of anæsthesia. The gauze mask, soaked with as much ether as it will conveniently hold without dripping, is gradually lowered to the patient's face. The second shows how a bag is improvised by means of a towel; and the third, how the percentage of ether is finally concentrated considerably by means of a thick substance. In this case, to emphasize the point I have used a blanket, but of course a thick towel would do equally well. Now, although this looks asphyxiating, the practical fact is that, when the respiration becomes shallow in the deep stage of narcosis, the patient, although most profoundly anæsthetized, is pink and rosy, provided always that the even, regular rhythm of respiration has not been obstructed by a too sudden increase of the strength of the ether vapour during the induction.

Operations upon the nose and throat stand out in marked contrast to all others in that blood can pass directly into the air passages. It is this fact that has hampered the operator. First and foremost is the necessity for hurry; secondly, obstruction of the view; thirdly, the necessity for sudden alteration in position of the patient. No abdominal surgeon is called upon to operate upon a rigid and vomiting patient; nor handicapped by the necessity for speed. We are familiar with operations upon the mouth for malignant disease in which the surgeon worked at a disadvantage and in a great hurry upon a patient sometimes struggling: certainly coughing up blood at intervals and in danger of suffocation. Gradual evolution in the operative and anæsthetic technique has now produced a quiet patient, some form of mechanical airway, allowing the operator ample time and opportunity to control hæmorrhage, resulting in a practically bloodless operation and a clear field for surgical manipulation. It seems to me that this general surgical principle may be extended to nose and throat operations, and I shall endeavour to maintain the view that this may be done with complete safety, with some slight alterations in traditional methods.

As the technique of anæsthesia differs so materially in different cases, it will perhaps be an advantage to classify the cases according to the conditions present. For practical purposes we may consider that operations upon the ethmoid, intranasal operations for drainage of the frontal sinus, removal of portions of the inferior turbinates and operations for drainage of the maxillary antrum both the intranasal and the radical Colb-Sudi operation have one feature in common: blood is introduced directly into the nasal cavities and so into the nasopharynx, while at the same time the head of the patient is raised. It is obvious that gravity will allow blood to find its way directly through the larynx into the trachea and lungs.

It appears that there are three alternative methods of preventing the blood entering the larynx :—

(1) To use light anæsthesia and allow the patient to cough up his own blood. This is generally effective, but has many disadvantages. There is the necessity for speed ; also the amount of blood collecting in the pharynx may become great, necessitating the interruption of the operation in order to mop it out. Furthermore I have seen blood sucked into the trachea even with a coughing patient.

(2) To shut off the nasal cavities from the pharynx by means of a well fitting post-nasal plug—using at the same time one of the many forms of mechanical breathing tubes, of which Hewitt's airway is a type, in the pharynx. There are two objections urged against this procedure. It is said that a post-nasal sponge by preventing the blood running into the pharynx and confining it to the nasal cavities causes it to flow from the nostril, so obscuring the view of the operator. But it appears to me that it may be urged upon the other side that although it may sometimes be an inconvenience, still if the blood is not confined to the nasal cavities it will run through the posterior nares into the pharynx, possibly slightly adding to the danger of the operation however lightly the patient may be under—furthermore it will certainly make the patient cough and by collecting will once more introduce the necessity for a speedy completion of the operation. It seems to me personally that the advantage gained by the opportunity of a leisurely completion of the operation, together with a patient who is absolutely safe from suffocation, even if it does increase the amount of mopping through the nostril, outweighs the transitory gain of a few moments' clear view accompanied by an increasing collection of blood in the pharynx which may at any moment give rise to a surgical emergency.

(3) The third method of preventing blood entering the larynx in

those operations in which the patient is in the raised position is by using the intratracheal insufflation of ether. That it prevents blood entering the air passages is universally admitted; it maintains an even, regular degree of anæsthesia, and a mechanically perfect airway whatever the position of the head. It will be very interesting and instructive to hear the opinion of those who have used this machine for these cases. Personally I think that for operations upon the maxillary antrum for malignant disease, or even for the radical drainage operation, it is almost ideal, but for the more delicate intranasal operations I have noticed that the return blast of air through the nostril churns up the blood and appears to cause the operator more inconvenience than a post-nasal plug. I have often used this machine in conjunction with a post-nasal plug—which prevents the air blowing through the nostril, but the apparatus is bulky for universal use in operations of this nature, and I think that the results obtained with a post-nasal plug and an airway are equally, if not more, satisfactory.

The next anæsthetic technique which I wish to consider is associated with the removal of tonsils and adenoids. I know that there is considerable difference upon this subject. It is apparent that the type and degree of anæsthesia to be used is intimately connected with the nature of the operation performed and it is in this operation I think more than in all others that a sympathetic co-operation between the operator and anæsthetist is essential both in the interests of safety and efficiency. I have said before that personally I believe ether should be used as the anæsthetic. That it is safer than $CH.Cl_3$ cannot be denied. That any degree of anæsthesia can be produced and continued with ether and ether only in the most vigorous adult is a fact that can be verified at any time. There are from the anæsthetist's point of view two methods of removing tonsils—one with light anæsthesia, the other with deep anæsthesia.

The first, which I will describe as the rapid method, consists in the removal of both tonsils with the reverse guillotine, the patient either lying on his back, or sitting up. After removal, the patient is quickly turned into such a position that the blood may drain out of the mouth. Some operators remove the tonsil with the patient lying upon the side.

In the second type of operation a profound degree of anæsthesia is used. Mechanical means are adopted to prevent the entry of blood into the air passages. The tonsils are removed either by the reverse guillotine or by dissection. All hæmorrhage is stopped before the patient leaves the table as in other surgical operations.

The profound anæsthesia necessitated a modification in the traditional position. It was essential in the interest of safety that gravity should no longer carry blood into the air passages, and to adopt that general principle of operative surgery of the mouth, that if the advantages of a deep anæsthesia, together with a quiet field of operation and opportunity for a leisurely performance are to be enjoyed, some mechanical means must be adopted to keep blood out of the air passages. In the special operation under discussion this end is secured in a very simple manner. The shoulders are raised upon a sandbag. The head is allowed to fall back and rest upon the vertex in such a manner that the front of the chest, the front of the neck and the chin are all in one straight line. This position places the larynx well above the field of operation and converts the nasopharynx into a dependent receptacle. In this position and in this position only deep anæsthesia may be used either for enucleation by dissection or by the reverse guillotine. The surgeon has all the facilities for which he asks—ample time, an immobile field of operation, and every opportunity to stop hæmorrhage.

The pros and cons of the different types of operation form an entirely surgical matter, but the question of safety of the various procedures is relevant to the subject under discussion.

The originator of this method adopted the position so widely used for other operations of a similar nature, such as those for cleft palate or hare lip and operations for removal of small parts of the tongue. These operations are essentially the same as tonsil operations: the conditions are identical. They used to be performed with the head slightly raised, causing considerable discomfort to the anæsthetist together with much mopping and coughing. But a cleft palate operation in the head-back position presents no difficulty or danger; the surgeon sitting behind the patient has ample time and perfect vision for a somewhat tedious operation. An occasional mop prevents blood collecting in bulk in the nasopharynx. The patient is under, with even, regular respiration, the palate is still, and there is no anxiety or difficulty for the anæsthetist.

It is the same thing with the tonsil operation. Carried out by two people who are conversant with the details I do not think it is too much to say that it is mechanically perfect, and that if blood does enter the larynx it is due to a failure of technique and not to a defect in the principle of the method.

Another group of cases in which the anæsthetic plays a considerable

part are those for direct examination of the larynx, trachea and œsophagus by means of tubes. It seems to me that a profound anæsthesia is essential for these cases: anæsthesia so profound that the cords stand abducted, even in the presence of a tube in the upper opening of the larynx. Spasm of the cords and contraction of the pharynx are the difficulties the operator has to contend with, and I do not think it is too much to say that some of the delicate intralaryngeal manipulations are impossible should this persistently occur. Again, I think ether is the proper anæsthetic to employ—it is sometimes necessary, especially with an irritable larynx to push it to an extreme degree, almost to the cessation of respiration.

Finally, there are operations upon the larynx for the removal of carcinomata. From the anæsthetist's point of view the conditions present are precisely the same as in cases of malignant disease of the upper opening of the larynx, pyriform fossæ, &c. The air passages are more or less obstructed by a tumour. There may be stridor, possibly definite respiratory obstruction. Now here we have a very different state of affairs compared with most other operations upon the nose and throat. The cases are of a really serious nature. An anæsthetic must be given which is possible in the presence of obstructed respiration, in order that a preliminary tracheotomy may be performed. Now if ether is administered to these patients the rise of blood-pressure during the induction may cause slight swelling of the mucous membrane and completely occlude the already narrowed airway, with the result that we have an asphyxiated patient, an emergency tracheotomy upon a patient not properly under, much explosive coughing upon the introduction of the tracheotomy tube with possibly the aspiration of blood and septic matter. Therefore I think that $CH.Cl_3$ is undoubtedly the proper anæsthetic because in the broad sense it is safer than ether in these special cases. A degree of anæsthesia must be maintained sufficiently deep to permit the tracheotomy tube to be introduced without producing reflex coughing—because pneumonia following aspiration of septic matter is a very definite danger, and a proper degree of anæsthesia will always ensure a peaceful tracheotomy.

In summing up I may say I have endeavoured to uphold: The safety of ether; the danger of chloroform as demonstrated apart from all scientific considerations by the perusal of the daily press; the improvements in the methods of administering ether so that the congestion, cyanosis and salivation of other days has given place to an anæsthesia which has all the merits and none of the dangers of

chloroform. The gradual evolution of operative surgery of the mouth—from the rapid operation and coughing patient towards the ideal of a quiet patient free from all risk of suffocating combined with every facility for a thorough and deliberate operation, much to the benefit of the patient and comfort of the operator.

Mr. Herbert Tilley.

There are two points we have to consider in this matter. The first and most important is the safety of the patient; the second is the convenience of the surgeon.

With regard to the first, I do not think anyone will dispute that general ether anæsthesia is safer than chloroform, or any mixture in which chloroform is an important constituent. I think my experience of chloroform must have been unfortunate, for I have seen eleven deaths under this anæsthetic: three of them in my own practice, two of which occurred before the operation had been begun. In each instance a comparatively trivial operation was to be performed. The patients were apparently healthy, indeed, one of them was as fine a specimen of a man as I have ever seen. After an experience of this kind one turns to any method of anæsthesia which promises to eliminate such disasters. The worst of chloroform is that one never knows when a calamity may happen. Dr. Rood will remember the instance of a Cambridge undergraduate, who was a member of his college boat. As he was so healthy, I asked Dr. Rood (in those days, when I felt less certain of the advantages of ether than I am now) if he would mind giving chloroform. The patient had taken only a few breaths, when he stopped breathing, became pale suddenly, the pupils dilated and he exhibited all those alarming symptoms which Dr. Rood has described in detail; fortunately this patient did not die.

With regard to the second point, on talking over with my friends the question of the convenience to the surgeon, I have often been met with the remark, "I agree that ether is the safer anæsthetic, but there is so much bleeding under it." Yet, I put it to those present, that in this respect there is an advantage in ether. If you give ether in the way that Dr. Rood has described this evening, it is true there is a little more oozing *during* the operation, but it takes place during the operation when the surgeon is there to deal with it and while the anæsthetist is present. But take the case of chloroform. The blood-pressure is

lowered under its influence, there is very little bleeding, the patient may be left without any hæmorrhage being evident, and is put back to bed. Later, when the blood-pressure has risen, some point, which would have been evident if ether had been used, begins to bleed ; then there is the trouble of stopping it—the patient, who has become restless, has been bringing up blood, and is getting paler, and collapsed. In a child, a fresh anæsthetic may have to be given, and practically a second operation performed. I hope everyone makes it a rule not to allow a patient to leave the table until bleeding has ceased. I have only been called back to two cases of bleeding after the operation of enucleation of the tonsils during the last five years, and in one of those I predicted I might be called because I had to finish the operation in semi-darkness. Dr. Shipway came to the second intervention and helped me to secure the bleeding point.

With regard to the posture of the patient, in order to prevent the blood going into the lungs, I always adopt the supine position with a sandbag under the shoulders. By this means no blood can enter the lower air passages, and I believe this to be the reason why I have never had a case of pneumonia after enucleation of tonsils. Hence my dislike of the sitting-up position or any lying-down position where the head is slightly raised on a pillow. I show you some slides indicating the position of the patient during the operation, and the way in which we turn him over afterwards, so as to ensure that not a drop of blood can get into the lungs.

Dr. William Hill.

Dr. Rood has omitted to mention one very important class of throat operation, that for adenoids and enlarged tonsils, where patients are quickly anæsthetized under ethyl chloride, and lightning operations are carried out. That is the most commonly performed operation in this country, and many thousands are done in out-patient clinics. In recent years open ether has been advised in the journals at frequent intervals, but it is obvious that the term " open ether " is not applicable to all the methods of giving ether, in contradistinction to the old closed method of Clover. Dr. Rood's method is really an enclosed ether—open ether sequence ; and Dr. Rood has told us the induction takes eight minutes. I know he often abbreviates that period. He has given us the maximum for fairly deep anæsthesia for a dissection operation on both tonsils and the removal of adenoids. If ten to twenty patients have to be operated

upon in the out-patient department before one commences the day's work, the enclosed ether—open ether sequence is not adapted for the reversed guillotine operation in a busy hospital. But it is known that these rapid operations are done satisfactorily under ethyl chloride, about which nothing has been said. Perhaps Dr. Rood's reply will be that he does not approve of it, or that he works for men who do their business in a more skilful and efficient manner—e.g., by prolonged anæsthesia and dissection instead of guillotine enucleation. For my own part, I am very fond of the dissection operation, although I was brought up on the guillotine method. I think we cannot ignore the short ethyl chloride anæsthesia and the use of guillotine and curette for the rapid operations which are necessary in out-patient work. I have seen only one case of difficulty, in which there was nearly a death, from ethyl chloride for a tonsil operation.

The gas-and-ether method was much in vogue twenty or thirty years ago, and I have removed many tonsils under it, but we were not so particular in those days about leaving behind a bit of tonsil, the result being that some patients turned up at another hospital, and there were reflections as to the way the work was done at the first hospital. In private practice, however, we must take care to remove everything. It must be remembered that ethyl chloride is not always given by experts; sometimes the one who gives it is a junior man, and yet I believe the record of accidents cannot be strikingly high. Such deaths rarely occur in out-patient departments in the ordinary morning's work. Therefore I regret Dr. Rood omitted this very important aspect of the subject in his paper, namely, the question of short anæsthesia.

Dr. G. A. H. Barton.

When I first heard of Dr. Rood's method of giving ether for the operations of enucleation or dissection of tonsils, I must confess I was somewhat sceptical as to its practicability. It was recommended in an article in the *Practitioner* last year by Dr. Hill. I spoke to him on the matter and expressed my surprise that he should be satisfied with an ether anæsthesia in these cases. He assured me that it was all right, and added that Mr. Tilley also strongly approved. For twelve years I was at the Throat Hospital, Golden Square, at the time when Mr. Tilley was on the staff, and always understood from his anæsthetist that his ideals in anæsthesia were very high. I felt, therefore, that if

the method were good enough for Mr. Tilley it should be good enough for anybody. In practice I was agreeably surprised to find how well the patients did under it.

In inducing the anæsthesia my method differs a little from Dr. Rood's. It is my custom in almost all inductions to use a little ethyl chloride, in these cases only a very few cubic centimetres, just enough to stimulate the breathing, confuse the patient's mentality, and dispel his feeling of resentment to strong ether vapour. I do not gather what thickness of towel Dr. Rood uses, or what quantity of ether he requires to get his patients to the desired depth of narcosis. I soak a mask in ether, and on to it as soon as the patient begins to breathe naturally I spray the ethyl chloride, cover the whole with a four-fold towel, and as soon at the breathing becomes stimulated, which is almost immediately, I begin pouring ether on the top of this, using from 1½ to 3 oz., according to the age of the child. I then cover in the whole again with a second four-fold towel. This suffices as a rule in four or five minutes to produce the required depth of anæsthesia.

As Dr. Hill says, the method is neither a strictly open nor a strictly closed one. I suggest that as it involves a considerable degree of air limitation it might be called the " aëropenic " method.

Dr. Rood did not include any reference to endolaryngeal operations. Tracheotomy is not always done beforehand, and if it is performed through an endoscope the administration of anæsthetics by the airway is difficult for the anæsthetist and embarrassing to the operator. In these cases I find rectal oil-ether particularly useful.

Sir StClair Thomson.

The subject of this debate crops up every ten or fifteen years, but, like our friendships, it has to be kept in repair. The only criticism I would make is, that the subject is too wide, because anæsthesia for operations on the nose, where the blood can be cut off from descending into the air passages by post-nasal sponges, differs very little from that in other surgical operations. In the same way, if blood is cut off from descending, say when laryngotomy is done and the larynx is blocked, or when laryngofissure is done and packing is put on top of the tracheotomy tube, it is one of the smoothest and least risky anæsthesias which exist. Anxiety comes in operations on the pharynx.

I will not trouble the meeting with hypotheses or prejudices, but

will contribute a small amount of observation. The fatalities with which I have been associated are three, and, unfortunately, they were in private patients. The observation I make on these cases is worth remembering: that neither the operator, nor his technique, nor that of the anæsthetist seemed to account for the fatality. In one of the cases I had removed the adenoids and was proceeding to attack the tonsils—I used to do the adenoids first in those days—and the patient died. In the next case, I had removed one tonsil by enucleation and was quietly taking it out of the ring and examining it, when the patient died. In the third, both tonsils had been enucleated; there was a pause before passing on to the adenoids, and the patient died. In none of the cases was death due to blood descending into the trachea, nor to traction—which is often advanced as a possible cause—nor to the giving of the anæsthetic. I could not say the patient was too profoundly under, or that the death occurred at any particular stage. In the three cases the post-mortem examination was made by an independent coroner's pathologist, who found status lymphaticus in all.

In America they have given ether so almost universally that people do not talk of chloroform, they talk of taking ether. Last summer I saw many operations there under it. It was rather frothy and bloody, compared with the chloroform operation. One of the most skilful operators on the tonsils, which he removes by dissection, in New York, Professor Coakley, has given up ether and gone back to chloroform.

With regard to what Dr. Rood said about operations under direct endoscopy, Dr. Chevalier Jackson carries out all his endoscopic operations without any anæsthetic at all. I have seen a boy aged 10, with a foreign body in the bronchus, lie down on the table, and without anything in the form of an anæsthetic, general or local—save for the operator's persuasion—have the foreign body removed. It is rather a *tour de force*, and I do not know whether we can all imitate it.

The subject of statistics has not been broached, and we have to be careful about that. Dr. Silk's figures in the Army of 100 deaths under chloroform can only be taken in conjunction with the total number of administrations of both ether and chloroform.

In conclusion, I would only say that if the anæsthetist can assure us that ether is safer than chloroform, we can do our pharyngeal operations as smoothly and as safely under it as with chloroform, and that if the results as regards hæmorrhage and bronchial affections are as good, we shall be prepared to change over to ether.

Dr. Silk.

As I have been referred to by Dr. Rood in connexion with some statistics of deaths under anæsthetics with which I have supplied him, I should like to take the earliest opportunity of disclaiming any personal responsibility for those unfortunate occurrences. The figures merely represent the cases which were reported to me in my official capacity.

I am fully convinced of the truth of Dr. Rood's view as to the advantages of ether, and as to the lesser death-rate which appertains to the use of ether. And that is not because I cannot give chloroform, for my career as an anæsthetist commenced under Lord Lister, who, in addition to his other distinctions was a very able and skilled anæsthetist.

Sir StClair Thomson says that in connexion with the death-rate no exact or comparative figures have been quoted, but that raises so large a question that it might well be discussed at a separate meeting. Of the figures quoted by Dr. Rood as coming from me, I would like to say that a considerable proportion came from Scotland, which I think is very suggestive, and fully bears out the opinions expressed by Dr. Rood.

With regard to the subject in general, I may not perhaps be altogether in sympathy with modern views, but I think that I can claim to have had considerable experience. This evening, however, I only propose to raise one or two questions. We have heard much as to the posture of the patient, but I am not altogether convinced that the prone position with the head hanging over a pillow, is the best. Another posture which I suggest as being possible, though not of course for adenoids, is with the patient sitting bolt upright. I have had the pleasure of giving anæsthetics for many of Sir StClair Thomson's patients in which this posture was adopted, of course with the feet raised on a table. The blood then flows out of the nose and mouth, and has no tendency to fall down into the pharynx or larynx. In none of the cases alluded to have I had the slightest trouble. It is true that I have heard of trouble from others, but even in those the trouble was always quickly corrected by lowering the head for a few seconds.

The question of ethyl chloride has also been raised. It is assumed—why I do not know—that this drug must always be given in a bag. I think that Dr. Rood and others will agree with me that it is the bag, not the ethyl chloride to which anæsthetists object; it is when the bag is used that fatalities are most apt to occur. Giving it on an open mask, in the manner suggested by my friend Dr. Barton, may not be perfectly safe, but is a good deal safer than when the closed method is

adopted, and it is certainly very much more rapid in its action than is any other anæsthetic given by the open method.

The chief objections to the use of 'open ether 'are the length of time required for induction, and the large quantity of the drug used. I suggest that both time and quantity can be considerably economized if a minute quantity, say 3 per cent., of chloroform be added to the ether. It "takes the edge off" the ether in quite a wonderful way, as I am sure that all those who have tried it will agree. Instead of eight minutes, a patient can be got well under in five minutes; instead of using from 8 to 10 oz. per hour, the quantity can be reduced to 5 or 6 oz. per hour. This combination has the additional advantage, too, of not giving rise to so much congestive hæmorrhage as when pure ether is employed.

Finally, I venture to bring before the meeting an instrument for the administration of anæsthetics in certain nose and jaw cases. It is known as Kühn's intubation apparatus, and is not, I think, so well known in this part of the country as it deserves to be. It is easy of introduction, and the pharynx can be packed all round with sponges and gauze so that no blood can get past, and when the size of the "feeding" tube is not of importance, the apparatus possesses many and great advantages. This is only an incidental allusion, but I think that in considering all possible means of giving anæsthetics for these operations, one ought to take into account every device which has been found useful, and which some might like to employ.

Mr. W. Stuart-Low.

I differ from Dr. Rood on some points in the definite attitude he has taken. I consider that there are many points other than the anæsthetics used with regard to the important bearing towards the success of administration. One point is the preparation of the patient. I believe atropine given before administration of the anæsthetic is very valuable. Only recently I heard of a case in a provincial hospital where a fatal result took place from the administration of the anæsthetic before which the patient had been allowed to wait in a corridor in his dressing gown and slippers and smoking a cigarette while waiting his turn. This I consider most careless and reprehensible. The patient should be kept quiet, and should be brought into the anæsthetic room on a trolley. The anæsthetic room should be well warmed—a most important point. In India where the atmosphere is

warm and the patient warm it is remarkable how few deaths occur under anæsthetics, a fact which is attributed to the temperature of the operating-theatre. I also attach great importance to the time of day at which the operation is performed. I believe as far as possible, unless in emergency cases, in operating as early as possible, and I invariably operate at 8 a.m. It has been shown that the vitality of the body is at its lowest at 3 a.m., and gradually rises until noon, after which it subsides till 3 p.m. It is, therefore, a great asset in favour of the patient's standing the anæsthetic and of a good recovery to operate at 8 o'clock in the morning. This is particularly advisable in the case of young children, who in this way are not kept waiting without food, and are not disturbed before they are taken by surprise and anæsthetized and carried into the operating-room.

Sir James Mackenzie, in some of his writings, complains that students are not trained in hospitals to become practitioners in manner and in general management of the patient, and I have found young qualified men and house-surgeons very apt to consider the patient to be anæsthetized merely as a case, and that they often fail to receive him with genial and kindly remarks while administering the anæsthetic. I am sure that sympathy, shaking hands, and pleasant words are most helpful, for it has been said that whatever the patient is thinking of when he takes the anæsthetic he will dream of while under its influence, and I am certain that a pleasant impression will soothe the mental irritation or apprehension.

Another point that I emphasize is that absolute silence should reign in the operating theatre during the administration of an anæsthetic. A distinguished anæsthetist enjoins this by putting up a large card with "silence" upon it while he is administering. To avoid clatter of feet upon the concrete floor rubber or felt shoes should by worn by operator, nurses, and assistants. Conversation should be strictly barred, as it is likely to interfere with the administrator's careful listening to and timing of the patient's breathing.

I lay great stress upon team work, and have emphasized this in recent correspondence in the *British Medical Journal*. Surgeon, anæsthetist, assistant, and nurses should be those accustomed to work together. I make a practice of reading reports of, and investigating, all cases of fatal anæsthesia of which I get to know, and I am certain that neglect of the conditions I have emphasized has in many instances led to a fatal termination. My sympathy is always with the anæsthetist, and I am sure some operators are too slow and do not sufficiently study the matter

of team work during throat operations, and that they expect the patient to be too long and too deeply under the anæsthetic. It is particularly reprehensible to readminister the anæsthetic before proceeding to remove the second tonsil, a procedure which is much to be deprecated.

Dr. Silk, in his remarks, speaks very strongly of the number of fatalities under chloroform in Scottish hospitals. I cannot bring myself to believe this, and think there is some mistake in his statements. The method employed is almost invariably of the simplest possible, and I have strong reason to believe the safest. I would say the simpler the safer is my experience. I should not like my patients to be swathed round with yards of lint, the entire head and face being obscured, and so much anæsthetic poured on in a given time, and, eight or ten minutes having elapsed, the head uncovered ready for operation. I think a mask or towel covering the nose and mouth, and careful observation of breathing, complexion, and pupils are essential to the successful and safe administration of any anæsthetic. At any rate, such has been my experience of thirty years, during which period I can truthfully say I have not had a single fatality in a young person.

In my opinion, chloroform is the best anæsthetic, but it must have the best administration—only the skilful, careful, and practised should be entrusted with it.

Dr. P. Watson-Williams (Bristol).

I have learned a great deal at this meeting, but there are many points upon which one would have liked to have information. For instance, there is the question of whether one should give scopolamine and morphine in conjunction with these anæsthetics And we have heard very little about blocking off the nerves by injection of branches of the fifth; this we have done in Bristol with considerable success. In this country generally there seems to have been little perseverance with the method, probably because our anæsthetists are able to induce anæsthesia so safely and comfortably. And we should like to hear about the dangers of adrenalin given in association with anæsthetics.

I find the chief topic of the discussion to-night is the relative value of safety of ether as compared with chloroform, and that is a large subject. In 1886 I held a resident post which involved the giving of anæsthetics, at the Royal Infirmary, Bristol, and I then introduced there the gas-ether method. I should have liked to hear opinions on that and other combinations, because though I have leaned towards the use of ether

from those early days, I have not given an anæsthetic for years. Still, as an operator, one feels a certain responsibility and gains some experience.

Many years ago I nearly had the same kind of misfortune as that to which Dr. Rood has alluded, when a young healthy child, during an operation for the removal of tonsils and adenoids, nearly expired under light chloroform anæsthesia. I was then strongly imbued with the view that ether was the safer anæsthetic for operations on the respiratory tract, and I have usually insisted on ether since, wherever available. I do not think ether materially increases the liability to hæmorrhage, and if it is given slowly by the open method I do not think it increases bronchial secretion. As far back as 1896 Dr. Flemming started giving it in Bristol by the drop-bottle method (*see* p. 1), which is the open method *par excellence*, and so we do not see the disadvantages with ether which upholders of chloroform maintain belong to it.

I was very interested in Dr. Rood's remark that in deep anæsthesia the vocal cords are widely abducted. In the early eighties, I think it was, Hooper, of Boston, conducted experiments on deep ether anæsthesia, and showed that, unlike almost all other conditions which induced vocal cord paralysis, in which the abductors go first and the adductors last, with ether anæsthesia the condition is reversed. Now during forced expiration, the pulse frequency is slowed, owing to medullary cardio-inhibitory impulses, and thus with toxic ether paralysis of the adductors there is a lessening of the influence of those pulse-slowing factors. That, I think, is one of the reasons why cardiac inhibition is less during ether than during chloroform anæsthesia.

There is the further point—the influence on the fifth nerve—in all operations on the upper respiratory tract. The bulbar communications, with the cardio-inhibitory nerve and the intense stimulation of the fibres of the fifth, are much more likely to induce cardiac inhibition than in operations on other regions of the body. For many years in Bristol we have advocated the use of ether for operations on the upper air passages. I know many of my colleagues in London were for a long time wedded to chloroform, and remained true to it long after we had given it up. I am surprised but pleased to hear few voices raised in defence of chloroform.

Sir WILLIAM MILLIGAN (Manchester).

I have had the good fortune—some of you may say it is a misfortune—to have been brought up in Scotland, and therefore I have had a leaning towards chloroform during my whole surgical career, and, at the moment, I have very little reason to regret it.

First, I would say I do not think any operation ought to be considered as a trivial operation. When we begin to talk about "trivial operations," our whole mentality, and the mentality of those who are associated with us in the operation, is altered. I think every operation is a serious matter, it certainly is from the patient's standpoint, and it may be so to the person who is performing it, as it should be. We should all approach an operation resolved to do our best in every way.

Secondly, I agree with the remark made by Mr. Stuart-Low: it is one I was going to make myself, to the effect that I think operations ought to be done in the morning, except those that are urgent. I am convinced that the patient is in an infinitely better condition for operation in the morning, and that the zeal of everyone is greater and keener.

In the third place, I presume what is said at such a discussion as this should be the embodiment of one's own practical experience; the meeting should know what the speaker has been doing. In the course of my experience, I have probably performed as many operations as anyone in this room, and I confess I have a perfectly open mind as regards the anæsthetic used: I hardly ever interfere with the anæsthetist. I have had the good fortune to have, working with me, four or five different anæsthetists, all competent men. They know my work, they understand my idiosyncrasies, and I understand theirs: and when I do an operation, I insist upon having one of the anæsthetists with whom I work in hospital. I have sometimes been chided for not giving work to outside anæsthetists, but I reply that I have men who have been working with me in the hospital, that they are accustomed to my work, and I am used to their methods of administration. I think there is much in the personality of the anæsthetist.

I have been pleased to hear Dr. Silk's remarks on Kühn's peroral intubation tube. Many years ago I showed the tube at a Laryngological meeting. At that time it was much derided, and I was told I was showing what was a fad. I have used that tube for ten years, and I employ it on all possible occasions: even at times when I am operating

on the nasal septum. The anæsthetist is at liberty to give open ether through the cone, or he can give chloroform. I almost invariably use Kühn's tube in operations for antral and frontal sinus suppuration, and the benefit arising from its use is very great indeed: you can entirely preclude any risk of blood entering the trachea if you pack properly. It requires a considerable amount of gauze at the back of the pharynx, but it is easily done, and it is an absolute preventive. The only risk of a Kühn's tube—I do not know whether Dr. Silk will bear me out in this—is that when the gauze is taken out of the mouth there is always a blood clot, and it is essential that it should be removed, otherwise the blood clot may be sucked into the larynx. I have only had that happen on one occasion, but it nearly cost the patient his life, and if it had not been in hospital the life would have been lost. As soon as the gauze has been removed, I pass a sponge along the convex surface of the tube, and lift up the blood clot. The use of this tube avoids all mopping out of the mouth, and it is a means of giving direct anæsthesia; moreover comparatively little anæsthetic is required, because it is introduced directly into the lungs.

The question whether other drugs should be used in connexion with anæsthetics is a very difficult one, and in regard to it one must use much discretion. I think if adrenalin is injected into the body when a general anæsthetic is being used, it will be found to be an extremely dangerous drug. If it is merely rubbed into the mucous membrane, and absorbed through it, it is comparatively innocuous, but I wish to utter a warning about injecting adrenalin if other anæsthetics are being used at the same time.

In roughly speaking 50 per cent. of my cases chloroform is used, and in 50 per cent. ether, but when I do have ether used, I insist that there be a preliminary injection of morphia and atropine, and under those conditions I have never been seriously troubled with hæmorrhage. So I am still in the position of knowing which is the best and safest anæsthetic. And I am thankful to say—I say it with no boastful feeling—I have never had a death, I have never seen a death under an anæsthetic. That, however, has nothing to do with me, it is to the anæsthetists' credit. Still, we must remember it is the surgeon who is responsible, according to law, for the death. Therefore we must back up the anæsthetist as far as we can, and my method of backing up the anæsthetist is to leave him to use the accumulated experience which is his.

Mr. Boyle.

I think we should not go away with the idea that ether is the only anæsthetic which can be used for nose and throat operations, and I would like to tell you of a method I have been using for two years. It consists of a combination of anæsthetics, and has three combinations or variations. The patients are given a hypodermic injection beforehand. The anæsthesia is produced by nitrous oxide and oxygen, in conjunction with ether, or with C.E. mixture, or, in some cases, with chloroform. The cases in which chloroform is used in this association are only those in which the surgeon is using the diathermy cautery. I have now anæsthetized in this way 590 cases, and residents and other men I have instructed have used it in 155. Of these 745 cases, 474 have been for operations performed by Mr. Harmer, who will perhaps give his views from the surgeon's standpoint. I am much indebted to him for his patience in the early days, and for valuable suggestions.

This is not the time, especially at this late hour, to enter into details of technique. The patient is given a hypodermic, which varies in quantity and quality according to the requirements of the case. The anæsthesia is produced by nitrous oxide and oxygen, and before the surgeon begins, the gas and oxygen mixture is allowed to bubble through the ether, the C.E., or the chloroform. The anæsthesia is, therefore, deepened, so that the patient is sufficiently under for a start to be made. Afterwards it is maintained by endobuccal or endopharyngeal methods, and it is remarkable what a small quantity of either ether or C.E. is required for throat cases when good anæsthesia has been secured at first. The anæsthesia is a light one, but sufficiently deep for the ordinary operation on nose or throat. The patient should be of pink complexion all the time. Cyanosis is bad for the surgeon, as it increases the bleeding; it is also bad for the patient, and indicates that the anæsthetic is being badly given. The method is said by the patients to be pleasant: there are neither strugglings nor unpleasant sequelæ; moreover, gas-and-oxygen has no odour, and even children go off quietly under it.

With regard to gas-oxygen-chloroform, I have been forced to give this for operations on the mouth and pharynx involving the use of the diathermy cautery, because if ether is used, a little accidental sparking will cause the ether to burst into flame. The endopharyngeal method is of very great value, and in some cases one can carry on the anæsthesia

with practically no chloroform at all. Last week, when Mr. Harmer was conducting a diathermy operation, I thought the patient was too lightly under, and I turned on the chloroform. This made the patient cough, therefore I continued with gas and oxygen alone, with an excellent result.

As I am not aware of this method having been used before, I thought this would be a fitting occasion to bring it before my colleagues, so that you may know there are some men in London trying to improve anæsthesia, not only for general surgery but also for nose and throat work.

Mr. Harold Barwell.

I have not anything to say which is new, but I think it is the function, and the duty, of those attending such a meeting as this, to state their beliefs, and relate their practice, so that the general practice may be known.

I think ether, administered by the modern methods and preceded by an injection of atropine is so convenient to the operator, and so safe, that probably it is, on the whole, the best anæsthetic to give in most nose and throat operations, especially when there is no obstruction to the air passages. But I do not restrict my anæsthetist to the use of pure ether: he has liberty to—and I think he sometimes does—add a small quantity of chloroform, in quantity according to the idiosyncrasy of the patient and the need of the case. I agree that for such operations as septal resection the intratracheal insufflation is inconvenient; we found the spraying back of blood through the air passages was a nuisance. I think the post-nasal plug, in resection, is an embarrassment to the surgeon, and these plugs are unnecessary if an adequate septal ischæmia is secured beforehand. I get this by applying a small amount of cocaine fifteen minutes before the operation, and injecting $\frac{1}{10000}$ to $\frac{1}{15000}$ strength of adrenalin submucously before the patient is anæsthetized. It has been proved at a meeting of this Section that the injection of even minute quantities of adrenalin while the patient was under chloroform anæsthesia was excessively dangerous, a fact which ought to be widely known. But the injection of adrenalin before giving the general anæsthetic is safe. You cannot always pack a septum satisfactorily behind a severe deviation, but adrenalin injected submucously passes back quite well.

I should not like it to go out as the unquestioned view that the only

position of the patient in dissection of tonsils is that which Dr. Rood illustrated on the screen: with head extended over a pillow under the shoulders. I think this hyperextension of the head tends to cause some discomfort and congestion, and it makes the removal of adenoids more difficult owing to increased convexity of the cervical spine. I like to do my dissections of tonsils and such operations as those for suppurating maxillary antrum, with the patient on his side: it is not quite so comfortable for the surgeon, but it is more so for the patient, and it is quite as safe, as I have never seen a case in which pneumonia supervened in this posture. The same applies also to operations for malignant disease of the tonsils. I know that this is not the position most often adopted, but I am stating what I like, and it is the fact that the blood runs into the cheek, whence it can be removed, and there is no embarrassment from blood getting into the air passages.

Dr. Brown Kelly (Glasgow).

I divide my cases into two classes: those requiring prolonged, and those requiring brief, anæsthesia. For the former, I leave the choice of anæsthetic to the anæsthetist, and chloroform is usually preferred. For brief anæsthesia I use bromide of ethyl. During the last twenty-five years my assistants and I have employed it in many thousands of cases. A pure preparation is supplied by Duncan and Flockhart, in ordinary bottles, no special tubes being required. The child and operator sit facing each other. A gag is inserted, which with a single ply of lint over the nose and mouth, is kept in position by the anæsthetist, who stands behind the patient. A couple of drachms of bromide of ethyl is measured out and a little is gradually poured upon the lint. A child of five or six years requires about a drachm; one of ten to twelve years usually needs about 2 dr. If the patient is older than this, I prefer one of the other anæsthetics. Induction only takes about a minute. Anæsthesia lasts from one to three minutes. The patient comes to quickly and can walk out of the room. The advantages of the method are: No special apparatus is needed; the position of the patient allows of our working with the usual light and forehead mirror; narcosis is rapidly produced; consciousness quickly regained; and, as a rule, there are no troublesome after-effects. On the other hand, the period of anæsthesia is limited, but ample for the removal of tonsils and adenoids in children.

Dr. Mennell.

Dr. Rood has given us some revolutionary ideas, revolutionary because of the depth of the anæsthesia. It was as a result of one of my conversations with him that I adopted it for throat work three years ago. I find that surgeons need educating up to this deep anæsthesia. The induction, as Dr. Rood described it, is easy, but I prefer to start with gas-and-ether sequence, and then go on to open ether. I think this form is quite safe so long as no chloroform is used, though I admit it is very tempting to add a small quantity of chloroform. For the deep anæsthesia described, I think chloroform added would be dangerous.

The position which he describes I consider essential, because of the wide abduction of the cords, which only takes place when the anæsthesia is deep: the pink colour only comes when the patient is deeply under, when the pupils are widely dilated, and are inactive to light.

Reference has been made to ethyl chloride, and to its safety. I can refer Dr. William Hill to two deaths which have occurred in one hospital this year under ethyl chloride.

With regard to the preference for chloroform in Scotland, I believe a notice is put up in several operating theatres in the Royal Infirmary, Edinburgh, that no chloroform is to be used in the theatre without special permission.

Dr. Cecil Hughes.

Dr. Rood said, I think, that there were two stages of chloroform anæsthesia under which throat operations were performed: Light anæsthesia when the patient might cough, &c., and deep, when the patient would be without reflexes, and in a dangerous degree of narcosis. But it is possible to obtain a degree of chloroform anæsthesia which is *not* dangerously deep, especially after inducing with ether, which can satisfy the surgical requirements of tonsillectomy, and which can be maintained without depressing the circulation, or making delayed hæmorrhage likely to occur. No unpleasant taste or odour of anæsthetic remains for hours with the patient, comparable to that following the ether saturation necessary to produce a prolonged deep anæsthesia.

Dr. Dundas Grant.

I would support what has been said in favour of chloride of ethyl for tonsil and adenoid operations in children; I have had it administered hundreds of times without danger. I will not say I would not sometimes have wished the duration of the anæsthesia to have been a little longer, but by working with anæsthetists to whom my methods are well known, we manage in a way which is eminently satisfactory. I do not usually allow patients to walk out of the room in the way Dr. Brown Kelly mentioned, but very nearly as soon, because of the quick recovery. I hold the tonsillotome in the right hand, and enucleate the right tonsil by Whillis's method, and the left tonsil by what was originally described as Sluder's method. If the patient is lying down on the table and having chloroform, it is important to remove the lower tonsil first, otherwise the blood from the throat slips on to it, making complete enucleation with the guillotine difficult. These are trivial details, but it is attention to them which makes the operation satisfactory. Of all the methods, gas-and-oxygen seems to be the combination which is followed by the most comfortable recovery. Those who use ether know that though the going-over is pleasant enough the after-vomiting is not a matter of unmixed enjoyment, in this respect analogous to getting drunk and getting sober.

Dr. McCardie (Birmingham).

I wish that Dr. Rood had said something about methods of maintaining anæsthesia. I believe that he is quite right in inducing very deep ether anæsthesia to begin with.

With regard to the maintenance of anæsthesia during intranasal operations I would like to describe a very simple and effective method devised by Mr. Seymour Jones, of Birmingham. It has been used for many hundreds of his operations on the nose and especially for septal operations. The device may be described as a "gauze curtain mouth mask," which I show. The procedure is as follows: After the patient has had a few breaths of ether a local anæsthetic containing adrenalin is injected into the nose and anæsthesia is deepened. Then a mouth-tube, usually of stiff plain rubber, with or without a gag, is inserted; a towel is firmly fixed round the patient's head and brought well over the cheeks; the mouth area is vaselined, and this "gauze curtain" strapped across the

upper lip and on to the towel. The curtain falls over the mouth. Then chloroform or a mixture of chloroform and ether is dropped on to the gauze and anæsthesia is easily maintained. The advantages of this method are: (1) Simplicity; no special or pumping apparatus is required. (2) The gauze screen prevents any blood, &c., from being coughed or blown in the surgeon's face.

In nasal work I nearly always maintain a light chloroform anæsthesia, particularly if adrenalin has been injected. This practice is contrary to Goodman Levy's statement that only deep chloroform anæsthesia is safe. I have seen two or three cases in which danger arose during deep chloroformization after adrenalin had been injected.

Speaking generally, I believe that for operations in or about the mouth, throat, or nose, it is better, if possible, to do without any pumping apparatus. In most cases a simple rubber or metal mouth tube (with or without packing of the mouth cavity), or nasal catheters, will suffice, and anæsthesia can be kept much lighter if a preliminary spray of 10 per cent. cocaine with some adrenalin be used to abolish pharyngeal and nasal reflexes before insertion of the tubes. I have proved the value of these simple tubal and curtain methods in many plastic operations on the jaw and face of soldiers as well as in ordinary civil work.

With regard to the nasal catheters I prefer those of the type of Crile's tubes, which have a bore of 18 English (30 French) with lateral as well as end openings. These tubes are passed down to the epiglottis. Two or three inches of each project from the nose, and over these ends a mask or gauze is held on which chloroform usually is dropped. I prefer chloroform to ether, because the deep ether breathing through comparatively narrow tubes causes strain on respiration and secondarily on the heart, which may tell against the patient in a long operation; whereas the quiet breathing produced by chloroform can be efficiently carried out without any strain on respiration. It is not advisable to use long tubes like Crile's for the same reason of increased strain on respiration.

It is some twenty years since ethyl chloride was, by mistake, used as an anæsthetic in Austria.[1] In 1901 I first administered it in this country by the semi-open method and inhaler used in Innsbrück, but I soon changed over to the closed method of administration with an Ormsby inhaler, and used it as a routine for adenoid and tonsil operations

[1] Papers in *Lancet*, 1901, i, p. 698; 1903, i, pp. 952, 1198; 1905, ii, pp. 1023, 1922; and *Trans. Soc. Anæsthetists*, 1904-5, viii, pp. 121-149.

and for certain difficult dental and other short cases. We have never had a death from it in Birmingham, although in the Ear and Throat Hospital alone it has been used for at least 20,000 short throat operations. I attribute this immunity from death partly to the rapid volatilization of the drug, and partly to the fact that operation had been done there with the patient placed in a moderate Trendelenburg posture.

As regards danger, it must never be forgotten that an anæsthetic which contains a halogen element is much more dangerous than nitrous oxide or ether. The safety of ethyl chloride can be greatly increased by the addition, which will be practically unnoticed by the patient, of 1 or 2 dr. of ether to the dose in the bag.

Mr. Douglas Harmer.

Mr. Boyle's method is practically a gas-and-oxygen method, and I regard it as a very valuable proceeding. I do not think it is possible, at this late hour, to do justice to all the points. I should have thought it would have been better to bring this up as a subject for special discussion at a future meeting.

Dr. Hugh Phillips.

I recently had the pleasure of seeing Dr. Boyle use his method, and it struck me as being extraordinarily fine, with one exception. I stayed in front of the patient and watched the pharynx. Many throat surgeons require that the pharynx shall be quite immobile, and this immobility cannot, I think, be secured by this method. It fulfilled all that Mr. Harmer required, but there are other operators for whom it would not be as good.

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Section of Balneology and Climatology.

President—Dr. Neville Wood.

The Rôle of Health Resorts in the State and their Use as "Temples of Aesculapius" for the Diagnosis and Treatment of Chronic and Functional Diseases.[1]

By F. Parkes Weber, M.D.

In regard to the rôle of health resorts there is both their use for the poorer classes and their use for the well-to-do and leisured classes to be considered. Whether there should or should not be any differences in the employment of health resorts for the two classes I cannot discuss here; the difference exists, and must be accepted for practical purposes; like our Vice-President, I do not intend to "tilt at windmills." In the present paper it is in respect of the well-to-do classes that I shall deal with the subject, and as far as possible I will avoid discussing the important points which have already been clearly and ably put forward by Dr. C. W. Buckley, in his paper before this Section on March 6 of the present year.[2] Some of my ideas may perhaps be at once relegated by my colleagues to the realm of any good or bad "Upoté"[3] or "Utopia."

[1] At a meeting of the Section, held October 30, 1919.

[2] C. W. Buckley, "The Health Resorts and the State," *Brit. Med. Journ.*, 1919, i, p. 517.

[3] A "Upoté," or, as the learned Erasmus suggested, an "Oudépoté," means a *never never land*, practically of course the same as a *nowhere land*—i.e., "Utopia" of Sir Thomas More, "Erewhon" of Samuel Butler.

(A) Ordinary Uses of Health Resorts.

An almost endless amount has been written concerning the natural indications for the various climatic and mineral water health resorts in different diseases and morbid conditions. It would be waste of time to go over all this again here—there are many text-books which constitute a store-house of information on the subject. But I believe that in the future it will still largely depend on the local doctors as to what classes of cases are specially attracted to any particular health resort. It is the doctor that knows most about the class of case in question (including the natural course of the disorder and its symptoms), who can likewise make use of the special local resources of the health resort to the best advantage for the individual patient. Naturally, therefore, in my opinion, the progressive part which can be played by health resorts in the future in the social life of a great country will depend largely on the general and special medical ability and experience of the local doctors.

(B) Health Resorts as Places of Chronic or Permanent Residence for Invalids.

In pulmonary tuberculosis health resorts are often used as places of residence for the patients, and so also in the case of scrofulous children. Health resorts should also more frequently serve as *places of residence* for chronic bronchitics, the subjects of chronic rheumatoid arthritis, and chronic invalids of various other categories. Not only will most chronic invalids be better off, from the medical point of view, at suitable health resorts (if my ideas of future development are correct), where they could be always under the supervision of doctors specially adapted for helping them, but London and other large cities would be probably benefited by their absence. . Some chronic invalids might have even two or more health resort homes, residing at one during one season of the year and at another during another season.

Someone may object that chronic invalids often "love to be in London," and that what I advocate would "break up family ties." If health resorts develop in the direction which I indicate, invalids (those who *cannot help* being invalids) will prefer them to London. At health resorts more than at any other places invalids ought to be able to live the fullest life of which they are capable—and I do not forget their mental life. Many invalids are mentally gifted beyond the average.

There would be literary, artistic, and scientific "circles," and doubtless cultivated ladies would be found willing and capable of holding "salons," at which the members of such circles would be specially welcomed. The "transactions" of the local societies would often rival those of the learned academies of large cities.

But not all at such health resorts would be invalids. The sons and daughters (whether "dutiful" or not) and other healthy relatives and friends would frequently pay visits to the invalid colony—and many healthy persons (apart from doctors and those connected with "business" and trade matters) would carry on the main work of their life there, attracted either by the beauty of the surroundings or wishing to live near invalid relatives or friends. Such places would also always remain suitable resorts for healthy recreation and would still be visited by the young, middle aged, and old, in need of relaxation and a holiday. For the younger ones and the healthy there would be abundant opportunities for open-air games and sport, as well as for intellectual entertainments, but for mere "loafing" on their part there would be no more encouragement than at any other places. Members of the learned professions, especially law and medicine, might well spend ample holidays at such places before it becomes too late for them. I feel convinced that many lawyers, physicians, surgeons, &c., die indirectly from overwork. They say that they "cannot" go for a long holiday, because their "practice would suffer." It might diminish to some extent, but they are often quite rich enough to afford it; even if some of their clients desert them and their professional rivals become the gainers, the maintenance of their health is much more important to them than such losses. The very clients most dependent on them ought to prefer to lose them for a time than lose them altogether. There are some, I believe, who do not admit that long-continued excess of exciting professional brain work is often a cause of relatively premature death, and I am not aware of the existence of any good statistical inquiry on the subject—for instance, a comparison of the average duration of life in doctors who have been known to take regular holidays with that in doctors (in the same kind of practice) who have practically taken no holidays at all. The latter is, I hope, a numerically fast diminishing class. These last remarks, however, are rather outside the main subject of my paper.

(C) Health Resorts as Places for the Diagnosis of Chronic and Functional Diseases.

As the doctors at different health-resorts would have extraordinary facilities for the observation of different classes of cases, certain health resorts should become admirable places for the diagnosis of special diseases. In this the doctors would naturally have all the modern laboratory and other aids to diagnosis at their service, including all kinds of blood examination, Röntgen-ray apparatus, methods for the examination of the circulatory organs, for the examination of the digestive organs, &c. At some health resorts there would be special facilities for the investigation and treatment (including psycho-analytical methods) of functional disorders of the nervous system. And, by-the-bye, the psychic factor in the treatment of ordinary cases at health resorts is often a large one (as has been pointed out and explained by various writers time after time), so that the acquisition of extra psycho-therapeutic knowledge by the local physicians would not be at all out of place.

People will, I hope, become gradually more and more inclined to avoid great cities when not *obliged* to live in them. Holidays will be largely spent at health resorts, and when one member of a family has a troublesome and somewhat obscure chronic or functional disorder, a health resort will be selected for the family holiday at which the local doctors have a great reputation for their correct diagnoses and successful therapeutical plans for the class of disorders in question.

The health resorts visited during the holidays by families will often be those where there are special facilities for having the physical and mental abilities of the younger members tested and estimated. There such questions will be answered as the following: "Is he able to play cricket and ordinary games like other boys?" "Will rowing in a race be dangerous for him?" "Is he mentally able to hold his own amongst other children of the same age?" "Ought he to be educated separately or at a special institution or can he be sent to school with other boys?" "Ought one to take special care in regard to his heart?" "Is the albuminuria in this case merely orthostatic, and, if so, is the boy or girl in other respects sound and of average physical capability?" "Is he physically up to the standard for the Army or Navy or Indian Civil Service?" At some resorts the effects of various kinds of violent physical effort would be watched by medical experts—capable of coming

to a correct conclusion as to whether the exertion is too much, or dangerous.

Chronic dyspeptic symptoms and disorders of the digestive system could be thoroughly investigated (if progress is made in the direction I indicate) at nearly all health resorts, because such troubles are very frequent and troublesome, though often not "dangerous." "What's wrong with this youth? Though tall and rather lanky, he's fairly strong in an ordinary kind of way. But he says he cannot play football in the afternoon like other boys, or only if he goes without his mid-day meal or the portion of it that he most enjoys (the fruit pie or pudding). When he plays soon after an ordinary meal, a thin tasteless fluid, like warm water, begins to pour into his mouth (reflex salivation) so quickly that he can hardly swallow it; he has a choking feeling in his throat and he begins to belch up wind like a neurotic subject of aërophagy. He has himself found out that if he then lies down flat on his back, all his discomfort rapidly disappears." Such a case could be investigated during holidays at health resorts. The Röentgen ray skiagrams would probably reveal considerable gastroptosis; owing to the gastroptosis more or less obstruction to the exit of food, together with reflex salivation, may be produced by playing football not long after a fairly copious meal.

I am afraid that these somewhat disjointed remarks form a very inadequate substitute for the address which the President has been unfortunately prevented from giving to-day owing to his illness.

Section of Balneology and Climatology.

President—Dr. Neville Wood.

The Health Resorts and the State.[1]

By C. W. Buckley, M.D. (Buxton).

The establishment of a Ministry of Public Health marks a fresh era in the history of the medical profession and demands the closest attention, criticism, and co-operation of every practising medical man. The awakening of the conscience of the State to its duties in respect of the health of the community has been a slow process, moments of wakefulness and hurried ill-digested legislation have been followed by periods of slumber, but now all the signs indicate that we are about to witness a definite attempt to grapple with the problems of the health of the nation in the right spirit, and it behoves our profession in all its branches to be in the van of reform. Too often in the past we have allowed legislation to be framed by bureaucrats and passed by a Parliament in which medicine had no representation worthy of the name. Criticism has indeed been forthcoming, but never with any organized body behind it, and too often it has been merely destructive, and not, as ought to be the case, constructive. It is therefore our urgent duty to make good the deficiencies of the past and lead the way in all future legislation having for its object the improvement of the health of the nation. We know more of the needs of the situation than any other section of the population, and if we give it our attention we can better devise schemes to meet those needs than a legislature, the majority of whose members are but lookers-on in the great war between health and disease. To them indeed, "Salus Reipublicæ suprema Lex," but we must bear as our motto that modification which was first suggested, I believe, by a Fellow of this Society and this Section, "Salus Ægrorum suprema Lex."

[1] At a meeting of the Section, held March 6, 1919.

The failure of the National Insurance scheme to satisfy either the doctors or the patients may be traced to the failure of the profession to take up the question first. We waited till our hands were forced by the introduction of the Bill, and having no alternative scheme we fell back on a line of criticism, in the main destructive, which did us no good in the eyes of the public and failed to achieve any useful result in any direction.

Such a mistake must never be allowed to occur again, but the time is short. The interest of the profession is widely aroused, meetings are held, pamphlets are written, many of them replete with the most valuable suggestions; but what is actually being done to consolidate our ranks and to bring forth practical suggestions having behind them such a weight of professional opinion that no Government could resist it?

The members of this Section have a twofold interest. Not only is it our duty to combine with the profession in general to secure the wider aims, but an opportunity offers for bringing the claims of the health resorts, the convalescent homes of the nation, prominently forward and securing that place in the early and preventive treatment in many diseases which it is our duty to fill. The Ophthalmological Society of the United Kingdom has already shown the way by forming a committee to advise the Government in matters affecting that branch of medicine and we may well follow their example either by forming a similar committee or by instructing our Council to act in a similar manner.

A Ministry of Health, whatever its initial programme, must develop in the direction of providing care and treatment for the sick—primarily the poor—but ultimately for the whole community so far as they may take advantage of it. The first steps have been taken in the providing of sanatoria for tuberculosis and in the provisions of the National Insurance Act, defective though they are in many respects and hampered and hindered by vested interests. Other national schemes of a like nature are the provisions being made for the care and efficient treatment of pensioners, village training centres, and the like.

Of the total working time lost by the population through illness, a very large proportion is due to chronic diseases of the rheumatic type which are capable of being effectively treated in our spas, and if sent for treatment early they can be restored to usefulness, but if neglected as they commonly are, they become hopeless cripples, such as may be seen in numbers in any of the great Poor Law hospitals. Of such cases I do not hesitate to say that very many might have been preserved in

full activity by proper treatment at the outset. In no class of case is there a greater prospect of saving years of usefulness to the State by early treatment, and such cases ought to be submitted to proper hospital treatment immediately they are diagnosed. But how can this be done so long as the only hospitals specializing in such work are dependent on charity and bound to limit the stay of patients by the subscribers' letter of recommendation or some other means of adjusting the supply to the demand. In the hospital of whose staff I have the honour to be a member, I have known there to be a waiting list of over 500 names, many of the unfortunate people losing time which must make all the difference to their chance of cure. The State should take over all such institutions and manage them so that no factor shall influence the stay of the patient but that of the maximum benefit having been secured. This must also be correlated with the work of the National Insurance doctors, so that the cases which come chiefly from that stratum of the population which they serve may be placed at once under the most favourable conditions. In urging this I am not advocating the claim of any particular mineral water, or indeed of any special line of treatment, but simply that such cases should be brought together under conditions of the most favourable kind as regards climate and surroundings, in fact, health resorts, and in such places must necessarily be congregated practitioners of the greatest experience in the management of these diseases. The equipment available in such places must be of every kind —balneological, dietetic, electrical, &c., and full provision for bacteriological and clinical research and X-ray examination must be provided.

Nor is it only for the rheumatic diseases that such provision need be made, nor for the poorer classes only. Sir Bertrand Dawson has pointed out in his "Addresses on the Future of the Medical Profession"[1] how great a field of usefulness exists for the health resorts among the middle classes suffering from strenuous work often under unfavourable conditions and who may be saved from breakdown by a period of rest, recreation and ordered mode of life in favourable surroundings. After discussing the possibility of properly ordered sanatoria in the neighbourhood of large towns where patients might reside for a period under a proper régime of diet, hydrotherapy, games, &c., while at the same time continuing in some measure to attend to their business, he goes on to say: "The spa is another application of the same idea except that it is on a larger scale and the patients are resting from their ordinary avocations. The proper attributes of a spa are

[1] *Brit. Med. Journ.*, 1918, ii, p. 58.

strangely misunderstood. The public visit the spa in the same spirit as Naaman visited Elisha in that they seek some magic short cut to health. I do not say that natural waters are not sometimes a useful adjunct to a spa, but they are quite subsidiary to its climate, its position, and the careful organization of its régime and environment, physical and mental. Testimony to the truth of this statement is that many of the most successful spas in Europe are those whose waters have no special qualities."

While we must join issue with Sir Bertrand in some points, holding as we do that if the properties of the mineral waters were more carefully studied by doctors before selecting a spa for their patients they would often obtain better results; the most fervid believer in the virtues of any special spring must, if he consider the matter impartially, admit, that the efficacy of the mineral water cure would be seriously diminished if taken under less favourable conditions. Further we must remember that there are many places which do not possess mineral springs, which are admirably equipped by Nature and Art for the restoration to health of the sick and ailing. I need only instance the sea-side resorts and some of those hill stations which are gradually coming into favour, and both have an equal claim with the spas to be taken into consideration as Health Resorts within the meaning of this paper.

It is not very long since a well known medical journal stated in a leading article that "hydrotherapy is now merely the luxury of the rich." The writer showed a deplorable ignorance of the splendid work which is being done by the great spa and sea bathing hospitals, work which has been brought prominently to the front by the use made of such institutions by the War Office during recent years. Nevertheless it is largely true that facilities for such treatment are restricted to the rich on the one hand and the poor on the other, while a large section of the population must forgo the advantages they might obtain by proper treatment since their income and social position exclude them from hospital treatment on charitable lines and on the other hand they cannot afford the often heavy cost of a visit to the sea or the spas. Sir Bertrand Dawson has in his mind such cases as well as others when he sketches out his idea of sanatoria, and his remedy is indicated in the following lines: "As far as realization goes it presents no insuperable difficulties. In any district it needs a far-sighted rich man and skilled advisers. Its underlying idea has been translated into effective action by the Army Medical Service under the stress of war in the shape of convalescent camps in France and at home. What the army can do in

war surely the civilian can do in peace. My profession cannot provide the rich men: it does not grow them, but it can and would provide the skilled advisers. Such a scheme needs careful planning and execution such as we alone can give. It must be a medical organization throughout."

I am afraid that the number of rich men is diminishing, and even if those available took up the matter with enthusiasm it is doubtful if they could provide an appreciable fraction of what is required, and it is not right that we should look to charity for the performance of a duty which belongs to the State. It is to the interest of the State that the fullest facilities for maintaining and regaining health should be brought within the reach of every individual, and, applying this principle to our own sphere of work and to those classes of patients which we have been discussing, it follows that some scheme must be evolved in our health resorts to bring their fullest advantages within the reach of everyone.

The time has not yet come for the discussion of details, such as whether every resort must provide for every class of patient or whether it would be better that there should be selected places wherein, by means of a State subsidy, the cost of treatment would be brought down from its present high level, and where good accommodation could be obtained under proper conditions at an equally reasonable rate. At present the first object of the administration of every resort is to promote the welfare of its own residents and ratepayers, and the welfare of the individual who goes there for his health is only an object of consideration so far as it pays to treat him well that he may return and bring others in his train. There are, of course, a few who take a broader view, but the majority determine the policy, and so long as the town has to provide all that goes to make a health resort beyond its climate and natural attractions, so long will health resorts be administered on the lines of any other trading concern.

Some form of State supervision is therefore inevitable sooner or later, and it ought to be in our own hands to decide what form it shall take. The small place must have its natural advantages, which may be far greater than those of its more prosperous rival, developed to their fullest extent for the common good. Perhaps a "cure tax" might be imposed, but this should be done by the State, and not by the town, and the funds accruing from this source should go to the development of the different places according to their need, supplemented by such further grants from the National Exchequer as might be necessary.

I have perhaps strayed into Utopia, but a Utopia which will, I firmly believe, be realized sooner or later. In the immediate present, however, there is scope and necessity for State supervision and control. The sanitary equipment of every place claiming to be a health resort must be made as perfect as possible, satisfactory water supplies ensured, and natural amenities developed. I would also like to see the medical profession in each resort organized, so that with the resources which we now have better and more scientific work might be done. There is great scope for team work, so that every case might be thoroughly dealt with as if in a modern and perfectly equipped hospital, the physician, the surgeon, the radiographer, the bacteriologist, and the pathological chemist all doing their share. The *malade imaginaire* would soon be eliminated, to the joy of the scientific practitioner, who, under present conditions, cannot always afford to be as candid as he might wish. Great discoveries would be made, I am convinced, in the beginnings of chronic disease and in its treatment, with infinite benefit to the community.

Before concluding, there is one other aspect of the matter that I must refer to, namely, the education of the medical student and the practitioner in post-graduate classes in methods of physical treatment especially hydrology and climatology, in order that an intelligent discrimination may be exercised in the selection of cases for health resort treatment, and also that those valuable hydrotherapeutic measures which are within the reach of every practitioner may be made full use of at home.

This country is far behind others in this respect both as regards spa treatment or hydrotherapy, and climatotherapy, for the therapeutic effects of which there are in many cases no satisfactory substitutes. In almost all the leading continental countries hydrology receives academic recognition and is practised in enlightened fashion in such large cities as Paris and New York. But while such practice of hydrotherapy is possible and often successful, it is there subject to the drawback that it is divorced from that strict attention to regimen which is desirable and the climate and atmosphere often leaves much to be desired. In this country we have to look for the nearest approach to official recognition of the value of health resort treatment to the fact that many Boards of Guardians and similar bodies find it economical practice to subscribe to the hospitals in the health resorts so that the sick who come under their care may take advantage of these conditions, and during the War the military authorities have found it to their

advantage to make the fullest use of the spas and seaside resorts, while the Pensions Committees are following the same lines. They recognize that the period of disability from certain maladies is notably shortened by such means and this amply justifies their action.

State recognition of the value of health resort treatment must therefore be accompanied by the establishment of systematic instruction in physical methods of treatment, by providing for courses of lectures in climatotherapy, hydrotherapy, and the use of electrical and mechano-therapeutic methods. Short courses of lectures might be given in the great mineral water hospitals on the methods of treatment in use, accompanied by clinical demonstrations of cases of a type which will be far more frequently met with by the student in his future practice than the so-called interesting cases which occupy so much of his attention in the wards of the teaching hospitals. Such a course might include a fortnight at one of the larger spas followed by a visit to two or three others of different type, for there are many small health resorts which might most advantageously be equipped with proper methods of administering to the needs of such classes of the sick as I have described and would be of greater value to the neurasthenic or the sufferer from the early manifestations of arterio-sclerosis than the fashionable spa or seaside resort. But until an enlightened Government takes the whole question in hand I am afraid that such instruction will only be sought by the few, and my own experience leads me to the belief that the great majority of our profession are woefully ignorant of the results which are to be obtained by health resort treatment, while even the doctors who practise in such places frequently fail to grasp the potency of the means which lie to their hand.

I have only touched upon many points which are worthy of fuller attention. If we do not act now by bringing all the influence we can command to bear on this and each subsequent Government in all matters touching the health of the country, we shall only have ourselves to blame if we find all the faults of the Insurance Act repeated in the formation of a State Medical Service with our profession at the mercy of a bureaucracy. Let us close our ranks therefore, shun the apathy which has so frequently been our characteristic in medical politics and remember that our duty to the State and to our calling demands all our energies and the subordinating of personal ambitions and inclinations to the common cause.

Section of Balneology and Climatology.

President—Dr. Neville Wood.

Visceral Fibrositis.[1]

By Wilfrid Edgecombe, M.D.

Fibrositis affecting the ligaments, tendons, intermuscular aponeuroses, nerve sheaths, and fibrous tissues in various parts is an exceedingly common affection. Visceral fibrositis, affecting the fibrous tissues of the internal organs, is of rare occurrence, or, if common, is not readily recognized.

Llewellyn and Basset Jones, in their monumental work on fibrositis, dismiss visceral fibrositis with a very brief account. Mention is made of chronic pharyngitis due to fibrositis of the pharyngeal muscles; of myocardial lesions due to involvement of the cardiac muscle in the same morbid process as the skeletal muscles; of ocular fibrositis involving the fibrous textures of the eye; and of affection of the fibrous tissues around the neck of the bladder. I can find no reference to fibrositis of other internal organs.

During the last few years I have met with cases that appeared to me to be suffering from fibrositis of the following organs: the breast, testicle, liver, and pancreas. All these organs contain fibrous tissue, and there is no inherent reason why fibrous tissue in these situations should not be irritated or inflamed by the same chemical or bacterial poisons as give rise to fibrositis in other parts. The following examples may be given:—

Fibrositis of the Breast.

Mrs. B., aged 47. Seen in 1917, suffering from acute fibrositis of the capsule of the right shoulder-joint. This ran an ordinary course, and under treatment by rest, hot-air baths, and, subsequently, massage and passive move-

[1] At a meeting of the Section, held March 4, 1920.

ments, cleared up in about six weeks. After it had subsided she complained of pain and swelling in the right breast: and on examination a bunch of tender nodules, about the size of an almond, was felt in the right upper quadrant. Under treatment the pain and swelling subsided, but the nodules were only slowly absorbed, traces of them being felt six months afterwards. It was thought they might be the common chronic interstitial mastitis of women about the climacteric period, and consequently a careful watch was kept over them lest any cancerous development should occur. At the expiration of twelve months they had disappeared entirely.

Miss M. C. W., aged 28, seen in June, 1917, suffering from widespread fibrositis of the shoulder-joints, scapular muscles, knees and ankles. She was under my care in a nursing home for some weeks, during which time she developed a subacute mastitis of the left breast, with swelling of the whole organ and localized tender nodules here and there in the breast tissue. With the subsidence of the systemic fibrositis the breast gradually cleared up. She remained well for a year, when she had a recurrence of fibrositis in December, 1918. She came under my care again in June, 1919, with fibrositis of the trapezius and scapular muscles of the left side. Examination of the breasts showed no trace of the nodules that were present two years previously.

Fibrositis of the Testicle.

Mr. B., aged 58, consulted me in August, 1919. He had had good health; no serious illness; but had suffered from occasional "rheumatic" manifestations from time to time during the last few years—lumbago, sciatica, neuritis of the arm. His present complaint was of pain in the right ankle, due to an acute fibrositis of the tendon sheaths over the front of the joint. During the course of treatment he developed acute pain and slight swelling of the left testicle. As far as could be determined the seat of the trouble lay in the tunica vaginalis: the epididymis was unaffected. A small effusion of fluid appeared in the sac. Under treatment the inflammation subsided in about a fortnight, along with the trouble in the ankle.

In this case gout was the probable cause of the fibrositis of ankle and testicle, though the physical signs of the ankle in no way resembled a typical gouty attack. He had never had articular gout, but his previous manifestations of fibrositis were probably of gouty origin; no other cause could be discovered.

Fibrositis of the Liver.

It is remarkable how frequently cases of fibrositis in whatever situation—lumbago, stiff neck, sciatica, peri-articular fibrositis—present on examination a swollen and tender liver. During the last nine months

I have seen at least ten cases in which this association was well marked Sometimes the tenderness is general, over the whole liver; sometimes definitely localized to the gall-bladder region. Common as is the association between fibrositis and temporary digestive disturbance, it may be said that these are ordinary examples of so-called "congested" liver, or "hepatic inadequacy." But in such the swelling and tenderness are usually transitory and disappear entirely on rectification of the digestive upset. The cases I refer to are more enduring; the swelling and tenderness remain and clear up *pari passu* with the systemic fibrositis. Sometimes local tenderness over the gall-bladder region endures for months afterwards, in the absence of any symptoms of cholecystitis or gall-stones. It may be that the seat of the trouble is in the ligamentum teres of the liver, which lies internal to the gall-bladder, separated from it by the width of the quadrate lobe. I have never been able to feel any definite fibrositic nodules in this situation.

Fibrositis of the Pancreas.

The following case of systemic fibrositis of two years' standing, involving the arms, shoulders, back and knees; accompanied by symmetrical fibrovascular growths on the fingers; followed by mastitis of both breasts; and subsequently by jaundice, ascites, and œdema of the legs, due as I believe to fibrositis of the pancreas, is unique in my experience, and is therefore related in detail.

C. M. O., male, aged 63, was brought to me by Dr. Greenwood of Ripon, on February 14, 1916. His history, taken from his diary, was as follows: He had always been a healthy man, of great activity, and a very hard worker. The subject of alopecia universalis of many years' standing, he had had no illnesses of any moment. For a year or more he had had occasional attacks of mild rheumatic pains, chiefly in the right wrist, but he took no notice of them. On March 24, 1914, after a hard day's work, he fell asleep in the train from London to Birmingham, and woke up with severe pain in both arms and shoulders, and found the elbows fixed to the sides. He went home to bed and called in his medical attendant, Dr. Clayton, of Edgbaston, where he then lived; and on the following day saw the late Sir Robert Simon in consultation, who pronounced the condition "acute neuritis." He stayed in bed a few days until the acute phase had subsided and then resumed work. A good deal of pain and stiffness of the shoulders remained, for which he had a five weeks' course of massage in April and May. In June, 1914, he was well enough to play golf for the first time for three months. Through July he was "wonderfully well," and walked a great deal at Southwold. Towards the end of August the "neuritis" returned in the shoulders, and he had another course of

massage for four weeks. He made no progress, and on October 20 went to Malvern for three weeks, during which time he got steadily worse with "very severe pains" in the hands, arms, shoulders and knees, which were swollen and tender, so that walking was a great difficulty. On November 17 he was back at Edgbaston and saw Sir Robert Simon again, who diagnosed teno-synovitis, and put him on a course of medicine. On December 4, 1914, he consulted Dr. A. P. Luff, in London, who told him he had fibrositis, and ordered him to Droitwich. Before going there he became worse, and had to spend some days in bed. From December 14, 1914, to January 13, 1915, he was at Droitwich under Dr. Foulds, where he made some improvement, but returned home still in great pain. During February and March he was at West Malvern, all the time in great pain and misery. On February 9 he went to London and saw Dr. Luff again, who put him on to iodide of potassium. By March 3 the pain was unendurable, and he went to Droitwich again to see Dr. Foulds, who stopped the iodide and advised him to return home. After this, through April and May, he gradually improved, and was able to do a moderate amount of work. On May 31, 1915, he went to Droitwich again for a further course of baths, and while there was ordered massage, but, owing to the pain, it proved intolerable, and had to be stopped. An entry in his diary at this period says, "still suffer great pain—nights awful."

On June 26 he returned to Edgbaston no better. In addition to the trouble in the shoulders and knees a further manifestation of fibrositis occurred in the form of a smart attack of lumbago, which lasted about a fortnight. Through July he gradually improved, and was able to do a fair amount of work up to September, though never entirely free from pain and disability. During July a curious symmetrical growth "like a raspberry," appeared on the inner surface of both index fingers near the base of the terminal phalanx. These were seen by Dr. Foulds and by Mr. Barling, of Birmingham, who advised treatment by radium. Under this treatment, applied by Dr. Loxton, they disappeared by the first week in December. In September, 1915, he went to Ilkley for three weeks, where he got much better, and returned to work in October. Early in November he had a relapse, and consulted Dr. Luff again, who prescribed iodide once more and a further course of massage. By November 8 he became much worse, and presently took to his bed with an obscure gastric attack, in which he "all but had jaundice"; this attack lasted about ten days. At the beginning of December he was still in much pain and was having very bad nights. On December 7 he removed to Ripon, and came under the care of Dr. Greenwood.

Through December, 1915, and January, 1916, his condition remained much the same: in continual pain day and night, in the back, shoulders, hands and knees; though by the exercise of great pluck he was able to struggle about and do a fair amount of work.

The history of his illness is given in some detail in order to accentuate the chronic nature of his trouble and the various manifestations it assumed.

When I first saw him on February 14, 1916, his condition was as follows:

He complained of severe pain in the back, shoulders and knees, which prevented him getting about except with difficulty, and which kept him awake at night. He was bent forward and could not straighten his back to the erect position. Tender nodules were to be felt in the lumbar muscles. Both shoulder-joints were fixed by fibrous ankylosis, the movement of abduction being very limited and that of rotation absent. He could not put either hand on the opposite shoulder; nor could he raise either hand to the back of the head. Any attempt at passive movement caused exquisite pain. There was some wasting of the shoulder muscles and nodular thickenings could be felt in the capsule and ligaments of the joints. The knee-joints were not swollen but moved stiffly and coarse crepitation was felt on flexion and extension.

Organically he was sound: heart, lungs and kidneys normal. The digestive system was somewhat disordered; teeth healthy and free from pyorrhœa; tongue slightly coated; appetite capricious; some flatulent distension of the abdomen; bowels fairly regular, but it was doubtful if they were sufficiently evacuated. A careful inquiry was made into his diet which, according to his statement, had never been taken into consideration in his previous treatment. It appeared that he was, as a rule, a hearty eater and was in the habit of taking a relatively large quantity of starchy food.

The following treatment was ordered: A diet of dry meals, with starch and sugar reduced to a minimum; hot water copiously an hour before meals; a morning draught of aperient sulphur water; dry hot-air baths; Swedish massage and passive movements to the affected parts; and aspirin to relieve pain.

For a fortnight little or no improvement took place. Ionization with sodium salicylate was then ordered to be applied to the shoulder-joints. By March 3 he had begun to improve, the pains in the back were better and he could hold himself erect. The shoulders remained very stiff and painful. He was then put upon atophan, 0·5 grm. four times daily; a dose of Epsom salts each morning, and a blue pill once a week; and he was enjoined to keep strictly to his diet limited in starch.

Through March, April and May, 1916, he made steady progress. The pains gradually disappeared until nothing was left but some stiffness remaining from the fibrous ankylosis of the shoulder-joints; and this had greatly diminished, the range of movement having steadily increased until he was able to use his arms with some freedom. The full mobility, however, had not returned. Through June he kept well and did his work easily; and the shoulders became sufficiently free to enable him to resume golf in a mild way.

Towards the middle of July, 1916, he had an attack of digestive disturbance, with epigastric discomfort, flatulence, furred tongue, with slight fever, and in a few days jaundice appeared. There was slight swelling of the liver and an ill-defined slight tenderness over the gall-bladder. There had been neither pain nor previous digestive disturbance suggestive of gall-stones.

At this time Dr. Greenwood noted a tender swelling beneath the nipple in the right breast plaque-like in shape, about the area of a half-crown piece and

twice as thick, with the nipple centred upon it, apparently a fibrositis of the rudimentary male breast. This gradually subsided, but was followed in about a fortnight by an exactly similar condition in the left breast, which I, too, saw and which also in course of time completely disappeared.

The abdominal condition was thought, tentatively, to be "catarrhal" jaundice, but as the jaundice showed no signs of clearing up he was sent by Dr. Greenwood, to Harrogate, on July 24, for a course of waters and liver packs. This course he began to undergo, and in a week's time the jaundice began to diminish. The abdomen, however, increased in size and ascites developed, together with œdema of the legs, scrotum and penis. The liver was a little enlarged and somewhat tender to pressure. The heart was perfectly sound, and the urine, though bile-stained, contained neither albumin nor sugar.

He was kept in bed and was seen on August 5, in consultation with Dr. Greenwood and myself, by Sir Berkeley Moynihan, who thought the condition was probably syphilitic in origin, in spite of the most careful and frank inquiry into the history of antecedent infection and the patient's strenuous denial thereof. A Wassermann reaction taken at this time proved negative.

The ascites increased rapidly and the abdomen became considerably distended; the œdema of the legs, scrotum and penis became massive, the limbs being nearly double their normal size, and the œdema extended to the abdominal wall. A course of daily mercurial inunction was given for three weeks, together with diuretics and liberal aperients. The whole time he was kept on a starch-reduced diet. In about a fortnight the jaundice gradually cleared up; bile reappeared in the fæces and disappeared from the urine; the œdema of the legs slowly diminished and the ascites subsided; until on September 4, it was noted "abdomen down to normal measurement, 30 in.; no ascites detected: œdema of legs gone."

He then went to Ilkley for a change and rapidly picked up his strength and walking powers. I saw him again on September 29, when he was free from muscular and joint pains; the movements of the shoulder-joints were almost normal; the abdomen flat; no ascites; superficial veins over the abdomen and lower ribs distended; liver not enlarged; some slight tenderness on deep pressure over the gall-bladder: no œdema of the legs; he expressed himself as feeling perfectly well and ready for work.

All through the winter of 1916 to 1917 he kept well, leading an active life and doing more than his normal work. He consulted me again in May, 1917, for an attack of dry eczema on the back, head and limbs; apparently correlated with some digestive disturbance, for his stomach was out of order, his tongue was coated and his breath was offensive. Rectification of the gastric disorder was speedily followed by the disappearance of the eczema which had previously resisted topical remedies. On inquiry I learnt that he had relaxed his diet in some degree and had been taking rather more starchy food. There had been no return of pain or fibrositis, and the shoulder-joints had regained almost the full normal range of mobility. For the first time I noted some irregularity in the action of the heart, in the form of extrasystoles, no doubt associated with the gastric irritation.

Since May, 1917, he has kept perfectly well, in full vigour of active work. Dr. Greenwood reported on December 10, 1917: "He has been very well, with the exception of a certain amount of eczema. The attacks of eczema are getting less severe and occur at longer intervals. There is no palpable enlargement of the liver and no evidence of any pancreatic disorder."

Ten days ago, on February 23, 1920, I saw him again and examined him carefully. He looked perfectly well and stated that he had never felt better in his life. Beyond recurrences of eczema on the back he had never ailed anything since his illness four years ago. The liver was normal in size; digestion good; no evidence of any visceral disease. There was very slight limitation of rotation in the shoulder-joints, from old adhesions, but no sign of fibrositis anywhere. He was leading an active life, playing golf regularly, and getting through a large amount of work.

What the actual condition was that gave rise to the syndrome of jaundice, ascites and dropsy must remain a matter of conjecture. No positive proof is possible in the absence of operative inspection and, happily, of post-mortem findings. A review of the history of fibrositis affecting various ligamentous and muscular parts, the curious symmetrical fibrovascular growths on the fingers, and the bilateral mastitis, leads one forcibly to the inference that a similar transitory inflammation must have occurred internally.

Anatomically it was possible for the symptoms to have arisen from pressure on the bile-duct or its tributaries, causing the jaundice; on the portal vein or its radicles, causing the ascites; and on the inferior vena cava, causing the massive œdema of the legs, scrotum and penis, and of the abdominal wall. The amount of this œdema was far more than I have ever seen result from the mere pressure of ascitic fluid within an abdomen; and in this case the ascites, though considerable, was not sufficient to occasion great tension within the abdominal cavity. The largest amount of fluid I have ever drawn from an abdomen is 604 oz., in a case of ascites with enormous abdominal distension but showing relatively little œdema of the legs. In my experience ascites is not necessarily associated with pronounced œdema of the lower extremities.

The only situations in which a single lesion could conceivably cause pressure on all three structures—the common bile-duct, the portal vein, and the inferior vena cava—are (*a*) in the neighbourhood of the hilus and posterior lobes of the liver; and (*b*) in the head of the pancreas. Enlargement of the posterior part of the right lobe, and of the Spigelian lobe, of the liver might compress the vena cava in the groove between the two lobes wherein the vein lies; and swelling of the liver a little

farther forward might put pressure on the common bile-duct and the portal vein at the hilus. As no great enlargement of the liver was detected by palpation, the assumption would have to made that a localized fibrositis swelling occurred in this region of the liver without involving the whole organ, a view difficult of acceptance.

On the other hand, a lesion of the head of the pancreas would readily cause obstruction of the common bile-duct and portal vein, and less readily, of the inferior vena cava. A transverse section of the body at the level of the pancreas shows the inferior vena cava to be in fairly close relationship with the head of the organ, in a position favourable for compression against the spine. Cabot, in his book on "Physical Diagnosis," says, when discussing cancer of the pancreas, "ascites and swelled legs may be produced by compression of the inferior vena cava." A swelling of the pancreas competent to exert pressure on the neighbouring structures must presumably be of a somewhat firm character; for the soft swelling of acute pancreatitis, and the subacute inflammation that occurs occasionally in mumps, do not produce pressure symptoms.

The most reasonable explanation of the lesion causing the symptoms would seem to be a fibrositis of the head of the pancreas. No swelling was detected by abdominal palpation, but the ascites and consequent distension of the abdomen rendered precise examination difficult. A singular feature was the absence of pain amounting to more than slight discomfort.

That no lethal or enduring organic disease was present is manifest from the complete recovery made and from the lapse of time since the symptoms cleared up. Syphilis, suggested by Sir Berkeley Moynihan, may be ruled out by the absence of a history and of any symptoms of this disease, and by the negative Wassermann reaction. The fact that the ascites and œdema disappeared during a course of mercurial inunction proves nothing; the result was *post* not *propter hoc*, for the systemic fibrositis and the mastitis disappeared previously without the aid of mercury.

As already said, actual proof is lacking; but the above appears to me the only reasonable explanation of the symptoms. The case is one of sufficient rarity to merit publication, which has been designedly delayed in order to make sure that complete recovery had taken place and that no recurrence ensued. An interval of four years of good health is sufficient to afford satisfaction on this head.

A point of importance lies in the dietetic treatment of the case.

My belief is that the key-note of the successful cure lay in the limitation of the carbohydrate intake of food. Broadly speaking, cases of fibrositis fall into two main groups: those due to poisons of bacterial origin, from some obvious or latent infection; and those due to poisons of chemical or metabolic origin, from errors in diet or digestive capability. It is not always possible sharply to differentiate the two. Setting aside the former group, my clinical experience with the latter is that, in the large majority of cases, errors of carbohydrate rather than of proteid metabolism are to blame. Reduction of the total intake of food, with especial limitation of the carbohydrate elements, together with free elimination *per vias naturales*, will in most cases effect a cure without the aid of drugs of doubtful utility.

The cases related above appear to me to suggest that fibrositis of certain internal organs is a definite clinical entity; and if looked for will be found of by no means infrequent occurrence.

Dr. EDGECOMBE (in reply to remarks by speakers): I have had two reasons for introducing the subject of the paper. First, to suggest that cases of visceral fibrositis are not uncommon, and to stimulate inquiry into their occurrence by members of the Section; and, secondly to elicit a satisfactory explanation of the remarkable case brought forward, tentatively, as fibrositis of the pancreas. I admit that the evidence is not conclusive and agree that the case must be looked upon as of undetermined origin if not as an unknown disease. Touching a possible bacterial origin, suggested by several speakers, no obvious source of infection could be discovered. There was no febrile reaction throughout and nothing to suggest a subacute bacterial invasion. The fæces were not examined bacteriologically for the reason that the identification of any organism as the definite infective agent among the extensive bacterial flora of the intestine is so beset with fallacies as to render it of doubtful utility. Had the case not progressed favourably further inquiry in this direction would have been made. The complete and permanent recovery militates against the theory of an infective origin.

Section of Balneology and Climatology.

President—Dr. Neville Wood.

Temperature Environment and Thermal Debility.[1]

By C. F. Sonntag, M.D.

Part I.—Temperature Environment.

Every animal, together with every tissue thereof, exhibits a characteristic series of changes when it is exposed to a gradually increasing temperature. Their activities are increased or decreased, chemical changes are induced in their protoplasm, and their colloids exhibit physical alterations. Again, in the case of the entire animal, immunity to different infections is modified.

As regards their activities there are four distinct thermal points. At the first, or *minimum point*, the animal is motionless, and apparently dead. When heat is applied it begins to exhibit movements which increase proportionately to the temperature till the *optimum point* is attained, when activities are greatest. Any further increase of temperature is followed by a diminution in activity till the *maximal point* is attained at which the creature perishes. The thermal range between optimum and maximum points is the "Dangerous Zone *of Duclaux*." In my own experiments on muscle work I found that the optimum point for the right flexor sublimis digitorum was 108° F., and above that the optimum duration fell one minute for every 2° increase in the temperature applied [1]. Sachs [2] has also shown that the same general principles hold good for the vital activities of plants. The maximum, optimum and minimum points for different animals and plants are shown in the table on p. 26.

The effects of temperature are modified by the amount of moisture present, the form in which the application is made, and the structure of

[1] At a meeting of the Section, held January 30, 1919.

the cells. *Moisture* intensifies the effects of heat and a hot bath exerts a more powerful action than radiant heat (fig. 3). Moist cold is more depressing than dry cold, and moist heat is more exciting than dry heat. Again the complex nerve cells perish at a lower temperature than the simpler bacteria. When the temperature applied is above the optimum, the changes induced in the cell are not so easily reversed by cooling, and when they are completely irreversible the cell is dead.

As regards chemical and enzymatic processes, the modifications induced by thermal applications follow the Law of Arrhenius and van't

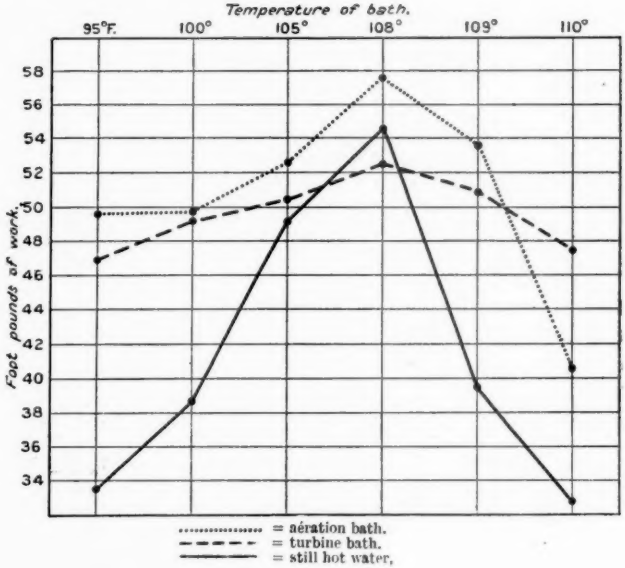


FIG. 1.

Hoff which states that "the velocity of any process is doubled or more increased by every rise in temperature of 50° F. between 32° and 104° F., but above 104° F. it diminishes."

The effect of increase in the temperature of the sea on marine animals has an important bearing on the production of thermal debility in man. The nerve cells surrounded by lymph can be likened to animals in water. When both media are heated the animal or cell cannot get sufficient oxygen from the fluids for their exalted metabolism so they are weakened or killed.

Cold-blooded animals have no heat regulating mechanism, whereas

warm-blooded animals have, and the latter becomes more perfect as we ascend the animal scale. Again it is less perfect in the young than in the adult. Consequently, baths and climates at extremes of temperature are likely to do more harm to children than adults. Edwards [3] has shown, too, how the young of carnivora and rodents, have a less perfect heat-regulating mechanism than the young of animals which are not born blind.

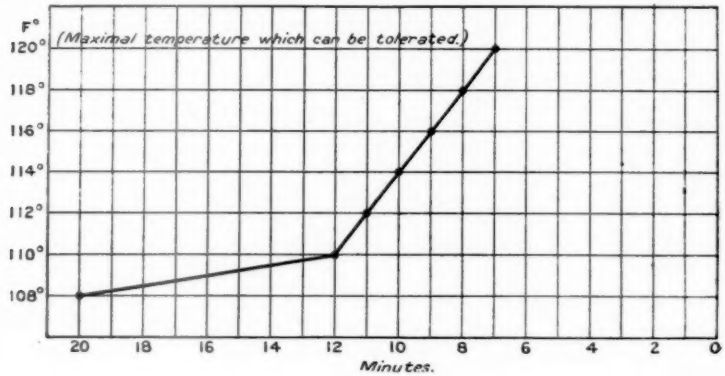


FIG. 2.

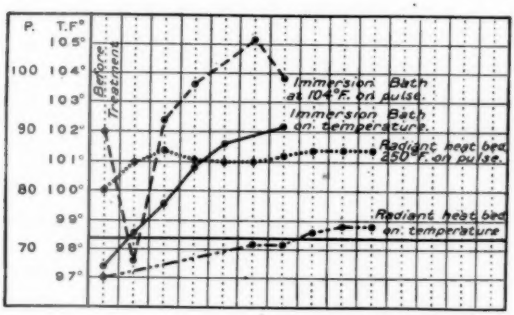


FIG. 3.

Man has the best heat-regulating mechanism and there are many records of its stability in different climates, so it is not necessary to enter into it here. If, however, any individual lowers his vitality by alcohol, or other physiological depressants, tropical climates, or occupations carried out in high temperatures will induce serious symptoms, and the commonest one is thermal debility.

TABLE SHOWING THE THERMAL POINTS FOR ANIMALS AND PLANTS.

	Minimum Degrees Fahrenheit	Optimum Degrees Fahrenheit	Maximum Degrees Fahrenheit
Bacteria of putrefaction	—	... 68·0 to 72·5 ...	—
Bacteria in the body	—	... 95·0 to 102·2 ...	—
Growth temperature for entire bacterial group	—	... 53·6 to 111·2 ...	—
Thermophilic bacteria	—	... 140·0 to 158 0 ...	—
Spirillum of cholera	60·8	... — ...	—
Culture of *Spirillum choleræ* ...	−25·6	... — ...	—
Green algae	—	... — ...	109·4
Algae in hot springs	—	... — ...	145·4 to 163·4
Some fungi	—	... — ...	113·0
Upper limit of phanerogams ...	—	... — ...	Up to 134·6
Protoplasm of cold-blooded animals	32·0	... — ...	—
Protoplasm of warm-blooded animals	59·0	... — ...	—
Centipede	− 58·0	... — ...	—
Frog	−18·0	... — ...	—
Fish	−15·0	... — ...	—
Thermal range for aquaria ...	50·0	... — ...	70
Temperature of most populated marine zone	55·0	... — ...	77

PART II.—THERMAL DEBILITY.

Thermal debility is induced sooner or later by hot climates and hot baths. The symptoms vary in degree from a feeling of enervation, such as that following a too prolonged hot bath, to collapse plus hyperpyrexia occurring in heat stroke and siriasis. The symptoms are produced by a combination of the following:—

(1) Diminished heat radiation.

(2) Diminished oxygen supply to the neurones.

(3) Toxæmia.

(4) Exalted metabolism and increased activity of the nerve cells.

The adult human skin has an area of about 15 square ft. It cools the blood by radiation and perspiration. When the body is immersed partly or entirely in a hot bath the area available for cooling the blood is diminished. The heart then beats more quickly to drive the blood through the skin which is not immersed. The larger the area immersed the greater is the increase in pulse-rate as shown in the following table:—

Part immersed	Pulse rate
Hand	76 to 80
Forearm	78 to 95
Foot	76 to 94
Leg	72 to 96
Both legs	72 to 98
Whole trunk	72 to 103

Each bath was at 106° F. for ten minutes.

The heated blood takes up less oxygen in the lungs, and less carbon dioxide in the tissues, which are, consequently, in a condition of thermal asphyxia. They must then derive much of their energy from their contained intramolecular oxygen. When that is used up the cells become adversely affected, and the nerve centres, especially the respiratory cells, suffer quickly, for they have little intramolecular oxygen.

Many experiments have shown how a deprivation of oxygen to the nerve centres is followed by serious effects, the grossest being vacuolation, cloudy swelling and extrusion of nuclei. When the asphyxia of the cells begins, toxins have an opportunity of acting, for the defending oxygen is diminished. These toxins are produced by cellular metabolism (intracellular), or invade the cells from without (extracellular). The intracellular toxins are produced in greater amounts because the heat applied excites metabolism.

It has been shown by the Mayer-Overton theory that salts will pass from liquids to fats when a mixture of lipoids and saline solution is heated. When the mixture is cooled again the salts leave the fats for the liquid. It is possible that in thermal debility toxins in the lymph pass into the lipoids of the central nervous system. It has been shown by Vincent [4] that the blood is toxic in the various conditions induced by prolonged heat. These toxins are frequently reducing in action, and still further diminish the oxygen available for the nerve cells. The heat may eventually cause the neurones to withdraw their processes in the same way as an amœba retracts its pseudopodia. The result of these actions is that the nerve cells have lowered vitality and send out fewer trophic impulses to the muscles, so lassitude and a feeling of debility result.

Thermal debility follows exposure to hot climates and to hot immersion baths, radiant heat and hot douches, but it is less likely to appear if a cold application follows the hot one. *It should therefore be a rule to apply cold after every hot bath.* It is more readily induced when illness, fatigue, improper clothing, abuse of alcohol, have depraved the physiological processes in the body. Again, it is easily produced by hot baths in the aged and infant. The symptoms come on during the bath, or a variable time afterwards. Moreover, the effects of a series of hot baths are cumulative and may make their appearance after a patient has returned home from a spa. The following symptoms appeared in nine healthy men submitted to hot baths till debility was induced. All the men had the same

symptoms, but their intensity was a personal factor, for a bath at 102° F. had a more powerful effect on one man than one at 104° F. on another. The most typical symptoms were observed in the man who was immersed in a full bath at 102° F. for sixty minutes. To make the picture as complete as possible no cold applications were made to the face and scalp beforehand, and during the bath, and there is no doubt that many of the symptoms were due to the omission of that measure. The contact of the hot water on the body surface produced a retrostasis of blood, and the internal vessels became dilated, so the patient complained of fullness in the head, and of a throbbing and a beating pain in his temples. The pulse, which was 92 per minute before immersion, slowed down to 72. In five minutes this excitement gave way to depression, and the cutaneous vessels exhibited passive dilatation. The blood increased in amount in the skin and decreased in the internal organs, which were thereby rendered anæmic. The strain on the heart was relieved, the pulse-rate was accelerated and the pulse became more compressible. Its rate increased progressively from 72 to 94 per minute, at which point it maintained a steady level for twenty-two minutes, and then it exhibited steady fluctuations with a tendency to fall off in the number of beats. The sweat glands were aroused to activity and the perspiration trickled down in streams from the forehead. The respirations became rapid and shallow, but it was difficult to count their number accurately because the body was completely immersed in the water. In addition to the alterations in the respiratory movements the patient complained of pain in the chest, and he developed bronchial catarrh. His other symptoms were a feeling of lassitude, nausea, thirst, and a sinking in the epigastrium, but he had no actual sickness. On attempting to rise out of the bath he fainted, but he was readily brought round again by affusions of cold water.

These symptoms are of a constant nature in all types of thermal debility, whether they are produced by climatic action or baths. They may not appear, however, after a single bath, or they may come on a considerable time afterwards. A course of baths, without terminal cold applications, may lead to severe thermal debility, with cardiac weakness or even dilatation, because the effects of heat are cumulative. There is, in fact, a summation of effects.

The circulatory phenomena were of constant character but of varying intensity after all baths the temperature of which lay between 98° and 105° F. (37° to 41° C.), but baths at 106° F. (41·5° C.) and at 96° F. (36° C.) produced different effects altogether. They are exhibited in the following table :—

Temperature of bath	Pulse-rate before	Maximum slowing	Maximum acceleration before level	Time of onset of fluctuation
106° F.	80	None	104	8 min.
104° F.	92	69	102	20 ,,
102° F.	92	72	94	22 ,,
100° F.	80	74	92	23 ,,
98° F.	80	78	80	27 ,,
96° F.	80	76	None	30 ,,

We see, therefore, that the pulse shows *four periods* when the temperature of the bath is 98° to 105° F. (37° to 41° C.). These are retardation, acceleration, a steady level, and a period of fluctations (figs. 4, 5, 6, 7).

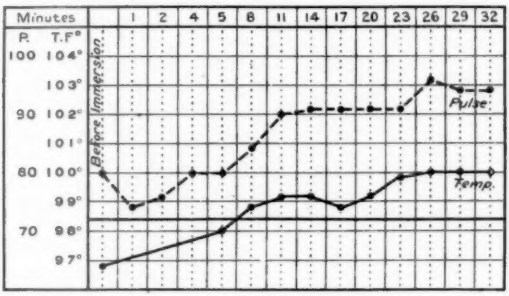


FIG. 4.

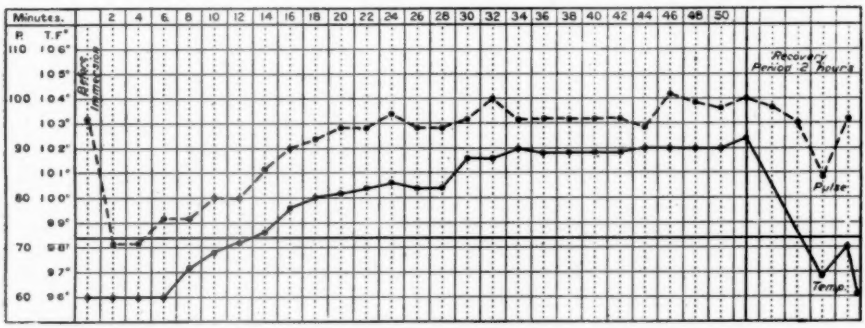


FIG. 5.

The treatment of thermal debility is mainly prophylactic. Prolonged hot applications should never be made to the whole body, but the limbs can be submitted to heat for a longer period than the trunk. In every

case the prescription must follow the physiological laws regulating the duration and temperature of baths, and, moreover, the temperature and duration must be regulated to suit each case. The individual should not indulge in the use of physiological depressants in a hot climate. Too many baths, unsuitable food, extreme fatigue, and excesses of all kinds should be shunned because they are predisposing causes of thermal debility, and the majority of the cases in the tropics occur in people who have been guilty of physiological unrighteousness. In every connexion, whether it be environment or treatment, children

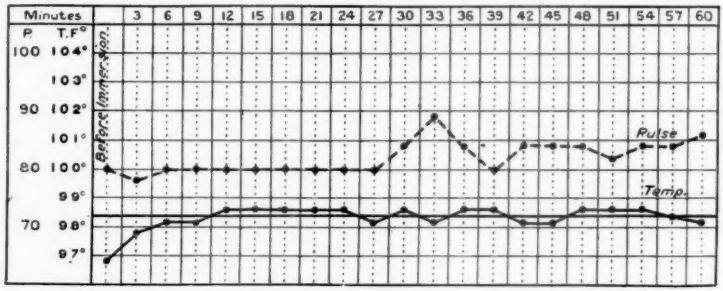


FIG. 6.

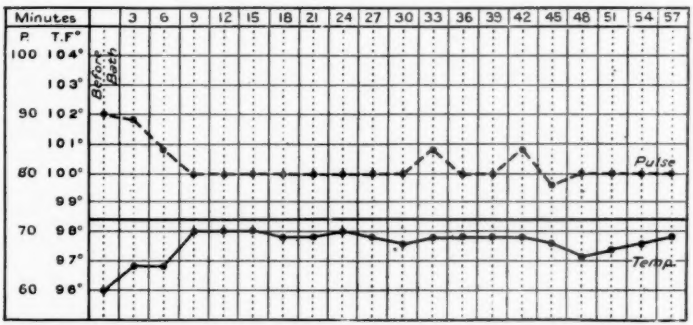


FIG. 7.

and old people should not be exposed to extremes of heat; but the aged tolerate them better than the young. Children of European parentage are sent home early to England because they would not grow up into healthy men and women otherwise. It is different, however, in the case of native children, because they are inured to heat by ancestry and

acclimatization. Every hot application must be followed by a cold one to maintain the tone of the skin and the irritability of the muscles, and to prevent the occurrence of thermal debility. It may be applied in the form of ablutions, affusions, douches or baths, and the method chosen must depend on the case. If thermal debility should appear cold must be freely applied, for it is the only rational antidote to the pathological effects of heat.

Numerous experiments have been performed on animals with the object of establishing the symptoms which appear until death takes place. Up to a point they agree with those noted by the author in the cases of the nine men aforementioned.

The effect of high temperatures on cold blooded animals has already been described, and it was shown how the temperature of 40° C. (104° F.) is lethal for the frog. Obernier, Litten, Weikart and Crawford have studied the effects of hot air for prolonged periods on the mammalia and the phenomena observed by them were identical. The temperature chosen was 40° C. The effects are due to heat accumulation, for radiation from the surface of the skin is checked. The temperature falls at first, but it soon begins to rise and is elevated 4° to 6° before death. Vascular tension falls, the heart becomes quick and irregular, and respiration is shallow and rapid, and asthenia, debility, spasms and convulsions precede death.

Perspiration is a means by which some of the accumulated heat is converted into work and used up. It permits man to tolerate higher temperatures than animals can, and if his head is not exposed to the heat he does not suffer so much. If he is exposed to too great heat for too long a time his skin becomes hot and burning, the cutaneous veins become engorged, and sweat is profusely poured out. The pulse and respiration become rapid and feeble, and headache, giddiness and asthenia indicate thermal debility of a severe degree.

The effects of climates have had a marked effect on the distribution of animal and vegetable life on the earth, and have had a profound influence on the distribution and habits of the different races of mankind. It is unnecessary to enter into details here, but the reader is referred to the works of Huggard [5] and Geikie [6] for the excellent accounts of these effects.

REFERENCES.

The following list contains the works referred to in the text, and gives a few books which enter into the question of the biological effects of heat and cold.

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[3] EDWARDS. "Encyclopædia of Anatomy and Physiology," ii.
[4] VINCENT. *See* HUGGARD.
[5] HUGGARD. "A Handbook of Climatic Treatment."
[6] GEIKIE, A. "Text-book of Geology," London, 1885.
[7] SPALLANZANI, L. "Dissertations relative to the Natural History of Animals and Plants," Translated, London, 1784.
[8] LOEB, J. "The Dynamics of Living Matter."
[9] EDGE and KROGH. *Rev. Hydrobiol.*, 1914, 48-55.
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Section of Balneology and Climatology.

President—Dr. NEVILLE WOOD.

Discussion on Dr. Ferreyrolles's Paper[1] on "Immunity and Mineral Water Treatment."[2]

Dr. MAHOMED: I regard Dr. Ferreyrolles's observations as most interesting if not epoch-making. I have been practising sea-water infiltrations for some time, and on reading his paper it seemed desirable to ascertain if leucocytosis followed them. For sea-water must be regarded to all intents as a mineralized spring. It maintains the same salinity and the same specific gravity in all oceans, it possesses radio-activity and a colloid content, exactly as springs do. I therefore arranged with Dr. A. C. Coles, whose observations on the blood are well known, to examine some of my patients both before and after treatment. Those observations I will refer to a little more fully in a paper which I have offered to this Section. I will only say at present that Dr. Coles finds a very slight leucocytosis to follow a course of injections, but he finds an increase in eosinophils—not very slight—to be produced. I think a good deal depends on the number of hours (or days) between an injection and a blood count.

Dr. F. P. NUNNELEY (Llandrindod Wells): I must apologize to the Section for the disjointed and scrappy nature of the remarks I have to make on Dr. Ferreyrolles's valuable and interesting paper. I have had very little spare time in which to consider it, and I am merely giving expression to the doubts and difficulties which it has raised in my mind, in the hope of getting some of them dispersed by other and more learned members of the Section. The Choussy-Perrière water contains an appreciable quantity of arsenic. It seems, therefore, to be at any rate possible that the immunity against micro-organisms produced by injection of Choussy-Perrière water may be due rather to the direct action of the arsenic itself than to an active immunity

[1] Printed in the last volume of the *Proceedings*; see *Proceedings*, 1919, xii (Sect. Balneo. and Climat.), pp. 1-12.

[2] At a meeting of the Section, held March 4, 1920.

akin to that produced by vaccines. The following considerations are in favour of this view :—

(1) The administration of arsenic is followed by leucocytosis.

(2) Tolerance to arsenic is, as a rule, easily acquired. Leucocytosis immunity to Choussy-Perrière water is also acquired.

(3) Repeated doses of arsenic are cumulative in their effects. Dr. Dapeyroux says that "the longer the period of the action of the water is protracted the longer does the effect of small doses persist"—i.e., the effects of repeated doses of the water are cumulative.

(4) The effects seem to be the same, whether the water is injected into the peritoneum of rabbits or directly into the marginal vein of the ear. If the process were one of active immunity, similar to that produced by vaccines, one would expect the peritoneal injection to be efficacious, and the injection direct into the circulation to have little or no effect.

(5) After the injection of Choussy-Perrière water, it is said that the animal "manifests principally lesions of the respiratory mucous membrane and of the skin, the signs of which are definite dyspnœa and itching." This is compatible with arsenical poisoning.

Against the view that the immunity is caused by the direct action of the arsenic, there are the following points :—

(1) When taken by the mouth, the Choussy-Perrière waters do not confer any appreciable immunity. As arsenic passes freely through the intestinal mucous membrane into the blood, one would expect the effects to be the same, whether it was administered by the mouth or by injection.

(2) The experiments of M. Bellin, who, by injections of carbonate of soda, rendered rabbits immune to injections of the virus of rabies and cultures of streptococci, *Bacillus coli* and hen cholera. In view of the astonishing results produced by small doses of a solution of washing soda, one is inclined to wonder what latent powers may not be concealed in the different varieties of tap water.

Dr. EDGECOMBE: On reading the paper I am frankly amazed at the results stated and if they should be corroborated by further research the work can only be described as epoch-making and of the utmost importance. The paper is open to criticism on many points, notably the want of precision in the statement of control experiments. Pending further information my attitude is one of scepticism. It is important to determine whether any of the waters of our home spas have similar powers of inducing leucocytosis, immunity, and disanaphylaxis; and this should be investigated by members of the Section. Owing to the lack of facilities for animal experimentation in this country a complete inquiry on the spot is fraught with difficulties. To send the water to a laboratory at a distance introduces a variable element. The production or non-production of leucocytosis, however, can easily be determined at each Spa by intravenous injection—into the human subject—of selected mineral waters, after bacteriological examination to determine their

sterility. Sterilization by heat is open to objection as possibly inducing immensurable changes. Whether the leucocytosis stands, as is suggested in the paper, in causal relationship to the immunity produced is debatable. If it were found that leucocytosis followed injection of any of the home mineral waters further inquiry could then be made as to the power of the waters to confer immunity.

Dr. C. F. SONNTAG: I have read Dr. Ferreyrolles's paper with great pleasure and interest, and I trust that it will stimulate investigators in this country to do work on similar lines. It commences a new form of research work in the field of crenotherapy, but I think its value lies more in what it suggests, than in what it contains. It is only the trained laboratory worker, however, who will realize the potentialities. There are a few criticisms which I should like to make:—

In the first place, I think it is much to be regretted that the paper is practically limited to a consideration of the arsenical waters, for we have none of them in this country. Consequently, we are unable to confirm or disprove Dr. Ferreyrolles's statements. Also, although he has given the strength of arsenic, and mentioned that it is combined with colloids, he has practically ignored it in the paper. Many of the actions which he describes are, moreover, very similar to those mentioned in Ehrlich and Hata's "Chemotherapy of the Spirilloses"; again, the precautions which he lays down in connexion with studies of immunity set up by medicinal waters are identical with those emphasized by Ehrlich and Hata. I would recommend members of this Section to read that book in conjunction with Dr. Ferreyrolles's paper.

Secondly, it is well known that several mineral salts will induce the body to develop immunity against them, and Dr. Ferreyrolles rightly points out that it may be necessary to increase the strength of successive doses of the water for this same reason. He does not, however, devote sufficient space to this aspect of the subject.

Thirdly, more details, or rather clearer descriptions, of the meaning of the term "control animals" should be given. When a simple experiment, such as the effects of a single injection of mineral water, is described, the nature of the control is easily understood. When, on the other hand, the effects of a series of different kinds of injections are mentioned, the meaning of the word control is not at all clear.

Fourthly, the large and important subjects of agglutination and the opsonic index, as modified by injections of mineral waters, are entirely omitted.

Fifthly, I am afraid Dr. Ferreyrolles's statements about the disanaphylactizing powers of mineral waters will be strongly opposed by bacteriologists. Personally, I cannot understand how they can possibly possess such a quality, and there is nothing in the paper in the form of any rational explanation. If Dr. Ferreyrolles sends us further reports of his researches, I would suggest that he give us more particulars about this effect.

Lastly, Dr. Ferreyrolles has given us insufficient data as regards his

technique and precautionary measures, and the following questions suggest themselves to my mind:—

(1) Is the mineral water sterile in the spring?

(2) Has he done control experiments on human beings?

(3) Did he sterilize the skins of the animals, and, if so, how did he do it?

There are many other problems, but it would occupy much space and time to enter into them now. Much work must, however, be done before we can really agree with Dr. Ferreyrolles's statements.

Dr. PERCY LEWIS: These observations made by Dr. Ferreyrolles, if correct, are of great importance. I should point out, however, that leucocytosis does not necessarily raise the opsonic index, since there are certain substances which raise the one and not the other. There is urgent need for further experiments and observations for the purpose of comparing the differences, if any, between the ingestion and injection of the various waters.

Dr. FERREYROLLES (reply, communicated in writing): My communication which has now been discussed at some length was, purposely, nothing but a rapid survey of the subject, abbreviated so as not to engage your attention too closely. In answer to Dr. Sonntag, who desires a more detailed *exposé* of my experiments, I refer him to the paper I have published on the biophysical qualities of the Bourboule waters and to other previous publications. But, as already pointed out, many points require further investigation. Since this paper has been published, many researches of a similar kind have been carried out in France, and I can quite easily answer the criticisms which some of the speakers have put forward in the course of that discussion. I do not believe, as Dr. Nunneley does, that the results obtained with the Choussy-Perrière water in regard to immunity and anaphylaxis are solely due to the presence of arsenic in the Bourboule waters. First, because the quantity of injected fluid does not contain more than seven hundredths of a milligram of arsenic; if the results were, as Dr. Sonntag assumes them to be, similar to those mentioned by Hata and Ehrlich, it would be proved (and besides Richet stated so) that in ordinary practice doses of arsenic are used which are much too strong, and our colleagues the homeopaths would be fully justified to make use of dilutions to which the present theories of biophysics are far from denying any therapeutic action, but I repeat that through its arsenic the Bourboule water has, without doubt, a disanaphylactizing action on lesions localized on the skin and mucous membranes. Secondly, and chiefly because a whole series of researches have been undertaken in France under the direction of Dr. Billard, and results similar to those I have published here have also been obtained through the water of certain springs of Royat (St. Mart and Eugenie springs), by Mougest, of Vichy (Chomel Spring), by Billard and Grellety; negative results with water that has been transported have been published by Messrs. Chassevent, Galup, Poiret, Delpech relating to the Mont-Dore, Luxeuil and Thonon waters. Dr. Edgecombe will no doubt modify his sceptical attitude. The bacteriologists

will, doubtless, reserve judgment, as Dr. Sonntag says, as to the possibility of bicarbonate waters protecting one against the real anaphylactic shock. But first of all it will be necessary to come to an agreement about the meaning of anaphylaxis. Is anaphylactic shock, according to Richet, a phenomenon of intoxication? Or is it, according to Besredka and Doerr, of a physical nature? That is the reason why I shall refrain from trying to explain the mechanism of that biological property of mineral waters. I shall simply draw your attention to the fact that mineral waters possess, when used in experiments on sensitized animals, a biological property entirely different from the property of artificial solutions, just as particular properties have been discovered in them that always differ from those of the artificial solutions when their biophysical properties have been investigated. Apart from the vaccine injections of small doses of antigen investigated by Besredka, different substances have been extolled as prophylactic against anaphylactic shock; but their action, in Richet's own words, "does not seem to be very great." NACl-$CACl_2$-$BACl_2$, ether, alcohol in strong doses, chlorure of calcium, &c. Well, there is, indeed, an enormous difference between the insufficiency of these chemical methods and the complete and perfect results obtained through mineral waters. The Société d'Hydrologie Médicale de Paris has just defined the study of that question as regards French mineral waters; it has put forward a scheme which will be followed out by the experimenters of every station. This scheme I shall be pleased to place at your disposal; it will give me great satisfaction if we can in this way assist the development of crenotherapy in England. I shall be pleased to communicate these complete results to one of your approaching meetings, if it is thought they will be of sufficient interest to your Section.

Section of Balneology and Climatology.

President—Dr. NEVILLE WOOD.

DISCUSSION ON THE MERITS AND DEFECTS OF BRITISH HEALTH RESORTS.[1]

Dr. NEVILLE WOOD (London).

FINDING it impossible to undertake the task of composing an exhaustive address I have been compelled to adopt the much easier plan of opening a discussion. Further, to lighten my load and in the belief that if this, our conference, is to prove of practical value we must consider our subject from every point of view, I propose to reproduce for your consideration a synopsis of opinions on British health resorts, expressed by members of the general community—an assessment, in fact, by patients and their friends of the goods which are offered for their acceptance.

I collected and noted down these opinions last summer during a prolonged stay at some of our best known health resorts, and I can warrant that they are views largely held by well travelled, influential and intelligent laymen.

Very little adverse criticism was levelled against our seaside resorts, which seemed to be regarded as satisfactory places for holiday making, while I do not remember meeting with anyone able to discuss intelligently the advantages and disadvantages of inland climatic stations for the restoration of health.

With the spas it was quite different, and I was surprised at the knowledge displayed even in matters of intimate detail. There was undoubtedly a strong consensus of opinion that, in general terms, foreign spas lead the van, while those of our own country lag behind.

[1] At meetings of the Section, held January 29 and February 12, 1920.

It was urged that although these places exploit their mineral waters, nevertheless (with but few exceptions) little or nothing is done to render British spas attractive to visitors upon their first arrival, this being specially in evidence in the neighbourhood of railway stations. The tree-planted promenades leading to the mineral springs in Continental spas are seldom found at the English health resorts.

Insufficiency and lack of variety in the entertainment provided at our spas constitute an almost universal subject of complaint. But such games as cricket and lawn tennis have only a limited number of enthusiasts at health resorts; tennis, unless played by champions, soon palls on those who are watching the game; football and hockey are not played during the spa season, and lacrosse, though exciting to witness, is not in fashion. Open-air music of the pseudo-classic stamp can only appeal to the very few, but folk-dancing competitions form an interesting and amusing spectacle, and are well worth introduction into spa gardens.

Concerning indoor amusement I received no suggestions, and have none to offer, except that whenever health seekers are crowded into halls impossible of adequate ventilation by currents of air an efficient ozone generator should be installed.

Reference has just been made to indoor air, but the freshness of the air even out of doors is threatened at some of our spas. It was pointed out to me that on the outskirts of one which happens to derive its chief fame from the reputed purity of the air, houses are in course of erection in grounds of insufficient area, while the new streets are unduly narrow and faulty in direction. It seems certain, therefore, that in the not distant future there will be a nearly solid barrier of masonry which will prevent the fresh country air from reaching the site of the springs in the centre of the town, which must ever remain the most important zone for invalids.

It is perhaps remarkable that none of the critics who, as I remark elsewhere, showed such a proclivity towards fault-finding, ever complained of inadequacy in the balneary equipment of British spas. Some, on the contrary, made merry over the huge number of appliances, all of foreign origin, enumerated in various spa pamphlets, and it was suggested that no properly docile patient was permitted to consider himself cured until he had worked through the whole list.

As might be expected, much fault was found with individual medical men in residence at the spas, but no doctor who has ever practised there or elsewhere can wholly have escaped this trouble, and probably the

larger his practice the more is he likely to suffer from this annoyance. Be that as it may, I have no intention of making myself the mouthpiece of criticisms levelled against men who have no opportunity for reply.

It can do no harm, however, to mention the existence of patients who were grievously disappointed on finding their own enthusiastic belief in the specific virtues of the waters of a locality not fully endorsed by the local practitioners, while it was also alleged that as a consequence of this lack of confidence the spa doctors show a want of precision in the fixing of indications, and couple with this uncertainty and timidity in dosage. These detailed complaints were made by patients suffering from chronic maladies who formerly had made yearly visits to Continental spas, and during the war were sent to British spas with presumably similar waters.

To sum up the numerous complaints I have heard and have had to repeat, some may be rated as wholly frivolous, and hence unworthy of further notice, while others no doubt point to the need for reform. In which category each should be grouped I leave to your judgment, but I hold strongly that none should be passed over in absolute silence, for however ill-founded a complaint may be harm is done by its frequent repetition, and I suggest that the proper remedy is to be found in judiciously conducted propaganda. Where reform is called for the remedy is obvious, but not always its mode of application.

In the whole conduct of my inquiry I was surprised by the apparent zest with which heavy indictments were drawn, and by the circumstance that few speakers found anything material to place on the other side of the account. Educated Britons are not malevolent by nature or by training; nor are they extremely prejudiced. They are, however, notoriously illogical.

These considerations led me to wonder whether some of the fault-finding might not be the expression of a vague and deeper discontent —one which the complainants had not succeeded in formulating even for themselves, and whether, if all they asked for were granted, full satisfaction would result. This led me to think again of the observation made by a very shrewd critic in a paper read not very long ago before this Section: "Somehow British spas seem to strike the wrong note."

It would be instructive to hear your frank and outspoken comments on that remark. Possibly its originator may be willing this afternoon to explain its full esoteric significance.

Dr. C. F. SONNTAG (London).

With the exception of the work of Bain and Edgecombe, of Harrogate, scientific research at British health resorts is practically non-existent. Its place is taken by a persuasive empiricism, and it is only when the claims made are submitted to experimental investigation that the absurdity of some of them becomes apparent. It is this empiricism which causes the profession to have a poor opinion of the so-called actions of our medicinal waters.

The conditions necessary for first-class research are absent from most of the spas. Few of their physicians have had any laboratory training beyond elementary requirements, and properly-equipped laboratories do not exist in many places. The clinical laboratory is good, but not sufficient, for it lacks such apparatus as the thermopile and galvanometer, the ergograph, and the plethysmograph. Without these, and other essential instruments, one cannot investigate the actions of medicinal waters satisfactorily. Moreover, treatment should be influenced by similar methods.

This state of affairs is reflected in books and papers. Several of the articles published are worthless from the scientific point of view. I shall quote three which have been written during the last eight years :—

(1) "The water of St. Anne's well, like others of its clan, acts by lixiviation of the tissues and promotes the powers of elimination." What that exactly means I have never been able to discover.

(2) "Isotonic salt waters tend to liquefy the contents of the bowel and facilitate the solution of the chyme."

(3) "Some of the iron in chalybeate waters is absorbed, but some passes away, and the colour of the fæces is a useful indication of the amount of absorption which has taken place."

These are perhaps extreme cases, but they are types of the empiric and archaic statements which deface the literature dealing with the spas. They conceal a lack of scientific knowledge under a mass of meaningless verbiage.

Many of the important effects of waters and climates are not investigated, and experimental work will, probably, prove that some of the so-called actions of these therapeutic agents are incorrect. I can only select a few of these for discussion. The view is entertained that mineral waters may resemble the vitamines, but no spa physician

in this country is carrying out investigations in this connexion. Experimental work of this kind must, however, be carried out on animals, so the difficulties of obtaining a licence to experiment must be taken into account. It has recently been discovered that many organic compounds will abate the symptoms produced by their absence from the diet. Gaglio and Rosen have found that human urine will remove the nervous symptoms produced in birds by withdrawal of the water-soluble B factor, or antineuritic vitamine. It would confer great credit on one of our spa physicians if he would see whether our medicinal waters, with their contained colloids and ferments, can remove the symptoms produced by the absence of any of the three vitamines. Moreover the spas are ideal, in fact the only, places where the whole problem of diet, waters, and hygiene in promoting a return to health after illness produced by errors of different kinds can be studied. The conditions cannot be imitated in an academic laboratory, so it is necessary to obtain a licence to experiment beside the springs. Mineral waters would lose their so-called unknown factor during their transit to the medical schools.

Within recent years much has been said about radio-activity and ionic dissociation, but we really know very little about the action of these agents on the animal body. Experimental work in this connexion is badly needed, and if it is not done one can hardly wonder if scepticism is engendered concerning the postulated effects of radio-activity and ionic dissociation. The part played by the radio-active emanations in the production of thermal debility alone requires a large number of experiments for its elucidation.

There is need for a monograph showing the comparison of the effects of ordinary and medicinal waters applied in different ways, at different temperatures and durations, and in different resorts. Climatological effects should be investigated more than they are at present, and all research should have the object in view of determining the parts played by all the different therapeutic agents at the spas, and the nature of the so-called unknown factor in mineral waters. The experiments of Ferreyrolles on the effects of mineral waters on immunity and anaphylaxis might be followed to advantage here.

Referring to clinical work and therapeutics, the average medical man who reads about the spas will see that every one is indicated in gout, rheumatism, dyspepsia and other maladies, but the specific forms of these disorders which should be sent to the different resorts is not stated with sufficient clearness and detail. Until the indications are

more minutely stated than they are at present the general practitioner will be more or less uncertain of the spa to which he should send a case.

The principal reasons for the neglected state of research are the following :—

(1) The greatest fault lies in the absence of teaching in these matters at our medical schools, and the student who is gifted with a love for experimental work, and might be keen on crenological research, does not have his energies and mind directed into the proper channels; indeed he knows nothing of value about the spas when he graduates. The physiological lectures he attends are useless for this kind of work, and experimental pharmacology is an optional and, consequently, an avoided subject. The medical schools are, therefore, much to blame in their failure to train for work at our health resorts.

Those who do specialize in experimental physiology and pharmacology remain attached to their academic departments with the view of obtaining professorships in the future. Few of them are ready to sacrifice their prospects for these posts by going to spas. They would have to earn their bread and butter by practice, and they would not be able to devote their whole time to research. Good scientists are mostly poor men, and unless you encourage them by attractive financial offers and properly equipped laboratories you will not get them at the health resorts. A move is being made in the right direction at Harrogate where a pathologist, a bacteriologist, and an analytical chemist have been appointed to investigate the effects of treatment and the nature of the waters.

The workers should hold licences so that they can perform the necessary experiments on animals, and they should repeat them in the different resorts. By that means the isolation, which characterizes the spas, will to a great extent be done away with, and we shall have a better understanding of the potentialities of each spa and its specific indications. Of course such workers will be forbidden to practise. There should also be a medical man in the position of a director of treatment on the lines laid down by the experimenters. He too would be forbidden to practise.

Lastly, all results should be correlated, and the Section of Balneology of the Royal Society of Medicine should be made a central bureau for collecting information and making it widely known in its *Proceedings*. If these suggestions are carried out, there is no reason why British crenotherapy should not come to occupy a prominent place in scientific medicine.

Dr. Buckley (Buxton).

The defects of British health resorts have received more attention than the merits. This is as it should be, the Section does not exist for the advertisement of the health resorts but for the extension of their usefulness in every way, and only by full recognition of their defects can those defects be remedied. Dr. Sonntag has pointed out the necessity for research being undertaken into the mode of action of mineral waters. Research into the mode of action of climate, and also into the pathology of those diseases which are sent to spas for treatment should also be carried out. For many years the Devonshire Hospital, Buxton, has employed a pathologist whose sole duty is research into the bacteriology and pathology of the rheumatic diseases, and we have made substantial progress in the direction of a better understanding of the causes of these conditions though much remains to be done. Research by private practitioners is handicapped by the want of properly equipped laboratories and lack of means to provide necessary apparatus. The younger practitioners are not generally men of means; the man of ample private means generally elects to prosecute his researches in London or other University towns. By the time our practices have increased to a sufficient extent to provide something more than a bare livelihood, the demands of patients upon our time seriously interfere with the possibility of adequate laboratory work.

Dealing with another point of criticism, that the British health resorts do not provide the attractions and entertainment which are to be had at the Continental spas, the primary difficulty is one of funds. Such provision can only be made out of the rates, since baths are rarely run at a profit, and it is not surprising that the municipalities, at least in the case of the smaller towns, find it impossible to provide entertainments on any lavish scale. On the Continent these expenses are met either by the State or by a cure tax, and this tax has not yet found favour in this country. Both the provision of laboratories and means for research and of entertainments would have to be met out of the public purse, and this would mean State control in some form. I have dealt with this subject on a previous occasion, and will therefore not discuss it now except to point out that it is closely linked with the question of the State provision and control of our hospitals. It should not be forgotten that much of the attraction of the foreign spa lies in its foreign atmosphere furnishing a complete change of habit and environment, which cannot be imitated in this country.

Dr. CALTHROP (Harrogate).

The British spas have effected great improvements in the equipment and administration of their bath establishments during the last ten or fifteen years, but the chief advantage of sending a patient to a foreign spa lies in a more complete change of surroundings and mode of life than can be obtained at one of the home spas; and in a few instances in the possibility of treatment by certain mineral waters, such as the arsenical, which are not obtainable in Great Britain.

As Dr. Sonntag has already said, further research into the therapeutic effects of the different home mineral waters and baths should be carried out, so as to acquire more precise data for prescribing them. The planning out of spas in several instances leaves much to be desired; either the commercial or residential development of the locality has been allowed to conflict with the proper requirements of the place as a spa or health resort.

As the population of these islands increases, so the spas will become national institutions of great importance and therefore their amenities must be carefully guarded and preserved for the benefit of the health of the people.

Dr. LESLIE THORNE THORNE (London).

Discussing the merits of the administration of what is known as the "Nauheim" treatment in this country, as compared with its practice in Germany, in the first place, with regard to the action of artificially prepared baths as distinguished from natural baths: I have had many opportunities of comparing the results of a course of baths taken at Nauheim with those of previous or subsequent courses in London, more especially since the year 1914, as I have had a number of patients under my care since that year who had formerly made a practice of going to Nauheim for treatment. I have also taken records on patients in the natural baths of Nauheim in 1912. These observations, compared with many thousands I have made in England, prove that the effects produced upon the heart and vessels by artificially prepared "Nauheim" baths, are identical with those obtained at the natural springs. This is a matter of great importance to the English balneologist, because none of our British health resorts contain springs which are exactly similar to those of Nauheim, and therefore, certain chemicals have to be added to the baths at all our health resorts to produce similar effects to those obtained at Nauheim.

With regard to the *results* of treatment in this country as compared with those obtained at Nauheim, I have found that the administration of the treatment in London has proved in every way as efficacious; indeed, there is a much wider range for home treatment than for treatment at a foreign spa, for many cases of chronic heart disease, arterio-sclerosis, or angina are quite unfit physically to undertake a long continental journey, whereas they are perfectly able to take a course of treatment that can be given in their own homes, a nursing-home, hydropathic establishment, or hotel, as the following case will show:—

A patient came to me early in 1916; he suffered from an irritable dilated heart in 1900 and since that date had found that he broke down in health and was unable to continue his work if he did not have a course of baths at Nauheim nearly every year. He had had ten courses in all, the last in 1914, but fortunately he was able to get away from Germany after sixteen weeks' detention. At the end of 1915 he broke down entirely and early in 1916 I gave him a course of baths in London. I have seen him several times since, and up to the present day, just four years from his last course, his heart has given him no trouble and he has been able to keep regularly to his work. This is a distinct improvement on Nauheim results.

There are three points I should like to emphasize in the balneological treatment of chronic cardiac disease in this country, as I have found that the best results are not obtained unless they are adhered to. It is absolutely wrong to begin a "Nauheim" course with an effervescing bath; this was never done at Nauheim, in fact many of the graver cases of heart trouble had *still* baths all through their treatment, and no heart cases ever began the treatment with effervescing baths. A large number of the more advanced cases, especially those with much hypertension, are actually harmed by effervescing baths given at the beginning of a course, and in many of these cases a better result is obtained if *still* baths are given throughout the entire course. Many nurses and bath attendants have been taught that a "Nauheim" bath is equivalent to a carbonated effervescing bath, and have consequently given entirely wrong treatment, sometimes with alarming results, doing harm to the patient and to the treatment. I believe that this error is still taught at many schools of massage and it is certainly taught in some books.

The second point is the fact that the best results cannot be obtained if the administration of the baths is left entirely to a nurse or bath attendant, however skilled she or he may be. It is my practice to watch the patient in the bath at least twice a week and to make a note of its effect upon the pulse and circulation. I find this is the surest

way of regulating the strength, length, and temperature, of the baths, so that the patient may receive the fullest benefit from them.

My last point is the importance in cardiac cases of giving the patients their baths in the same establishment as that in which they live. It is quite suitable, and in many cases desirable, for a rheumatic or gouty patient to have to make a certain amount of effort in following out his course, but in a case of cardiac disease any exertion is a great strain on an already overworked heart, and to give treatment under such circumstances is to minimize materially the benefit which would be obtained from a course given under more satisfactory conditions.

The English balneologist can benefit his cardiac and circulatory patients as much at home as if they had gone to a foreign spa, if not more so, provided that he can spare the necessary time to attend to the details of the treatment. I remember that when the late Sir Lauder Brunton returned from a course of baths at Nauheim he told me that he had had no idea of the immense importance of the small details of the treatment until he had himself undergone it.

Dr. F. G. Thomson (Bath).

Though the spas of this country may not, on the whole, compare favourably with certain continental health resorts in matters pertaining to what one may term the accessory factors of a so-called "cure," yet we have a wide range of health resorts, embracing all varieties of climate to be found in the British Isles, and well equipped in all essential matters to deal with those types of disease amenable to treatment by physical agencies. But when we come to inquire whether we are making the best use, in the interests of our patients, of the advantages and facilities we possess, we must admit that much remains to be done. Hitherto in this country there has been a deplorable lack of original research into the physiological action of the various natural mineral waters to be found here, and of the various balneological and other methods by which they are customarily employed.

There is a wide field of research open at the present time, not only into the physiological aspects of balneology, but also—and what is of much greater importance—into the ætiology and pathology of the various forms of arthritis, "rheumatism," and gout, which are so prevalent among the patients at all spas. Research which would be of any value in these matters is necessarily arduous, lengthy and difficult, beyond the powers of those in clinical practice, who have in most cases neither the time nor the training to enable them to carry it out. A certain amount of hitherto unpublished but valuable work has been

done recently, but it represents only a tithe of what remains to be done.

The amount of invalidism, and incapacity to work, resulting all over the country from the so-called "rheumatic" diseases, is so great, that the question becomes one of considerable importance, and it would be sound policy for the Ministry of Health to institute and endow some comprehensive scheme of research into the many biochemical, bacteriological, and other problems associated with these diseases. Researches of this nature, carried out by picked men, at such special hospitals as the Devonshire Hospital at Buxton and the Mineral Water Hospital at Bath, where a large amount of clinical material is always to be found, would undoubtedly prove of the utmost value, and it is only by such means that the treatment of these diseases can be put on a really sound scientific basis.

Dr. F. P. NUNNELEY (Llandrindod Wells).

The defects of British spas are chiefly due to lack of attention to details on the part of—

(1) *The doctors* who, too often, content themselves with prescribing a course of waters or baths and do not trouble to give any directions as to how their patients are to spend their time when they are not actually undergoing treatment.

(2) *The municipal authorities*, who do not make the best of their towns by laying out gardens and walks, providing seats, shelters, latrines, &c., where they are most likely to be needed.

Both doctors and municipal authorities seem to concern themselves chiefly with present finance, rather than with the future prospects of their spas. In most British spas there appears to be a lack of the capital necessary for the proper development of their natural advantages. As things stand, the only remedy for this seems to be the control of the spas by financial syndicates, as in the case of Strathpeffer.

In addition, too many of the medical profession, outside the spas, seem to regard them as places to which they can send their patients when they are bored with them—not because they have any faith in spa treatment. The attitude of the doctors reflects itself in the minds of the patients, who are too often frankly sceptical as to the benefits which they are likely to derive from spa treatment.

Until these various defects are remedied and the public, generally, is interested in spa treatment in this country, I do not believe that it is the slightest use to ask the Government for financial aid for research at the spas themselves.

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Section for the Study of Disease in Children.

President—Mr. J. P. Lockhart-Mummery, F.R.C.S.

Sequel of the Case of Lipodystrophia Progressiva, shown on January 24, 1919.[1] [2]

By F. Parkes Weber, M.D., and T. H. Gunewardene, M.R.C.S.

With a MICROSCOPICAL REPORT by H. M. TURNBULL, M.D.

Director of the Pathological Institute of the London Hospital.

The girl, aged 13 years, was readmitted to the East London Hospital for Children, on May 19, 1919, with purulent otorrhœa on the left side (of one month's duration) and swelling in the left mastoid region (of six days' duration). A left mastoid operation was performed on May 20, 1919. Soon afterwards there was a septic (pyæmic) type of pyrexia, which lasted till the patient's death (from pyæmia) on September 8, 1919. Necropsy, on September 9, 1919: The body was altogether much wasted, but by naked-eye examination fat was practically completely absent from the subcutaneous tissue of the upper part of the body (above the pelvis). A moderate amount of fat was present in the gluteal regions, orbits, omentum, about the kidneys, heart, and pericardium, and under the serous membranes. Had the child died, not from chronic pyæmia, but from some disease not associated with extreme wasting, there would doubtless have been much more fat left in the regions unaffected by the lipodystrophia. There

[1] At a meeting of the Section, held October 24, 1919.

[2] *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Study Dis. Child.), p. 13.

was no evidence of any tuberculosis anywhere. Excepting what was connected with a left empyema and the pyæmia (including parenchymatous nephritis and large septic liver and spleen) and the lipodystrophia, there was nothing obviously abnormal to naked-eye examination. The thyroid gland was relatively rather large, and by macroscopic and microscopic examination was found to be very rich in colloid material. The thymus gland was represented by a scanty remnant. The brain did not appear in any way diseased.

Microscopic examination of one ovary, the pituitary gland and both suprarenal glands, showed nothing special. A piece of the anterior abdominal wall and a piece of the hairy scalp from the occipital region were found microscopically to contain no fat vesicles in the subcutaneous tissue (special stains for fat were not employed), thus confirming the naked-eye appearances mentioned above, and likewise confirming results of "biopsy" examinations made in previously published, typical cases of lipodystrophia progressiva.

The fat-atrophy in the parts specially affected in cases of lipodystrophia progressiva seems to be more extreme than that met with in post-mortem examinations on even the most wasted subjects of chronic pulmonary tuberculosis. Nevertheless, persons with lipodystrophia progressiva appear as a rule to enjoy good general health and their average capacity for work is probably quite up to the normal for ordinary persons of the same age as themselves. Their facial appearance is the chief source of trouble, because employers think they must be suffering from "consumption," and this may prevent them from obtaining work; fellow clerks and others may for the same reason be frightened to work in the same room with them. Their wasted cadaverous aspect may be seriously distressing to them, especially in the case of females, and still more so if they have to earn their livelihood by dancing, &c., on the stage.

When all the fat has disappeared from the parts of the body affected in lipodystrophia progressiva, the disease naturally comes to an end, and therefore, strictly speaking, can no longer be correctly termed *progressive*. As far as we know, in cases of lipodystrophia progressiva, the fat, when once atrophied, never returns, but, as the patient grows older, the subcutaneous connective tissue probably increases somewhat, and the emaciated face no longer arrests attention to the same degree as it did. Moreover, thinness of the face is naturally less noticeable in elderly persons.

The present seems to be the first published *post-mortem examination*

on a case of lipodystrophia progressiva. We, therefore, thought that as much critical information as possible ought to be obtained in regard to the microscopical appearances. Dr. H. M. Turnbull, of the Pathological Institute of the London Hospital, to whom our best thanks are due, has very kindly examined the sections, and has furnished us with the following report:—

MICROSCOPICAL REPORT BY DR. H. M. TURNBULL.

The sections are stained with hæmatoxylin and eosin.

(1) *Scalp.*—The skin is cut obliquely. Consequently the roots of the hairs are cut transversely. In the deeper part of the section there are transverse sections of a few hair-bulbs with their papillæ, and several roots cut immediately above the level of their papillæ. It is clear, therefore, that the section includes a depth of scalp at which under normal conditions subcutaneous fatty tissue would be present. Indeed, beneath the level of the hair papillæ the greater part of the section includes a layer of horizontal fibres and vessels. There can be little doubt that this layer represents the aponeurosis which normally lies beneath the subcutaneous fatty tissue. External to the hair follicles in the deeper part of the section there are a few small areas which may contain small fat cells. Apart from these small doubtful areas, there is no evidence of fatty tissue. The interstitial tissue of the scalp is much denser than normal. The arrangement of the fibrous bundles appears, however, to be normal, so that there is no evidence of inflammatory fibrosis. The density appears to be due simply to the absence of fatty tissue.

(2) *Abdominal Wall.*—The section at one small spot passes right through the abdominal wall from the epidermis to the parietal serosa. In the remainder of the section the deep aponeurosis and peritoneum are absent. Sections of similar portions from normal abdominal walls of adult females show a deep layer of subcutaneous fatty tissue between the dermis and the muscle, a variable amount of fatty tissue in the intramuscular septa, fatty tissue between the muscle and deep aponeurosis, and lobules of fatty tissue between this deep aponeurosis and the fibrous peritoneum. In none of these positions can I detect fat cells. Owing to the method of preparation of the section this observation does not exclude the presence of such cells, but it can be said that, if any fatty tissue is present, it must be astonishingly scanty.

(3) *Thyroid Gland.*—The section appears to include a complete transverse section of a lateral lobe. There is an unusual amount of colloid in the acini throughout the section. The excess of colloid is associated in some acini with a slight desquamation of proliferated epithelial cells. The condition of the acini suggests that the gland was in a state of moderate colloidal over-activity. At the same time, as far as can be judged without a special stain, the interstitial tissue is more abundant and denser than in control specimens showing colloidal hypertrophy in children of the same age, and, indeed, more abundant and denser than in the normal thyroid. The appearances of the whole section suggest that a moderate degree of over-secretion of colloid was associated with a slight fibrosis and actual diminution in size of the gland.

(4) *Both Suprarenal Glands (Sections A and B).*—Section A is taken from a portion of the gland peripheral to the medulla. Section B includes medulla. The cortical cells are small. Their cytoplasm is deeply stained and displays little vesiculation. I conclude from this that the zonæ glomerulosa and fasciculata contain less lipoid than is usually present at the age of 13 years. It is unfortunate that very little retroperitoneal tissue is included in the sections. There is, however, sufficient to show that fatty tissue is present (section A).

(5) *Pituitary Body.*—The section has been made transversely through the anterior lobe, a small part of the stalk being included. Since our routine procedure is to make a sagittal section through the centre of the gland, I am unable to make use of controls. Further, the method of staining has not differentiated clearly the chromophobe cells from the chromophil, and the cyanophil cells from the eosinophil. I can only say, therefore, that several acini enclose a central mass of secretion, and that I detect no gross abnormality in the gland.

(6) *Ovary.*—I can detect no abnormality in the ovary. It contains numerous primary follicles lined by a single row of flattened or of cubical, epithelial cells, a follicle lined by stratified epithelium and containing a little liquor, and one fully developed Graafian follicle.

Conclusion.

In the sections of the scalp and abdominal wall the only evidence detected of fatty tissue was the presence in the scalp of a few small areas which may have been occupied by fat-cells. In the absence of special stains it is not possible to exclude the presence

of some fat-cells, but the sections suffice to show that fatty tissue is almost completely, if not completely, absent.

One of the sections of the suprarenal bodies includes a little of the surrounding retroperitoneal tissue. Definite fatty tissue is present in this.

In no section are there lobules of embryonic fatty tissue such as are found in the fœtus, and in infants during the first and even second year of life.

I can detect no abnormality in the ovary. If I had sections of the anterior lobe of the pituitary gland stained in a manner to differentiate the cells, I might record the relative number of the different kinds of cells, but I could not attempt to interpret their significance without making a special investigation into the changes found in health and disease in the anterior lobe of girls of 13 years of age. In the suprarenal bodies there appears to be less lipoid than usual in the cortex. This difference from available controls is so slight that it would be very dangerous to consider it of special significance. It would appear to fall within the limits of normal physiological variation. Further, the effect of the infection which caused death cannot be excluded.

In the thyroid gland there is an excess of secretion of colloid. An excess of this degree is often found at post-mortem examinations. It falls far below that which frequently occurs at puberty. Little significance can be attached therefore to this excess of colloid, if it is the only unusual feature. But the excess of colloid appears to be associated with fibrosis. Without a special stain, however, I cannot say definitely that fibrosis is present. The naked-eye examination and the measurements and weight of the gland should give exact information on this point. If the excess of colloid was such as occurs physiologically, especially at puberty, the gland would be correspondingly enlarged and the interstitial tissue would appear rarefied rather than thickened. If the colloidal over-activity in this case was associated with fibrosis and with no enlargement, or with actual diminution in size of the gland, then a pathological condition was present, and not a phase of normal activity. It would be reasonable to consider that such a pathological condition of the thyroid was connected with the abnormality in the subcutaneous fat.

Case of Lipodystrophia.

By Frederick Langmead, M.D.

The patient is a girl, aged 7 years. She was quite well and normal in development until Christmas, 1917, but about that time wasting was first observed; it was not noticed to be localized. About two months later, after an attack of measles, the wasting became more obvious and progressed more rapidly. After the measles the child had an attack of acute rheumatism, for which she was confined to bed for six weeks. Later, with convalescence, the wasting was remarkable, and led to her admission to hospital. Dietetic treatment was followed by no improvement in her gaunt and emaciated appearance, and repeated examinations failed to reveal any cause. A stay in the country was attended by no better results.

Examination shows that the wasting is confined to the upper part of the body and ends about the level of the umbilicus. The face and neck are distinctly affected, all the fat seeming to have disappeared. Her face is consequently gaunt, with prominent bones and hollow cheeks and eyes, reminiscent of a famine picture. The arms are wasted, but to a less extent, and the thorax and abdomen show the same condition progressively less in a downward direction. The lower limbs are normal, straight, and slim, and, though not wasted, have no appearance of pseudo-hypertrophy, as is sometimes seen. The disappearance of fat is accompanied by no loss of power or other symptom, the child being in good general health. The only disability is a cosmetic one, which causes much anxious inquiry after tuberculosis or cancer by well-meaning friends and relations, and strangers.

Dr. F. Parkes Weber: I regard Dr. Langmead's case as a typical one in every respect, for the patient is of the female sex and the fat-atrophy of the face is extremely marked. The loss of subcutaneous fat does not seem to have as yet quite reached the pelvis, but no doubt it will in time do so. Lipodystrophia progressiva may likewise affect the male sex.[1] Some of the cases, in young men, of bilateral facial atrophy are almost certainly the same disease, in which the face is the only part affected, and the atrophy is limited to the subcutaneous fat. In typical cases of facial hemi-atrophy the deeper tissues are also involved.

[1] Cf. F. Parkes Weber, "Lipodystrophia Progressiva in a Male," *Proc. Roy. Soc. Med.*, 1917, x (Sect. Study Dis. in Child.), p. 117.

Enlargement of Cervical Lymphatic Glands in a Case of Congenital Syphilis.

By H. C. CAMERON, M.D.

E. W., aged 8½ years, is the eldest child. Then followed a miscarriage, then a child who died at the age of 6 weeks of convulsions, then a child who died at the age of 6 months, cause unknown. There is one other child 9 months old, said to be doing well. This child is said to have been well till the age of 5 years, when interstitial keratitis developed. A year ago she had nasal diphtheria, and it was then noticed that the cervical glands were enlarged. She was sent to the seaside, and the glands were treated as tuberculous.

On examination, the child is seen to show clear evidence of a congenital syphilis, with Hutchinson's teeth, keratitis, and extensive cicatrization of the palate. There are numerous enlarged glands in the neck, on both sides, over the occiput, under the chin (submental glands), above the left clavicle and in the left axilla. The subcutaneous tissue of the neck is swollen, has been œdematous, and forms a thick collar encircling the throat, in which the glands can be felt.

The case is shown to elicit opinion as to the nature of the glandular enlargement, whether syphilitic or tuberculous. There is no evidence of enlargement of the thoracic glands, and the spleen cannot be felt.

My opinion is, that the enlargement is directly connected with the syphilis which she has; there are, as already stated, keratitis and Hutchinson's teeth, and, in addition, she has recent and active ulceration of the soft palate. If asked to support my opinion, I would answer that the involvement is so widespread; it is not common, I think, to find tuberculous enlargement in the occipital glands or the submental group. And it is in keeping with this that there was actual œdema of the part, as if the infiltration was extensive, and had resulted in complete lymphatic obstruction. During the last fortnight the glands have been getting smaller, and the child has been having mercury by the mouth.

DISCUSSION.

Dr. GOFFE: How does Dr. Cameron exclude lymphadenoma here?

Dr. F. J. POYNTON: With regard to this interesting glandular case, I recall occasional cases in which there has been a general enlargement of the

glands all over the body, resembling lymphadenoma, occurring in some curious septic condition. These are very puzzling and rare cases, which may go on for several months, and yet there is nothing to be found except enlarged glands. A group which I have also seen, though rarely, is that in which all the weight of the tubercular infection seems to fall on the lymphatic glands, which become much enlarged, and there is splenic enlargement too. There is also a third group of glandular cases of which I have seen a very striking example—namely, the syphilitic. This child was in the hospital many months, and by many people who saw the case it was diagnosed as a straightforward one of lymphadenoma, with enlarged glands all over the body, an enlarged spleen, and with that intermittent fever with which some child-subjects of lymphadenoma are affected. That child died, and at the post-mortem examination, to one's astonishment, it was found to be a syphilitic case. The enlargement of the spleen was due to gummata, and there were also gummata in the liver. Apparently the whole condition was syphilitic. Cases such as the present one must be rare, and I cannot recall such a striking example of syphilis simulating lymphadenoma as the one I have just mentioned; but I have never seen another. It is most interesting, scientifically, to come upon these unusual examples of infections producing a condition so clinically similar to lymphadenoma.

Dr. F. PARKES WEBER: This glandular case reminds me of the cases in adults occasionally met with in which, in connexion with late secondary syphilis, one finds an enormous enlargement of cervical glands. These, I think, are rather rare: yet they are fairly well known. Hodgkin's disease (lymphogranulomatosis maligna) has sometimes been wrongly diagnosed in syphilitic patients. A medical man told me that when he was young (soon after he set up in practice) a well-known pathologist in London diagnosed lymphadenoma (Hodgkin's disease) in his case on account of glandular enlargement (after a "biopsy" microscopic examination). The medical patient remembered he had had a small sore place on his finger, and it dawned on him that the glandular trouble might be syphilitic—it was long before the days of the Wassermann reaction. He then underwent a thorough antisyphilitic treatment, and the condition cleared up, much to his delight. Hodgkin's disease (if correctly diagnosed) is probably a fatal disease.

Dr. E. BELLINGHAM SMITH: Some years ago, in my out-patient department, I saw a case similar to Dr. Cameron's. The child had marked syphilis, also enlargement of glands, not only in the axilla, but also in the groin. It improved steadily on antisyphilitic treatment, and eventually got well. One of the glands in Dr. Cameron's case is breaking down: I do not know whether he thinks that is through syphilis, or whether it has been secondarily infected. I suppose it is possible that even syphilitic glands can become tuberculous.

The PRESIDENT: I think there is no doubt that these are syphilitic glands. These are comparable to gummata, and they may break down, though, as a rule, they do not. I do not think this child's case is characteristic of tubercular

gland enlargement in the neck. I agree with Dr. Parkes Weber that many supposed cases of Hodgkin's disease are not typical of that disease.

Dr. CAMERON (in reply): The question of lymphadenoma was discussed, but the bulk of the evidence seemed to me to be against it. There is such an active syphilitic process in the immediate neighbourhood of the enlarged glands, in the shape of ulceration of the palate, that I had meant to put it to the proof by watching the effect of antisyphilitic treatment. I think the enlargement of the glands will disappear. It may be worth while trying to find the spirochæte in a gland.

Sudden Occlusion of the Right Brachial and Femoral Arteries in an Infant aged 10 Days, with Gangrene of Arm and Leg; Recovery of Arm, Amputation at Thigh.

By H. C. CAMERON, M.D.

M. B., ADMITTED to Guy's Hospital on the eleventh day of life. Two days earlier the right arm had become stiff, cyanosed, cold and immobile. On the next day, the tenth of life, the right leg passed into the same condition. Up to the ninth day the child's condition had not been satisfactory and a rash of "large blisters" had appeared.

On examination the child was seen to be very small, weighing less than 5 lb. The skin appeared to be recovering from a condition of pemphigus or erythrodermic desquamation. The superficial epithelium in half a dozen patches on the trunk had desquamated and hung in large shreds around the margins of the patches. The umbilical cicatrix was not healthy and exuded a little blood-stained serum. The arm below the middle of the humerus was cold, swollen, blue and immobile. The hand and fingers were flexed. There were many small points of hæmorrhage under the nails. Pulsation could be felt in the axillary and subclavian artery, not in the brachial or radial. The condition of the leg was similar. Pulsation could be felt in Scarpa's triangle but not below. For eight days the child was watched in the ward. By the end of this time the arm had completely recovered. It had again become warm and pink and pulsation could be felt at the wrist. A few tiny points of necrosis still remained at the finger tips, which rapidly healed with very superficial scarring. The condition of the leg, on the other hand, rapidly became worse. On the fifth day after admission, the skin over the femoral artery in the middle of the inner side of the thigh broke down, exposing a mass of blood clot. Below this the leg

was black and gangrenous. On the same day a line of separation began to form and spread around the thigh with remarkable rapidity. On the eighth day after admission, the nineteenth day of life, amputation through the thigh was performed. The temperature had gradually risen to 102° F. It is to be noted that throughout the curve of the daily weighing showed a steady rise. Calculating the weight of the amputated limb to be one-tenth of the body weight, the gain in weight during the first three weeks after admission was about 18 oz. (550 grm.). On discharge the weight had risen from 2,400 grm. to 3,650 grm. in nine weeks. From the clot in the femoral artery, and from the stools, a streptococcus was obtained on cultivation.

Case of Leukæmia with Scalp Nodules.

By T. H. Gunewardene.

(Introduced by Dr. F. Parkes Weber.)

The case is that of a boy aged 3 years and 4 months who has been under observation with fairly regular blood counts for the last seven months, and whose condition, at first thought to be an aplastic anæmia, subsequently, owing to the quick recovery under treatment, renamed "secondary anæmia of unknown cause," now turns out to be leukæmia, probably of the myeloid variety, with the simultaneous development of nodules, mainly of the scalp.

As to the history of the case, the illness dates from December, 1918, when the child was treated as an out-patient in a hospital for three weeks for "bronchial catarrh" and was reported to be pale and slightly jaundiced at the time. Our observation dates from March 14, 1919, when the boy was admitted to the East London Hospital for Children, Shadwell, under Dr. Clive Riviere who has kindly given me permission to present the case before you.

A word as to the previous and family history. The child is said to have had measles at 10 months of age, but most probably that must have been a mistaken diagnosis considering that rashes simulating measles are common during infancy, and that the boy actually developed measles while in the ward on April 29. The boy is one of two children, the other, as well as the parents, are healthy.

The symptoms on admission were those of any severe anæmia—

namely, disturbed sleep, irritability, disordered appetite, anorexia, headache and a troublesome cough. Bowels had been open daily.

On admission, that is on March 14, we found the boy extremely pallid, of a lemon yellow tint but not jaundiced, fairly plump, lethargic appearance and no purpuric manifestations. The gums and teeth were healthy. The tonsils were large but not inflamed. The posterior cervical, axillary and inguinal glands were just palpable, firm, discrete and freely movable. The lungs showed nothing abnormal. The heart was much enlarged with diffuse pulsation over the precordium, and dullness extended to 1 in. outside left nipple line, and nearly to the right nipple line. Loud systolic and diastolic murmurs were heard all over the precordium. Abdomen: The liver was enlarged, being 2 in. below costal margin, but the spleen was not found to be enlarged to percussion or palpation. Ophthalmoscopic examinations on two occasions gave negative results. The Wassermann and von Pirquet tests were also negative. The urine was examined several times and showed on a few occasions, as the pathologist states it, "the faintest detectable trace of albumin." A few red cells and a few granular and leucocytic casts have been seen at times. No ova nor parasites were present in the fæces. Cultures from them revealed the presence of *Bacillus coli*.

The first blood examination done on admission gave the following result: Red cells, 760,000; white cells, 8,400; hæmoglobin, 11 per cent.; colour index, 0·8. Differential count: Polymorphonuclears, 9·5 per cent. (798 per cubic millimetre); small and large lymphocytes, 86 per cent. (7,124 per cubic millimetre); hyalines, 2 per cent.; myelocytes, 2 per cent. Two hundred cells were counted; no nucleated red cells were seen. There were marked anisocytosis, poikilocytosis, and polychromasia. This blood count shows an absolute and relative diminution of the polymorphs, but an absolute and relative increase of the lymphocytes. The unusually low red cell count, the great diminution in hæmoglobin, absence of nucleated red cells, and no apparent cause for the anæmia led us to make the tentative diagnosis of aplastic anæmia notwithstanding the extreme rarity of the disease.

To describe the progress of the case it is necessary to mark out three periods:—

(1) From the date of admission, March 14, to April 29, when the child developed measles and was sent home.

(2) From the date of discharge to August 7 when the nodules were first noted.

(3) From August 7 to present date.

To take the first period: The child was extremely pale, with a hæmoglobin percentage of 11·1, a red cell count of 760,000, white cells 8,400, polynuclears being 9·5 per cent., lymphocytes 86 per cent., and myelocytes 2 per cent. Then under treatment there was a marked improvement at the end of five weeks, as shown by the greater interest taken by the child in his surroundings, his improved colour, the hæmoglobin of 61 per cent. and nearly 3 million red cells. The percentages of polymorphs and lymphocytes were now quite reversed, being 62 per cent. and 21 per cent. respectively, instead of 9½ per cent. and 86 per cent. Nucleated red cells appeared, and poikilocytosis became less and less marked. The cardiac dullness became much diminished; both systolic and diastolic murmurs were now absent.

Treatment during this time consisted of intramuscular injections of soamin 1 gr. and ferri et ammon. cit. 2 gr., on alternate days. Twelve injections were given in all; red marrow and arsenic and iron were administered by the mouth.

Now to take the second period covering the time during which the child was out of hospital until the discovery of the nodules on August 7. I must add here that I had been examining the child for nodules at each of his frequent and regular visits. Judging by my notes the nodules must have appeared some time after July 4, and this period I missed as the child was away at the seaside. A synopsis of the blood counts made at this time shows with considerable regularity 4 million red cells, 60 per cent. hæmoglobin and an average of 13,000 leucocytes. The differential counts were normal for a child of this age, with the exception of a polynuclear increase at the expense of the lymphocytes on one occasion. It is noteworthy that no myelocytes nor myeloblasts were found in the blood during this second period.

Now, to come to the third period dating from the first appearance of the nodules on August 7. There were eleven nodules of varying size on the scalp, the largest being about ½ in. in diameter. They were hard and paler than the rest of the skin, and as the skiagram shows, not attached to the bone or periosteum. The left palpebral fissure was narrower than the right, this being due to what was apparently a nodule in connexion with the periosteum at the left infra-orbital margin. The superficial glands which had never been more than just definitely palpable were now recognizably enlarged, discrete and freely movable. No abnormal cells in the blood were seen at this stage.

On September 6 the nodules in the scalp, eighteen in number, and

the one over the infra-orbital margin were larger still, and two cutaneous nodules appeared in the right posterior iliac region and the right thigh in front. The glands were enlarged as before, and both testicles were now found to be equally enlarged and of almost stony consistence without any tenderness. It was on this date, September 6, that myeloblasts to the extent of 20 per cent. first appeared in the blood picture with a hæmoglobin percentage of 72, 5½ million red cells, and a leucocyte count of 13,000. No poikilocytosis nor nucleated red cells were present.

Immediately after admission the child was not given any treatment for three weeks, with the result that, although the nodules became decidedly smaller and the testicles regained their normal size and consistency, the hæmoglobin content of the blood depreciated from 72 per cent. to 44 per cent., and there was a drop of about half a million red cells; these changes were evidenced clinically by progressively increasing pallor. Now comes an important series of events:—

On September 23 the highest recorded leucocyte count was obtained, namely, 69,000. On the 24th treatment was commenced, which consisted of an injection of soamin and iron citrate; on the 25th the temperature, which had hitherto been normal, registered 101° F., and a swelling appeared in the right axilla and unconnected with the site of injection. This swelling simulated an abscess very closely and was incised without any pus being found. For four weeks subsequent to this the temperature fluctuated between normal and 102° F. This, with the exception of a slight temperature of one week's duration in March, was the only time the child ran a temperature. The differential count on September 23 shows, as I have already stated, 69,000 leucocytes with 86 per cent. myeloblasts.

To-day the child's clinical condition, as you have seen, is as follows: Pallid and with a few nodules on the scalp. Has a lethargic appearance. No enlargement of the spleen, liver firm and 1 inch below costal margin, otherwise general examination negative. A skiagram of the chest shows a small shadow which is apparently the thymus, not enlarged.

The clinical history of the case may be summed up as follows: A boy, aged 3 years and 4 months, with an apparently idiopathic severe anæmia of 11 per cent. hæmoglobin and 760,000 red cells, responds, as evidenced by his general condition and blood picture, to treatment in five weeks. Then he contracts measles, and about three months later develops nodules mainly in the scalp, exhibiting at the same time a different blood picture, which is also most unusual, in that he shows a hæmoglobin percentage of 72, red cells 5½ million, together with what

we look upon as myeloblasts. Without treatment some of the nodules disappear and others become smaller, but with a reduction of the red cells and the hæmoglobin content. With the commencement of treatment *post hoc* or *propter hoc*, the temperature shoots up and a soft tender tumour develops, but judging both by the general condition and blood picture the child to-day seems to be taking a turn for the better.

I must emphasize the fact that the spleen was never palpable, nor was it ever enlarged to percussion.

I am much indebted to Dr. Ross for the numerous examinations he made in connexion with the case; the following is his report on the blood examinations:—

"Between March 19 and October 5 sixteen complete blood examinations have been made; for ten of these I am responsible in their entirety, and I have examined stained films from the remaining counts. When the patient first came under observation with a red count of about three-quarters of a million, a hæmoglobin percentage in the neighbourhood of 11 and an absence of nucleated red cells, a tentative diagnosis of aplastic anæmia was considered. During this period the lowest white count was 5,000, the highest 14,200 and the average 8,000. In the four complete counts made by me during this period there was nothing specially noteworthy about the leucocytes, having regard to the prevalence of a leucocytosis and the notable instability of the hæmopoietic system in children of this age. Thus the polymorphonuclear percentage varied between the limits of 33 and 67, while the figure for the small lymphocytes was between 56 and 20. In two of my counts 1 per cent. of myeloblasts was found, but this did not particularly attract my attention at the time. The patient left the hospital for the first time with some 3 million red cells and 61 per cent. hæmoglobin.

"From June 1 to September 6 the patient was out of hospital. Three counts were made, the last of which was on August 7. The red cells were stationary at 4 million with 60 per cent. of hæmoglobin. The leucocytes averaged 13,000. The count of June 1 showed a polymorphonuclear increase of 86 per cent.; in the two remaining counts the polymorphs were approximately 60 and the lymphocytes 27 per cent. Once a low grade eosinophilia of 8·5 per cent. was present. No myelocytes nor myeloblasts were noted at this time except a negligible half per cent. on June 1.

"On September 6 Mr. Gunewardene readmitted the patient and a blood examination done on this date for the first time revealed the

character of the blood seen in this stage of the case. He found about 50 per cent. lymphocytes and about 20 per cent. myeloblasts. I examined the film later and confirmed his diagnosis of a myeloblastic change. To summarize the blood changes found at this stage of the disease from September 6 to 23, the red cells averaged 5 million while the hæmoglobin progressively decreased from 72 to 44 per cent. The leucocytes were 13,600, 18,000, 15,600, and in the count of September 23, 69,000. This was the only occasion on which anything like a big leucocytosis was seen. On September 11, 19, and 23 the myeloblasts were 82·66, 65, and 88 per cent. respectively, this latter figure was accompanied by the leucocytosis of 69,000. A careful examination of the films taken during the height of the myeloblastic wave showed the presence of a few stray neutrophil myelocytes, negligible in number, difficult to find, and often missed in a count of 300 cells.

"The last two counts of October 7 and 15 showed further changes; a drop of 2 million red cells accompanied by an average hæmoglobin percentage of 27. With the lower red count the leucocytes were 7,200, and when $3\frac{1}{2}$ million red cells were reached, the leucocytes fell to 5,300. It is unfortunate that owing to the pressure of work the leucocyte total was not watched between September 23 (69,000) and October 7 (7,200).

"Between the dates September 6 and 23 the polymorphs never reached even 5 per cent., but on October 7 and 15, 13 and 41 per cent. were found, the corresponding figures for the myeloblasts being 65 and 40 per cent. An improvement in the general condition of the child seems to have coincided with return of the polymorphs at this last count.

"At present I prefer to regard this case as one of myeloid leukæmia, comparable at least in its final stage, to three others reported by me in the *Lancet* of December 2, 1916, and also to certain cases given by Panton and Tidy in a contribution to the German hæmatological journal in 1913.

"A point of interest and importance is, should we have made a diagnosis of myeloid leukæmia *before* September 6, that is before the appearance of myeloblasts in any considerable numbers? In this connexion it is only possible to say that while the appearance of 1 or 2 per cent. of premature type of cells, that is myeloblasts not myelocytes, is generally without significance in the blood of a child under or about 5 years of age, the chance that the case may develop into a myeloid leukæmia should always be remembered. My other three cases did

not come under observation in the earlier stages of the disease, and when first seen their differential count corresponded to our stage three in this case, except that nodules were absent. Early in the history of this case when 86 per cent. lymphocytes were present, and later when the nodules appeared, the suggestion that it might be a lymphatic leukæmia was put to me. I replied that I did not think so, and if it was a leukæmia at all (about which I must confess that at that time I was very doubtful) it would more likely turn out to be one of the myeloid variety. I have never seen a case of lymphatic leukæmia in eight years' experience here, and this only goes to confirm Panton's remark in his text-book on 'Clinical Pathology' as to the extreme rarity of this disease in children, and I should in fact go further than Panton and say that, at any rate in infants, lymphatic leukæmia—perhaps better termed lymphæmia, is so rare as to be quite a pathological curiosity.

"There is one anomaly to be mentioned—the occurrence of a leukæmic blood picture at the time that diagnosis was first established, associated with $5\frac{1}{2}$ million red cells and 72 per cent. hæmoglobin. I cannot remember having seen this before, and quite the contrary happened in the three other cases alluded to. This high red count persisted for three weeks although the hæmoglobin content declined.

"Myeloblasts are recognizable on their morphology in films stained by Leishman's method. As further confirmation the oxydase reaction was done by von Gierke's method. Some of the cells gave the reaction, and we conclude that the others which did not, could be looked upon as premyeloblasts."

In conclusion I ask the following questions:—

(1) Is this one and the same disease from beginning to end, or had the measles anything to do with the blood picture?

(2) Is it possible that the treatment was carried too far and that the nodules pointed to reaction to treatment? The only treatment the child had three months previous to the onset of nodules was B.P. syr. ferri phos. co.

(3) If this was a true leukæmia of the myeloid variety how is it that there is no true leucocytosis, and why no actual myelocytes considering the chronic nature of the case?

DISCUSSION.

Dr. F. PARKES WEBER: We are very much indebted to Mr. Gunewardene for bringing forward this case. I think it is the only leukæmic case on record exactly of the same (clinical) kind. Imagine a case of (probably, myeloid) leukæmia in a child commencing with extreme anæmia (the worst conceivable kind of anæmia), with less than a million red cells to the cubic millimetre of blood. After a little time, under treatment, the patient develops a condition of polycythæmia, with great excess of red cells in the blood. I am not prepared to say that in cases of leukæmia the transition from extreme deficiency of red cells to great excess has ever been recorded. But, apart from leukæmia, cases apparently resembling pernicious anæmia have been known to develop polycythæmia, with great excess of red cells in the blood. The other interesting point is the drop in the white cell count, within a few days, from 69,000 to 7,200 in the cubic millimetre of blood. Such a drop occasionally occurs in leukæmia in connexion with severe terminal infections, but that is different from what occurred in Mr. Gunewardene's case.

Dr. F. J. POYNTON: It is impossible to discuss such a case as this without careful study, but there is no doubt it is a contribution to the history of blood diseases in children. A complete history such as this is very difficult to get.

Postscript.—The child died on November 27. For further notes in connexion with the case and results of post-mortem findings refer to the next issue of the *British Journal of Children's Diseases*, October-December, 1919.

Isolated Disease of Tarsal Scaphoid—Köhler's Disease.

By P. MAYNARD HEATH, F.R.C.S.

A BOY, aged 7 years, knocked his left foot six months ago. He walked badly after the accident, and the foot swelled a little, but it gradually recovered, and he made no complaint for three months. The swelling of the foot then began again without pain or redness. When first seen, the swelling had decreased, but was still present. It was diffuse over the inner border of the foot, and most marked over the scaphoid. No redness nor local tenderness were present. There were no other bony lesions. The boy walked with the foot abducted. There was no paralysis, but the calf was a little wasted.

The foot was put in plaster of Paris in corrected position, and the boy was allowed to walk on it. Recovery ensued in three months.

There is a strong history of tuberculosis on the father's side, and the boy looks delicate. The mother states that since the cold weather came on, the boy has suffered from "bronchial trouble," and that when his chest was bad, his foot swelled again slightly.



FIG. 1.

Isolated disease of tarsal scaphoid—Köhler's disease. Condition in May, 1919.

The diseased scaphoid shows the following X-ray changes:—

(1) Smaller than normal.

(2) Outline irregular.

(3) Architecture impossible to recognize, owing to cortex and spongy portion running together.



FIG. 2.

Condition in October, 1919.

(4) Density greatly increased.

These agree with the description originally given by Köhler, except that in his cases the outline of the bone was regular.

Recent X-rays, taken after an interval of six months, show that the bone has returned to normal size; its density is very much decreased, but it is apparently divided into two parts by a cleft.

The boy has now no symptoms.

Three cases of this condition were described by Köhler in 1908; twelve other cases have been recorded, all true to the original type. The condition is always found in boys between the third and the ninth years. Onset is, as a rule, gradual, but may be sudden. There is often an indefinite history of trauma, but in many cases no injury has occurred. Pain and lameness are the first symptoms, accompanied in most cases by swelling and tenderness over the scaphoid. One case showed local heat and redness. The condition may recover and relapse again, but the ultimate tendency is towards cure, irrespective of treatment. Köhler stated that the disease is liable to extend over two to three years. The scaphoid is then said to return to normal.

The X-ray appearances are very characteristic:—

(1) Half to a quarter smaller than normal.

(2) Form generally regular, but may be irregular.

(3) The architecture impossible to recognize, cortex and spongy portion running together.

(4) Density increased two to fourfold.

Pathology.—Various suggestions have been made as to the origin of the condition:—

(1) Compression fracture (Stumme), negatived by the absence of trauma, gradual onset, and the rapidity with which it heals.

(2) Tuberculosis.—Fassett believes it to be a tuberculous lesion, healed and sclerosed. Most authorities agree that the condition is not tuberculous, on account of its clinical course. Hetzel investigated his case carefully, and the von Pirquet and Wassermann reactions were negative.

(3) Preiser reported several cases of typical fracture of the carpal scaphoid, with the following mechanism: Following slight injury, there has been disturbance of surrounding ligaments, causing pressure on blood-vessels, with resulting defect in blood supply to the bone and rarefaction of the latter, with finally compression fracture. He argues that a similar sequence may occur in the foot.

(4) O'Brien thinks the condition is due to delayed development of the ossific centre of the scaphoid, which is stimulated to osteogenesis by trauma—though the agency of pathogenic organisms has not been ruled out. Pain and swelling may be due to post-traumatic osteitis.

I think the most interesting facts in the ætiology are that in one case reported by Köhler a similar process was discovered in the patella of the same side, and that the patient in Rotch's case had a twin brother in whom X-ray showed a similar condition in the scaphoid without symptoms. These two cases show that the condition is primarily one of defective ossification of the scaphoid. I am inclined to think that it is not a delayed development, as others have stated, but a premature ossification in the scaphoid, which is normally the last bone in the tarsus to ossify, this leading to diminished size and increased density. The small size may lead to weakness in the attachment of the scaphoid ligaments with subsequent stretching and weakness of the foot. Any strain will then give rise to pain and swelling.

Treatment.—Relief of the strain on the inner border of the foot by an appropriate boot or plaster of Paris, and exercise for the calf muscles.

I suggest that we call the condition "Isolated abnormality of the tarsal scaphoid." I believe it to be more common than is generally supposed.

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The PRESIDENT: This contribution is very interesting. The only unsatisfactory part is the difficulty in knowing what the condition is due to. I think it must be an error of ossification of some kind. One of the remarkable features is the extreme rapidity with which, apparently, the bone goes back to a normal condition: in a short time the whole appearance of the bone is completely changed. So it does not seem to be a premature ossification, nor a disease, but, as Mr. Maynard Heath says, an abnormal ossification of the scaphoid.

Splenectomy for Splenomegalic Cirrhosis.

By E. Bellingham Smith, M.D.

This patient first came to me in my out-patient department in 1911. He was a small puny child, and was said to be wasting: there was nothing definite to be found about him, except that he had a moderate enlargement of spleen and liver. At that date the spleen was about three finger-breadths below the costal margin, and the liver was two finger-breadths below. I had him under observation for nearly two years. In November, 1913, the spleen appeared to be actively growing, and the liver, after first enlarging, began to shrink. After three months' observation, the spleen was found to be down to the umbilicus, and the liver had contracted to below the costal margin. At about that time he had some bleeding from the mouth, and was said to have had hæmoptysis. He was admitted into hospital in March, 1914, when the spleen was practically resting on his left iliac crest. The liver had decreased in size and was not palpable. The blood contained 5,400 leucocytes, and that was maintained during two months of active antisyphilitic treatment. The blood gave a positive Wassermann reaction. Three months after admission, despite the antisyphilitic treatment, the leucocytes dropped to 2,400, and it was decided that something further should be done as the child was obviously going downhill.

The clinical features of the case suggested that it was one of splenomegalic cirrhosis, or Banti's disease, which had occurred in a boy the subject of congenital syphilis. In August I asked my surgical colleague, Mr. Blundell Bankart, if he would care to remove the spleen. He did so. For two days following the operation the pulse-rate was 170, and the respirations increased to fifty per minute, both being probably due to the shock produced by removing such a large organ. At the operation the liver was found to be cirrhotic. The child made an uneventful recovery. Twelve days after the operation he had a leucocytosis of 40,000, although a month before the operation the leucocytes were only 2,400. We thought there must be a mistake, and a further examination was made in a day or two, and the result was 32,000. The blood is now practically normal: 5 million reds, 80 per cent. hæmoglobin, leucocytes 9,400. The operation

was done five and a half years ago, and the mother says the boy keeps well and does the things ordinary children do.

I should like to ask whether any other members have any experience of the Wassermann test in cases of splenomegalic cirrhosis, and if so, what proportion have been shown to be positive? The causes underlying or giving rise to these giant enlargements of the spleen in childhood still seem to be obscure, and it would be interesting to know what part congenital syphilis exactly plays in their development.

DISCUSSION.

Dr. F. PARKES WEBER: I suggest that such cases as this should be grouped together as examples of enlarged spleen and (later on) cirrhosis of the liver in children with congenital syphilis, and that they should be sharply separated from those cases which have been described under the name Banti's disease. I once wrote a paper connected with that subject,[1] and I think the evidence is very strong that there is some unknown connexion between congenital syphilis and the condition under discussion, namely, enlargement of spleen, a tendency to epistaxis, and finally cirrhosis of the liver. I do not think that the disease of the spleen and liver is due to the local presence of spirochætes, and I believe also that severe anti-syphilitic treatment (by mercurial inunction, &c.) is dangerous in some such cases. I congratulate Dr. Bellingham Smith on the as yet satisfactory result of the operative treatment.

Dr. F. J. POYNTON: The last case of Banti's disease I had proved fatal. It was that of a girl aged 14 years who died of terminal jaundice with hæmorrhages. That patient was proved, post mortem, to have a cirrhotic liver, which we had recognized, with the enlarged spleen. The Wassermann reaction, which was done on several occasions, was negative each time. At first this was thought to be a syphilitic case, but there was no evidence of that. If we are going to put great faith in Wassermann's reaction then this is a fairly conclusive example of *non-syphilitic* Banti's disease. It is the only case I can remember likely to give any help in the inquiry made by Dr. Bellingham Smith.

The PRESIDENT: I am very glad Dr. Bellingham Smith has brought forward this case, because, surgically, it is so interesting to know what happens in these cases years after the surgeon has removed the spleen. Often surgeons are called upon to remove a spleen, but they are not aware of what happens to the patients some time afterwards.

[1] F. Parkes Weber, "Splenomegaly with Recurrent Jaundice, ending in Hepatic Cirrhosis and Ascites; with Remarks on the Splenomegaly of Inherited Syphilis in Children," *Proc. Roy. Soc. Med.*, 1911, iv (Sect. Study Dis. in Child.), p. 62.

Dr. BELLINGHAM SMITH (in reply): It is a common failing at meetings of this character to exhibit one's successful cases and ignore one's failures. Some little time before the spleen was removed from this patient I had a similar operation performed in a case of acholuric jaundice. The child had an extremely rapid pulse and respiration, and died in a condition which suggested massive collapse of lung on the same side, or pneumonia. The operation should therefore not be undertaken without due consideration. From what Dr. Poynton has said, I think it is clear that all cases of Banti's disease are not necessarily syphilitic.

Dr. F. PARKES WEBER (in further comment): As I have ofen stated, I believe that leucopenia is found in nearly every chronic non-leukæmic enlargement of spleen—including splenomegaly, due to chronic malaria, chronic kala-azar, chronic syphilis, tuberculosis, Hodgkin's disease, &c. It is a mistake to suppose it occurs exclusively in so-called Banti's disease.

Family Osteo-arthritis.

By RICHARD ARMSTRONG.

HAVING regard to the extreme prevalence of acute arthritis in childhood, particularly in association with rheumatic fever, the occurrence of chronic arthritis of whatever form is by no means frequent. Indeed it may fairly be said that the only variety of long continued joint disease commonly encountered in early life is the group of rheumatoid affections, associated with the name of Dr. Still. Again, the importance of a family tendency to acute rheumatic fever is widely recognized, although whether the tendency be due to the passage of a specific virus from parent to offspring or to a true inheritance of a degree of resistance to rheumatic infection below the average, is open to discussion. It will be readily understood, therefore, that a form of chronic joint disease in a brother and sister having the characters of an osteo-arthritis and moreover the very remarkable history that a similar condition had affected four successive generations of the same family at once aroused interest.

In August of this year a boy, aged 12 years, was brought by his mother to Dr. H. Thursfield's out-patient department at St. Bartholomew's Hospital. The boy complained of pain in the left knee-joint, and had had occasional attacks of indefinite pains in the joints since the age of 4 years, definitely localized in the left knee

at the age of 7 years, but had never complained of his hands, although his mother noted that his finger-joints were swelling as early as the age of 4 years. A year ago he had general rheumatic pains affecting most of the body, was not feverish, but had a slight swelling of the left knee and ankle-joints, which were tender to the touch. No abnormality of the left knee-joint was visible, and nothing could be found on palpation, nor was there any limitation nor undue freedom of movement in the knee and ankle; the other major joints were normal. Very marked enlargement of the finger joints was present—the more pronounced for the relative slenderness of the phalanges themselves, due to wasting of the soft parts, and this condition was especially definite in the distal interphalangeal joints, particularly of the second and third fingers. There were well marked knobs or bosses on the sides of the distal joints which could be fairly likened to the Heberden's nodes characteristic of the type of osteo-arthritis of the smaller joints which occurs in adults. Skiagrams showed only atrophy of the ends of the shafts and epiphyses, but it appeared not improbable that later in life bony overgrowths might occur in the bosses, although these consisted at the time of examination only of cartilage infiltrated to so slight an extent by bone salts as to give no shadow on the screen. The terminal phalanx of the little finger of each hand had undergone actual pathological dislocation towards the radial side. No grating could be felt, and no evidence of erosion of cartilage could be obtained, but movements of the fingers were undoubtedly limited, and the boy was unable to make a satisfactory fist. A similar but less advanced condition existed in the toes, and his habit of everting his left foot when walking expressed an unconscious attempt to lessen the stress of walking on the toes of that foot, of which he complained more than of the right. In all joints the pathological condition was articular, with wasting of surrounding tendons and tissues, rather than peri-articular, in sharp contrast to the well-recognized disease known as rheumatoid arthritis in children.

Fig. 1 is a reproduction of a photograph of the hands of the boy (observers' left), his mother (centre), and his sister, and demonstrates satisfactorily the condition of the fingers described above.

Skiagrams of the hands (fig. 2) showed an atrophic condition of the heads of the middle and to a less degree of the proximal phalanges, with a rarefaction and partial pathological dislocation of most of the middle ones. No osseous lesion was described by the radiographer in the left knee-joint, but there was an appearance of irregularity about the margins

of the femoral epiphysis and on the inner side of the head of the tibia, which in the opinion of the author was abnormal.

The bony changes in the fingers, and particularly the characteristic radial dislocation of the terminal phalanges, is well shown in the print of a skiagram of the hands of the boy, his mother and sister, taken from the dorsal aspect (boy's hand on observers' right) (fig. 2).

In appearance the boy was rather pale and small and below the average physical development for his age—weight 4 st. 2½ lb., height 4 ft. 7 in. He was bright and intelligent, in fact at times so restless and excitable as to have suggested chorea, though he was certainly not suffering from any such condition. No focus of infection was found in the nasopharynx or teeth, his appetite was fair, his bowels regularly opened, his lungs showed no evidence of disease, his heart was natural, and his urine contained no abnormal constituent. There was no enlargement of lymphatic glands, and the spleen was not palpable.

His sister, aged 11 years, showed a similar but less advanced condition of the finger-joints, the bony changes being relatively slight, but as in her brother's case, first noted by the mother at the age of 4 years. As in his case, the condition was slowly progressive, but she is to all appearance otherwise a healthy child.

The mother had a typical osteo-arthritis of the smaller joints, which she said was slowly becoming worse. She, too, had begun to suffer from the disease in early childhood. Her hands were stiff and at times painful, but her other joints were not affected.

There are two children in the family, both girls, aged respectively 3 years and 9 months and 9 years, who are free from the condition and perfectly healthy. On the other hand one child of the maternal aunt—i.e., a first cousin—a girl aged 20 years, developed a similar affection of the joints in childhood and has crippled fingers now, and the maternal grandfather and great grandfather were also affected.

The relationship of healthy individuals to those affected is demonstrated by the diagram of the family tree, fig. 3, shown herewith, affected individuals being underlined.

Dr. Thursfield devoted considerable time to a search through current literature but failed to find any reference to a condition similar to that described. Dr. Still, in his original paper on "Rheumatoid Arthritis," describes also a chronic fibrosis of joints in rheumatic fever and a type of chronic joint disease associated with bony changes, of syphilitic origin, but neither of these conforms to the type of disease under discussion. Rolleston mentions rare cases of chronic arthritis in



FIG. 1



FIG. 2.

children with much destruction of cartilage, but these he considers in no way comparable to the osteo-arthritis of adults. It would seem, therefore, that this form of chronic arthritis, beginning in childhood and affecting successive generations of the same family has not been described hitherto, and such was the view taken by Dr. Parkes Weber and others when the cases were first demonstrated. It is probable, however, that chronic joint disease of osteo-arthritic type, does from time to time occur in childhood, although descriptions have not found their way into the literature of the subject. But the cases are possessed of an interest apart from their rarity and their remarkable family history. Just as Still's disease in children is strictly comparable to

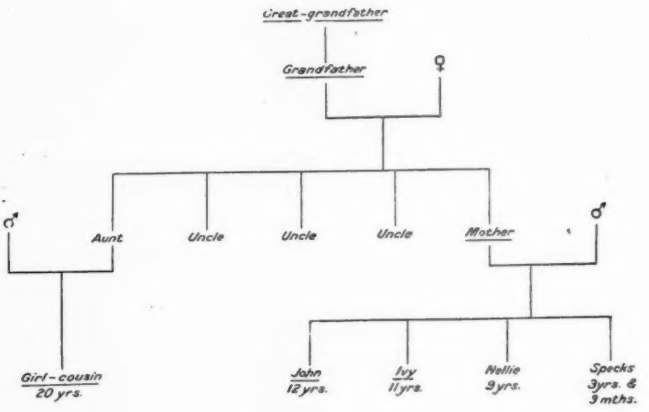


FIG. 3.

rheumatoid arthritis in adults, so does this form of osteo-arthritis resemble the small-joint type of disease recognized by the homely title of "washerwoman's" hand.

It is believed by many that osteo-arthritis, being incident in later life and old age, is truly a disease of degeneration rather than due to an infective agent. May not the cases described be considered to shed further light on this point? It is surely unbelievable that the same micro-organism or toxin has successively affected so many individuals belonging to no less than four consecutive generations.

Families are known which suffer from premature arterial degeneration of unknown cause; is it not possible, therefore, that the children represent a precocious form of joint degeneration due, it may well be,

to the action of a toxin, but susceptible to that toxin to an extent found normally only in old people?

In conclusion, I beg to express my thanks to Dr. H. Thursfield for allowing me to describe the cases, and to Dr. Finzi and Dr. Stone, of the X-ray department at St. Bartholomew's Hospital, for their great kindness in preparing and permitting me the use of the skiagrams and prints.

DISCUSSION.

Dr. F. PARKES WEBER: I feel very strongly that we have here to do with an absolutely new familial developmental disease. There are a great number of family developmental diseases: many families have produced developmental diseases of a peculiar type of their own, and this is an example. With this I would compare such minor deformities as the familial occurrence of peculiarly bent little fingers. If a name were needed, I should be inclined to call it "Thursfield's and Armstrong's familial osteo-arthritis-like deformity of fingers." I have known a condition resembling clubbed fingers occurring in several members of two families, and I published particulars about them in the *British Medical Journal* four weeks ago.[1] A similar family group has been published by von Eiselsberg in Vienna. The features of the familial condition shown by Dr. Armstrong somewhat resemble Heberden's nodes, and the recent opinion among those who have specially examined Heberden's nodes is, that they constitute a disease apart, with its own pathology, not related to gout or ordinary rheumatoid arthritis. We are much indebted to Dr. Armstrong for having brought the case, and I hope his description in the *Proceedings* will be very complete and well illustrated, as it may be years before such a family group is described again, though similar groups are certain ultimately to come under observation.

Dr. COCKAYNE: Is there any hope of Dr. Armstrong being able to see the mother of the first cousin who was so affected? because it seems to be so very unlikely that she actually has no sign of this disease, and yet transmits it. I am inclined to regard the disease as a degenerative familial one comparable to the myopathies and to Friedreich's ataxia.

Dr. F. J. POYNTON: Is it so very rare to see these rheumatoid affections passing on in families? I think I can recall cases, often starting with a gouty parentage, coming on at different ages. I can remember a case in which it was in the grandmother, and the son is suffering from it, and that man's son and his daughter are suffering from an obstructive form of arthritis, and they recognize it as being a family disease. So I am not so much impressed with that as I am with the age.

[1] F. Parkes Weber, "The Occurrence of Clubbed Fingers in Healthy Persons as a Familial Peculiarity," *Brit. Med. Journ.*, 1919, ii, p. 379.

Section for the Study of Disease in Children.

President—Mr. J. P. Lockhart-Mummery, F.R.C.S.

Chronic Jaundice; Congenital Defect of the Bile-ducts.[1]

By Edmund Cautley, M.D.

Male, eighth child, born on February 13, 1919, at full time and apparently healthy. Jaundice appeared on fourth day and has persisted.

Admitted to Metropolitan Hospital from April 4 to 12, and from September 18 to November 23. At first he was breast-fed, well nourished, 9½ lb. in weight, jaundiced, and presented acholia, bilinuria and a large liver. The degree of jaundice varied. He was readmitted with temperature 103° F., falling to normal next day. He weighed 13 lb. There was a history of diarrhœa and vomiting for two days, but neither symptom occurred after admission. Jaundice was still present, but had been clearing up recently. The liver was large and hard, and the spleen enlarged.

During his stay in hospital he had a variable degree of fever, due to bronchial catarrh. Acholia and bilinuria have been persistent and the weight has varied from time to time, according to the amount of catarrh and his appetite for food, which consisted of milk and water, with Horlick's malted milk and Benger's food. The liver has decreased somewhat in size and become harder and irregular; and the spleen has got larger and harder. The Wassermann reaction is negative.

The case is interesting from the point of view of diagnosis and the duration of life. I am disinclined to think it one of cholangitis, in view of the absence of febrile disturbance in the early stages. On the other hand most cases of congenital obliteration of the bile-ducts only live six to eight weeks. Life is, however, sometimes prolonged for

[1] At a meeting of the Section, held November 28, 1919.

eight or nine months. Dr. Kenneth Kellie showed a patient of this type in 1911, a male baby aged 5 months, who died subsequently under my care at 7 months of age, and had a large, smooth, green liver, a hard spleen, a rudimentary gall-bladder and a common bile-duct represented by a fine cord. Slight jaundice had been present since the fourth day of life. It is curious that the jaundice is slight and variable in degree in some of these patients.

DISCUSSION.

Dr. F. PARKES WEBER: The curious thing is that this child should have lived so long. But there have been cases recorded which were supposed to be due to congenital defects of the bile-duct, in which the child has ultimately recovered. There seem to be considerable differences in these cases, in regard to the extent of the defect or occlusion, as proved by post-mortem examinations. In a case in which the congenital occlusion affected merely the cystic duct, the abnormality would probably not cause the child's death. If only the distal part of the common duct was occluded, the patient might possibly be relieved by an operation. Supposing that in the present case the whole of the common bile-duct was obliterated, the bile might be let out from a large intrahepatic bile-duct, as it was in another kind of case which was under my observation at one time. In that case[1] one of the intrahepatic bile-ducts was opened through the abdominal wall, and the bile was allowed to run out by a fistula in the front of the abdomen. But that is an unsatisfactory kind of operation, as there is a great tendency for the fistula to close spontaneously. In these remarks I have not entered into the question as to whether the lesion is merely developmental or is due to intra-uterine inflammation.

Dr. COCKAYNE: I would go even further than Dr. Parkes Weber. I think the cases of so-called congenital obliteration of the bile-duct belong to the group of toxæmic jaundices in children. In some of them severe blood destruction takes place. Most of them die soon, others recover, and in these the anæmia remains for a long time after the jaundice has disappeared. In some there is much more cholangitis. In the cases which do not die quite early these obliterative changes may take place. They do not die directly of the toxæmic changes, but of obliterative cholangitis. In other cases the brunt of the disease falls on the liver cells, and in that organ there are severe fatty changes and necrosis. The milder cases may recover; those of moderate severity die slowly, and show signs of commencing cirrhosis of the liver. The most severe die very rapidly. I think they all belong to the same group: it is simply a question of degree. In some the disease falls more on

[1] Weber and Michels, "A Case of Chronic Jaundice and Great Enlargement of the Liver, due to Primary Carcinoma of the Extrahepatic Bile-ducts, commencing at the Junction of the Hepatic Ducts," *Med. Chir. Trans.*, Lond., 1905, lxxxviii, p. 247.

the liver cells, in others on the bile-ducts, in others again on the blood. In this case the damage seems to have been done chiefly to the liver cells and bile-ducts.

Dr. CAUTLEY (in reply): It is difficult to understand why a child like this should not develop jaundice until the third or fourth day of life, and why the jaundice is not more severe. Dr. Kellie's patient did not develop jaundice until the fourth day. It lived seven months, and was found to have complete obliteration of the common bile-duct. It is possible that the cases which recover may have complete obliteration of the common duct, but have an aberrant bile-duct, such as has been found, for instance, in some cases of duodenal atresia, in which bile has been vomited although there has been complete atresia above the entrance of the duct into the duodenum. I regard the prognosis as practically hopeless, even though there is no very severe jaundice. The changes in the liver have been steadily progressing, and it is obviously cirrhotic; also, the spleen is enlarged.

Additional Note.—Death occurred at the age of 11½ months from broncho-pneumonia. Post mortem: Obliteration of the common bile-duct, absence of the gall-bladder and fibrosis of the liver.

Specimens from Four Cases of Punctate Ulceration on the Stomach in Infants.

Shown by ERIC PRITCHARD, M.D., and W. T. HILLIER.

THESE specimens were all taken from cases of young infants who died of a peculiar form of gastritis occurring in cases in the wards of the Queen's Hospital for Children; they show numerous and very minute areas of ulceration; excessive vomiting was characteristic of all the cases, and in most of them blood-stained mucus was present in the vomit. Similar ulcerations of a follicular character were also found in the intestines.

Specimens from a Case of Hypertrophic Pyloric Stenosis with Associated Hypertrophies.

Shown by ERIC PRITCHARD, M.D., and W. T. HILLIER.

THESE specimens were taken from a baby aged 3 months, who died of hypostatic pneumonia; they show great hypertrophy of the lower end of the œsophagus, of the pylorus, and of the ileo-cæcal sphincter.

At the cardiac orifice there is a polypus-like growth of mucous membrane about the size of a small pea. The stomach *intra vitam* had a capacity of about 4 oz. The transverse colon opposite to the greater curvature of the stomach was dilated.

Macroscopically the adrenals were normal.

The interest of this case lies in the association of hypertrophies at three points in the alimentary canal, which it has been suggested may be dependent on overaction of the adrenals either before birth or after, or on want of compensation by antagonistic secretions.

I thought these specimens were of interest, in view of Mr. Tyrrell Gray's paper, in which he suggests that all these hypertrophies may possibly be due to some uncompensated action of the adrenalin secretion. Unfortunately in my case the adrenals were not examined microscopically, but Mr. Hillier says that macroscopically there was nothing abnormal about them. There is a curious feature about these specimens, because although the hypertrophy and stenosis of the pylorus, as also of the cardiac orifice, were considerable, there was no difficulty in gaining entrance to the stomach with an ordinary catheter, and apparently the stomach contents passed freely through the pyloric sphincter, because the motions were never very scanty, and after the child had been in hospital about three weeks, it had natural motions. Therefore I imagine that such stenosis as exists was brought about without any great degree of spasm.

DISCUSSION.

Mr. TYRRELL GRAY: This kind of case interests me very much from several points of view. One is, that it is very unusual to find cases with multiple hypertrophy. And Dr. Pritchard raised the question of the influence of the adrenals in the production of hypertrophy. The supposition that hyperadrenalism is a cause of hypertrophy before birth does not necessarily mean that the suprarenals are at fault in the child. I think possibly the suprarenals in the mother may be at fault. And there is something to support that in the fact that I have been given to understand (it is second-hand information and came to me very indirectly) that Dr. John Thomson was said to have stated that he saw more cases of pyloric hypertrophy during the war than at any other time. If that is true, coming from an observer of such standing, it seems to support the possible view that the mother's suprarenals may be at fault. I have also myself seen infinitely more such cases during the war, but whether it is a matter of cause and effect I do not know. There is also the fact that the majority of the children so affected are first babies, which means that the mothers were subjected to a period of greater anxiety in labour and delivery. The effect of mental strain on the exhaustion of the supra-

renal glands is well known, and it is not impossible that this condition may be an accompaniment of hyperactivity in the mental sphere. I would like to know whether this child was a boy, and whether it was its mother's first child. A more interesting feature still is, that though there is such typical pyloric hypertrophy, the child never showed signs of obstruction. That is of interest to me because Dr. Pirie and I believe many children are born with pyloric hypertrophy who never develop symptoms of it, and so never come under medical examination. There are two classes which do. The first is that in which the hypertrophy is so excessive that the anatomical condition prevents food passing through, and so symptoms develop early. The other class, I think, might never develop symptoms if it were not for what one might call some "side show," or some pathological condition, such as might be associated (to mention one important factor) with the external genitals. For I do not think the profession as a whole has yet sufficiently realized the fact that sexual impulses are perhaps the most important impulses in physiology (as of course it is proper they should be), since on them depends the propagation of the species, and any pathological condition associated with the external genitals is bound to have far-reaching effects. We have laid stress on the association of phimosis with the development of symptoms of gastric obstruction, and I think the slight evidence of precocity in the external genitals in many of these cases is some indication of this close relationship. I may quote the case of a twin—a boy and girl—both premature; I saw them, I think, last August. The boy was very small, and when a week old developed complete obstruction of the stomach. The child was sent to me, and he had visible peristalsis and a tumour could be felt. The girl never developed symptoms. The boy was circumcised, was sent out, and never had any return of the symptoms. I have not yet been able to examine the girl, but I intend to do so.

Dr. PORTER PARKINSON: How is the fact accounted for that the adrenal condition, if present, picks out certain sphincters, certain parts of the alimentary canal? If the condition had been due to abundance of adrenal secretion, the other unstriped muscle would have been affected too, for instance that of the uterus. I do not know whether that is so. I could not hear Mr. Tyrrell Gray's remarks plainly.

Dr. E. CAUTLEY: The association of hypertrophy or hyperplasia in three different regions is specially interesting, because one of the arguments which has always been advanced against this hypertrophied condition of the pylorus in infants being congenital is, that such hypertrophy does not occur elsewhere. But I would like to urge many points against this hypothesis of hyperadrenalism being the cause. To believe this, we should have to accept the view that hyperadrenalism is present, and that when present it excites spasm of these particular parts, and, thirdly, that the spasm will cause hypertrophy. A hypothesis dependent on three assumptions stands in need of more support than has been advanced. It has been urged that hyperadrenalism may be transmitted from the mother. There is no need to make another

assumption, because a relative hyperadrenalism is probably present in every fœtus. The adrenals are as large as the kidneys before birth (Keith), and do not decrease until some time after; the cortex is much greater than the medulla. And the point has been made that the condition occurs much more frequently in boys than in girls, and it has been suggested that reflex stimuli from the penis, due to phimosis, may cause the spasm. But you may take that argument and twist it the other way round. If hyperadrenalism is the exciting cause, why is not this disease present as frequently in girls as it is in boys? It is four times as frequent in boys. As to phimosis, I have seen cases in Jewish boys in life, and in Gentile children who have been circumcised. There is no proof of this hyperadrenalism being present. I ask whether injections of adrenalin induce further spasm or prevent relaxation. Another point which bears strongly on this is the observation that in marasmic infants the adrenal glands are distinctly wasted, and there is a great deficiency of adrenal secretion. Wasting should, therefore, induce hypo-adrenalism, relaxation of the spasm, and cure of the hypertrophy. It may also be urged that if hyperadrenalism has anything to do with the hypertrophy, why does it influence the pylorus only in the first three months of life? It is exceptional to see pyloric hypertrophy at a later age.

The PRESIDENT: The whole question of these hypertrophies is extremely interesting, and badly needs working out. It always seems to me that the chronic hypertrophy in these cases of pyloric stenosis is really a normal hypertrophy, that there is nothing pathological about it; and it seems difficult to realize that Nature will kill a child by a normal hypertrophy. I think the hypertrophy must be secondary to spasm, that the initial cause is not the hypertrophy but congenital spasm of the pylorus: it is this spasm which kills. We find a more marked case of that in megalocolon, in which there is a tremendous hypertrophy of the musculature of the bowel at one part, but with no traceable stricture in front of it, and in some cases the dilatation reaches the anus, which is normal. There is something here which we do not understand. During the last fifty years we have been studying conditions from the anatomical standpoint, but surely there can be functional congenital defects apart from anatomical defects. Is it not possible that the hypertrophy in megalocolon is due to reversed peristalsis in some section of the gut below the point of hypertrophy? This is what is found to occur in the monkey or dog in experiments in which 2 or 3 in. of intestine are reversed and the intestine is re-anastomosed: there is a hypertrophy immediately above the point where the reversal has been brought about. I think these cases of congenital pyloric stenosis also have a congenital defect of the neuromuscular apparatus resulting in spasm of the pylorus, and possibly reversed peristalsis below where the spasm occurs.

Dr. PRITCHARD (in reply): The child was a boy. There is no note as to whether there was phimosis or not. I do not defend the adrenal theory of these hypertrophies, but Dr. Parkinson asks whether there were no hypertrophies

in other parts. In Still's case, to which Mr. Tyrrell Gray refers, I believe there was hypertrophy of the sphincters of the two ureters. As to why there should not be hypertrophy of other parts of the bowel, I think that is a peculiarity of the condition associated with hyperadrenalism, its selective action on the nerve mechanism or nerve strands which control these sphincters. The effect of adrenalin is not to cause hypertrophy of circular muscles but rather relaxation except in the case of these particular sphincters in which the action appears reversed. I do not know about the uterus, but it is supposed to be owing to the embryological unity of the adrenal bodies with the sympathetic that adrenalin acts as a reinforcer of the sympathetic system. I have been much struck with this adrenalin theory, though I think there are weaknesses in it. We seek for a common cause in these hypertrophies, and if the nervous mechanisms which control the muscles concerned are highly sensitized by a relative excess of adrenalin secretion, or some other hormone, it offers a plausible explanation. I do not understand why excessive adrenalin secretion in the mother should produce a like excess in the fœtus. If the effect is comparable to thyroidin it should act in the opposite way. It has been proved that when there is a deficiency of thyroid in the mother (myxœdema) the fœtus compensates the defect by manufacturing for itself thyroidin in larger amount.

Mr. TYRRELL GRAY (in further comment): One is liable to wander in such a discussion as this, into the action of the various endocrinic organs, and in such an involved subject it is difficult to know when to limit one's comments. As to why other parts of the bowel have not hypertrophied, the explanation is a simple one: the sympathetic supply to the intestine is inhibitory to circular muscle, and Elliott has shown that reversed innervation takes place in the sphincter areas in the intestine. Any stimulus, therefore, which inhibits the bowel muscle will cause a concurrent spasm of the sphincter areas. Sympathetic nerve endings in involuntary muscle are acted on by both nerve stimuli and suprarenal hormone. Hence hyperadrenalism would inhibit the contraction of the circular muscle except at the sphincters, when spasm would result. I think Dr. Cautley has rather mistaken one's whole attitude on this question, for his comment on the prevalence in males is one of the points we elaborated in support of our views. I am interested to learn that the condition occurs in Jews, because in five years, among eighty cases, we have not been able to discover a Jew case. Though hypertrophy must undoubtedly occur in Jews, the development of symptoms, I think, must be very rare. I do not think there is any selective action in hyperadrenalism in boys as compared with girls; it is only the development of symptoms which is commoner in the former, as I have explained. As the hypertrophied portion grows, so the cavity will become larger, and the tendency to obstruction diminish with time. I do not see how true hypertrophy could occur except by over-action, and there must be some general cause which produces the over-action. No better explanation of such over-action has, I think, been offered than Dr. Pirie's

suggestion of hyperadrenalism. Unless there is enough obstruction to bring the child under observation, I do not see how we are likely to "spot" the condition, unless we examine the pylorus of every baby. In consequence it is a legitimate assumption that hypertrophy of some degree exists in many subjects undetected, because it is not of sufficient degree to cause symptoms. I place Dr. Pritchard's case in this category.

Extensive Primary Tuberculous Disease of the Heart.

Shown by T. H. and H. O. GUNEWARDENE.

THIS interesting specimen is from a boy, aged 6 years, who died at the East London Hospital for Children, on June 26, 1918, ten days after admission. The heart and pericardium weighed 12 oz. Both ventricles were hypertrophied and dilated. In the right ventricle, just above the attachment of the tricuspid valves, there are two nodular masses about 1 cm. in diameter projecting into the cavity and probably touching one another during life. There are no valvular lesions. On section one could trace small isolated areas of white firm growth infiltrating the inter-auricular septum. This infiltration is widespread in the wall of the left auricle, and there are isolated patches at the root of the pulmonary vein. The involvement of the inter-auricular septum is very complete. The growth surrounds both auriculo-ventricular rings, and is continuous from side to side. The endocardium over the growth is smooth and not eroded. The visceral pericardium was completely adherent and shows no tubercles. This is essentially a disease of the myocardium.

Sections of the growth show caseation and giant cells but no tubercle bacilli were seen, though many sections were specially stained for its identification. A piece of the growth, macerated and stained by Lorenz's modification of the antiformin method, shows the bacilli which you have just seen. There was no naked-eye evidence of tuberculous affection elsewhere, the only changes in the other organs being those secondary to any uncompensated heart. This could be looked upon as a primary tuberculous disease of the myocardium.

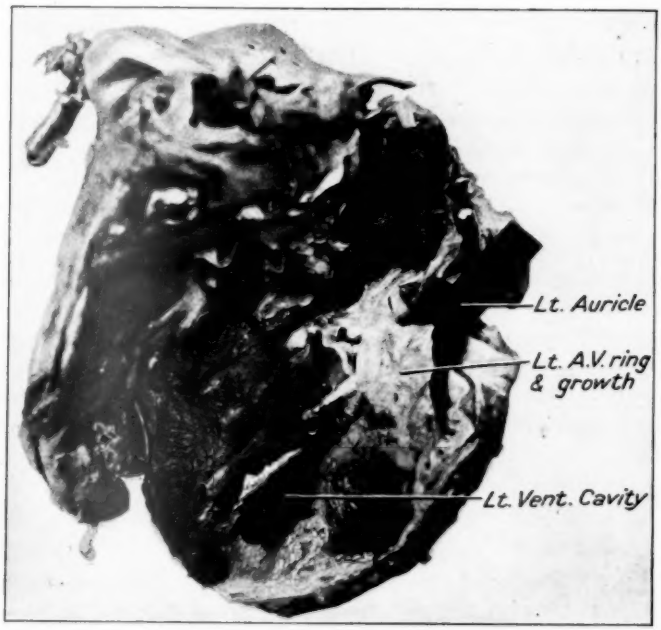
The points of interest are:—

(1) Extreme rarity of so extensive a tuberculous lesion of the heart.

(2) History of only two months, judging by the onset of the symptoms.

(3) Absence of any signs suggestive of a primary cardiac lesion apart from hypertrophy and dilatation with failing compensation.

We are indebted to Dr. Clive Riviere for allowing us to show the specimen before this Section.



DISCUSSION.

Dr. F. PARKES WEBER: I would ask Mr. T. H. Gunewardene how many cases of tuberculous disease of the myocardium or of the heart valves he has found records of. I have come across a considerable number recorded at the Paris medical societies, but few have been recorded in England.

Mr. T. H. GUNEWARDENE (in reply): So far I have been able to trace forty-one cases of various degrees, but only one in which there was such an extensive invasion as in this; but this is the only case that could be considered primary.

Case of Cirrhosis of Liver and Spleen.

By Reginald C. Jewesbury, M.D.

Boy, aged 4½ years. Admitted to St. Thomas's Hospital on October 30, 1919. Healthy at birth, full time; breast fed. Jaundice when 2½ years old, after this the "stomach started to swell." Has had attacks of epistaxis but no hæmatemesis or passage of blood *per rectum*. He used to suffer from frequent attacks of vomiting but has had none for the past four months. His mother said she has been in the habit of giving him gin, a teaspoonful at a time, in hot water, "on and off" since he was 12 months old up to the time he was brought to the hospital.

Family history: Father dead, cause unknown; mother healthy; one other child healthy.

On examination: A pale thin faced boy, undersized for age, with a very distended abdomen. The superficial veins in the abdomen are dilated. The liver is much enlarged, the edge is felt 2 in. below the level of the umbilicus; the liver is irregular (hobnail) and very hard but not tender. The spleen is much enlarged and hard, it reaches to the level of the umbilicus. The abdomen is dull to percussion all over but a fluid thrill is not definitely obtained, nor is shifting dullness definitely made out. Chest: Heart, occasional soft systolic murmur at apex. ? Congenital heart; lungs normal; fingers and toes show well marked "clubbing." Blood: Wassermann reaction negative. Blood count: Red cells, 2,810,000; white cells, 2,320; hæmoglobin, 58; colour index, 1·0; polymorphs, 59·5 per cent.; lymphocytes, 36·5 per cent.; eosin, 2·0 per cent.

DISCUSSION.

Dr. F. Langmead: I thought the child had congenital heart disease, and I based that view on the fact that the murmur seemed to be loudest in the pulmonary area, and was heard fairly clearly in the back, in the usual position to which a congenital pulmonary murmur is conducted. If that is correct we have an explanation for the clubbing of the fingers and toes and the dusky appearance of the patient, apart from the liver condition. Clubbing of fingers and toes is an important part of Hanot's cirrhosis, but I do not know that it has been described in association with cirrhosis of the multilobular type.

Dr. F. PARKES WEBER: I think the evidence here does really point to this being a case of hypertrophic alcoholic cirrhosis in a child, though ordinarily that is a diagnosis which I mistrust. The child has, so it seems, been given, at intervals, a dangerous dose of a toxin of the hepatic cells, resulting in a destruction of some of those cells from time to time, accompanied by an increase of the interstitial connective tissue—a sort of "substitution-cirrhosis." Such a case may be contrasted with acute or sub-acute hepatic atrophy following chloroform or some other poison of the kind, which destroys liver cells in large amount, so as to cause a more rapidly fatal result.

Dr. JEWESBURY (in reply): I am interested to hear of the paper mentioned by Dr. Langmead. I was not aware that in cases of cirrhosis of the liver in children it had been proved that alcohol was such a frequent cause. This would tend to show that the tissues of a young child may be easily damaged by the administration of alcohol and should make us hesitate before giving it even for therapeutic purposes. I think there is no doubt that this child was given alcohol (gin) over a long period and that it was the cause of the cirrhosis. The child eats and digests his food well, and is of a lively and precocious type. In contrasting this case with a similar condition in the adult, it is interesting to note the size of the liver and spleen. In this case both these organs are very much enlarged, whereas in the case of the adult the liver is usually atrophic when the cirrhosis is so advanced, and the spleen is very rarely, if ever, as large as is the case in this child.

Isolated Disease of the Scaphoid Bone.

By E. A. COCKAYNE, M.D.

B. W., AGED 4½ years. The girl is a cretin and has been a regular attendant at Great Ormond Street since the age of 2 months. For the last year she has complained on and off of pain in the left foot, and at times has walked with a limp. The mother thought she was suffering from "growing pains" and attached little importance to it. Lately the child has become worse. She complains of pain in the foot, and limps. There is some tenderness over the dorsum of the left foot, but no redness nor œdema. The X-ray shows normal ossification in the bones of the right foot, but in the left foot the scaphoid is very narrow, about half the normal width, very dense and the structure is indistinct. The outline is sharp except for four small projections. The other bones of the left foot are normal. There is no evidence of tuberculosis in the child and no history of tuberculosis in the family. The condition appears to be that which is sometimes known as Köhler's disease, of which

an example was shown at the last meeting of the Section.[1] The unusual features are the early age of onset and the fact that the child is a girl.

DISCUSSION.

Mr. B. WHITCHURCH HOWELL: May I suggest a method of treatment? It is that this child be put into plaster of Paris until the valgus condition, which is very evident, and probably due to the diminished pull of the tibialis posticus on a relatively smaller bone, has been corrected. When X-rays show that the scaphoid has regained its size, the child should have an external iron with a valgus T-strap, also wedging the inner side of sole and heel—on orthopædic principles.

Dr. COCKAYNE (in reply): I should not have guessed what the condition was had it not been for Mr. Maynard Heath's paper at the last meeting of the Section. It struck me that the symptoms in this case were so similar, that I had a skiagram taken. I thank Mr. Howell for his suggestion of treatment; it will be started immediately.

[1] *Proceedings*, 1919, xiii (Sect. Study Dis. in Child.), p. 17.

Section for the Study of Disease in Children.

President—Mr. J. P. Lockhart-Mummery, F.R.C.S.

Notes of a Case of Œsophagectasis in an Infant, with Radiograms.[1]

By Frederick Langmead, M.D.

The patient, a girl infant, was born at full term by a normal labour. She was a very small baby, weighing no more than 4 lb. at birth. Shortly after birth it was noticed that "something was wrong with her stomach." On the first day she retched and "vomited," even before her first feed. She was breast fed for one week, during which time she "vomited" occasionally, and her stools were green. At the age of one week she was weaned and put on cow's milk and water. She was now constipated, but the "vomiting" continued; it occurred about an hour after food, was not projectile, and was often brought on by change in posture. At 4 months she was wasted, and weighed "no more than 3 or 4 lb." Afterwards the "vomiting" was not so frequent, and occasional feeds were retained. The constipation continued, hard small stools being passed. After her feeds she used to be very quiet, pale and "collapsed," but recovered, and the colour returned after "vomiting." She slowly gained weight and reached 14 lb. 5 oz. at a year and 3 months old, one month before admission. For three or four months before admission the "vomiting" followed immediately after or coincided with a meal, and the food "did not seem to enter the stomach at times." The vomit was unaltered milk, sometimes accompanied by mucus.

Family history: The father and mother are both healthy, and there is no history of neurasthenia or neuroses on either side of the

[1] At a meeting of the Section, held January 23, 1920.

family. The patient is the third child in a family of three, the elder members being quite well. No miscarriages.

November 28, 1919: On admission to the Hospital for Sick Children, Great Ormond Street, the child was found to be fairly well developed and nourished, but with a pale face, slightly sunken eyes, and hanging folds of skin, indicating recent loss of fat. The fontanelle was slightly depressed. There were no signs of rickets. Examination



FIG. 1.

Radiogram taken immediately after a bismuth meal. Some food in the stomach; the remainder held up in the œsophagus.

of the abdomen revealed no tumour nor gastric peristalsis. Heart and lungs were apparently normal. The knee-jerks were very exalted, otherwise there were no abnormal nervous phenomena. The urine contained acetone in definite amounts.

After admission the child cried incessantly, apparently without pain. A stomach tube was passed, but no food was washed out. During

the first twenty-four hours the child refused all food, and was fed by a tube; she vomited afterwards. Next day she took equal parts of milk and water a little better, but cried almost continuously if not nursed or petted. A single vomit contained mucus; the milk was not digested and contained no curd.

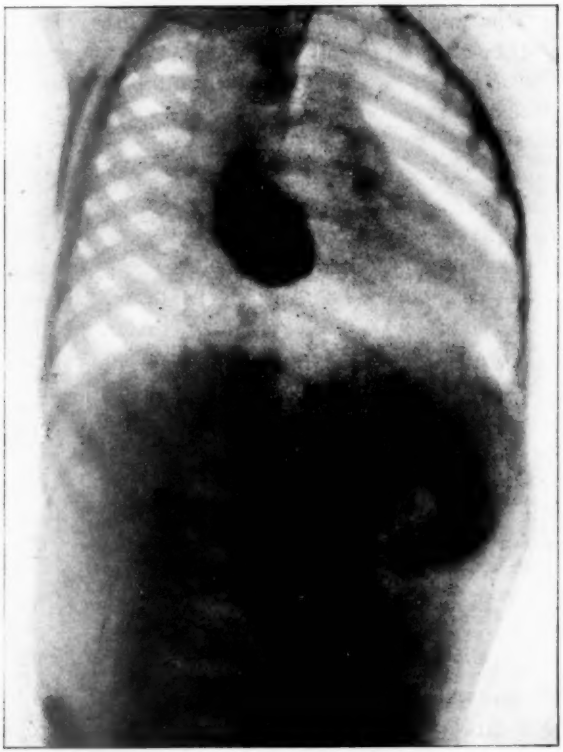


FIG. 2.

Radiogram taken three hours after a bismuth meal. Some food still in the dilated œsophagus.

On December 4, six days after admission, X-ray examination was made after a bismuth meal (figs. 1 and 2). Three screenings were done, one immediately after the meal, one an hour later, and the last three hours later. A dilatation of the œsophagus was detected near its lower

end, centrally placed, and about the size of a hen's egg. Immediately after the meal, most of it was seen to be retained in the œsophageal sac, but some had passed on into the stomach. The second and third screenings showed that part of the meal was still retained. There was no obstruction in the alimentary tract below the cardia.

As the result of this examination an attempt was made to pass a stomach tube twice a day with a view to introducing food and dilating the cardia. This appeared to be successful, but there was some doubt whether the tube curled up in the sac or traversed the cardiac opening; the vomiting was now less, but only 6 oz. could be held, the rest flowing back. To elucidate this point an opaque tube was passed under X-rays, and it was found that the tube, $\frac{1}{3}$ in. in diameter, would not pass through the cardia, but curled back into the sac on reaching the lower end.

Since the child was losing weight and a tube could not be passed, Mr. O. Addison was good enough to see her with me with a view to surgical treatment. The œsophagus was examined with an œsophagoscope, which passed freely into the stomach under an anæsthetic, and revealed no organic cause for the retention of food in the gullet. No operation was considered advisable. The mother was very anxious to take the child out, and as further attempts to pass a tube were uniformly unsuccessful and return of food and loss of weight continued in spite of frequent small feeds, this was complied with.

Remarks.—The case appears to be one of œsophagectasis due to want of relaxation of the cardiac sphincter, dating from birth, and to be similar to the examples of greatly dilated œsophagus recorded in adults. The absence of the cough and dyspnœa, which occur in adults, can be explained by the relatively small degree of dilatation. There is probably a close analogy between this condition and that of pyloric spasm, and it would appear that it bears the same relationship to hypertrophic stenosis of the cardia that pyloric spasm does to hypertrophic stenosis of the pylorus. An example of hypertrophic stenosis of the cardia was shown before the last meeting of this Section by Dr. Eric Pritchard (p. 33); this was associated with hypertrophic stenosis of the pylorus. In this case the disorder at the cardia is the only defect. The endoscopic examination is important as demonstrating the absence of an organic lesion.

DISCUSSION.

Dr. F. PARKES WEBER: I feel much interested in this class of case because some years ago I had the opportunity of observing a typical case of the condition in a man 80 years of age. Ultimately, I was able to make a post-mortem examination, when it could be demonstrated that there was no organic obstruction. The case has been described in the Society's *Proceedings*.[1] I was also interested in the fact that Dr. Langmead found that in his case there was curling up of the œsophageal tube before it reached the stomach, as I am convinced that that sometimes may become a source of the gravest errors. A test-breakfast may be given for the purpose of examining the gastric secretion, and perhaps one imagines that the œsophageal tube which is passed brings back the contents of the stomach; but it may be found to contain no free hydrochloric acid, the tube having coiled up proximally to the point of the non-organic obstruction, at the cardiac end of the œsophagus. I do not know whether there have been many fatal cases of this condition in infants, but certainly fatal cases of it do occur in adults. Men are, occasionally, starved to death: they may ultimately die of cardiac syncope, but the real cause of death is chronic starvation. Much symptomatic knowledge is to be obtained from the literature of the subject, but what the real nature of the condition is has not yet been proved. I was astonished to hear Dr. Langmead say he was not aware of cases having occurred in infancy, for that is one of the periods of life when this so-called "cardiospasm," with dilatation of the lower part of the œsophagus, is known to occur. However, W. G. Morgan, in the *Medical Record* for 1911 (ii, p. 172), went into the subject of "cardiospasm" in infants, and he could only discover the records of five cases, besides his own case, in which the diagnosis of cardiospasm with dilatation of the œsophagus had been made in infants under 2 years old. The first of those was by Méry, in 1906, in a child of 10 months. Adams reported the second case, in 1908, the subject being a baby of 6 months. An autopsy was made on that case, and a broken-down tuberculous gland was found between two local dilatations of the œsophagus. In 1909 La Fetra described a case which was one of spasmodic stricture of the œsophagus: the child was 16 months old, and the condition was confirmed by radiography. Carl Beck (1911) described two other cases in infants; and after that came Morgan's own case in an infant 9 months of age (regarded at first as a case of spasm of the pylorus). Morgan failed to see reports of some other cases. In 1908 F. Göppert described a case in the *Therap. Monatshefte*. Larozière (1914) described a case in a child aged 16 months. Quite recently I saw a probable example of this condition in a boy aged 5 years. Guisez (1914) described a case in a girl aged 7 years. Looser (1909) described one in a young woman of 21 years. I do not think

[1] F. Parkes Weber, "Spasmodic Stricture of the Œsophagus (Cardiospasm), with Fusiform Dilatation above it," *Proc. Roy. Soc. Med.*, 1914, vii, (Clin. Sect.), p. 147.

there is anything to be gained by calling the condition "achalasia," especially as we still do not know the real nature of the abnormality. In recent years there have been meetings of this Society at which this condition has been specially discussed, and a number of cases have been recorded in the *Proceedings*. A complete meeting of the Medical Section[1] was devoted to the subject; and the Section of Laryngology[2] have published many pages on the same matter and have referred to various post-mortem specimens from English cases.

Dr. H. C. CAMERON: We should have great difficulty in making a diagnosis of such a condition without such excellent skiagrams as Dr. Langmead has shown us. I can see that even if this condition is relatively common in children we shall have to examine very many children with X-rays before we find it in a few instances. The interest of the case, to me, is the proof of the existence of the condition at this early age by means of X-rays. I wonder if Dr. Langmead tried passing the mercury tube. I have had one or two of these case in adults, none in children. In the adult, in whom the signs are very characteristic, I have recognized it several times. My colleague, Mr. Mollison, has used a rubber tube filled with mercury which, when oiled, passes by its own weight, and there is no need for any difficult manipulation. That the tube is in the stomach can then be demonstrated by X-rays. I do not think anyone ought to be allowed to die of achalasia or cardiospasm because, as far as I know, the passage of the mercury-laden tube is always successful in overcoming it.

Dr. JEWESBURY: I would like to know if Dr. Langmead can give us any information as to the frequency of the vomiting in this case. Had it a reference to meals, and did it occur after every meal the child had? Or were there times when it could take food without difficulty?

Dr. G. A. SUTHERLAND: Before the advent of X-rays certainly our means of diagnosing this condition were very few. I can recall a certain number of cases in which there was difficulty in passing an œsophageal tube, and we concluded there was probably some stricture about the cardiac end of the stomach. We did not get any further. The children were very young. Whether they were cases of the kind now described I am unable to say, for we had not then the exact diagnostic means which Dr. Langmead has made use of. The question of the pathology is an extremely interesting one. Dr. Langmead does not take us very far when he says this condition "bears the same relationship to hypertrophic stenosis of the cardia that pyloric spasm does to hypertrophic stenosis of the pylorus," because, after all, what is hypertrophic stenosis of the

[1] H. Batty Shaw and A. W. Woo, "Six Cases of Œsophagectasia," *Proc. Roy. Soc. Med.*, 1916-17, x (Sect. Med.), pp. 1-22.

[2] "Discussion on Dilatation of the Œsophagus without Anatomical Stenosis," *Proc. Roy. Soc. Med.*, 1918-19, xii (Sect. Laryng.), pp. 33-100.

pylorus? Is it due to overgrowth of tissue, or to a continuous and prolonged spasm? In Dr. Langmead's case, I do not think the evidence is at all conclusive that there is, necessarily, spasm present. I do not see why this condition should not be produced by a paralytic state. I suppose the history Dr. Langmead gives can leave no doubt that the lesion in his case was congenital: it is interesting to note that it was noticed on the first day of life, and vomiting had occurred within the first week.

The PRESIDENT: As a surgeon, I feel particularly interested in what has been said about the passage of the tube in this case. Some little time ago we did some experiments to find out whether a long rectal tube really went into the bowel to the distance it was supposed to go, and the result was to convince us that it practically never did. The long rectal tube, such as that for the Plombières douche used at Harrogate, never really gets far up the rectum, and it is the same with other tubes, unless some special means are used to get them to the spot desired. I have no doubt many people have passed tubes into the stomach, as they thought, when in reality they simply curled up in the œsophagus or a pouch.

Dr. LANGMEAD (in reply): I am aware that at meetings of other Sections of this Society specimens of great dilatation of the œsophagus have been shown, in connexion with which this condition of the cardia has been discussed. My reason for bringing this case before the Section is the extremely early age at which it has been possible to make an absolutely definite diagnosis. I am really indebted to Dr. Parkes Weber for having shown, by his remarks, how very uncommonly such an early case has been proved. I did not say that the condition had not been observed in early infancy. In answer to Dr. Cameron, the question of the passage of a mercury-laden tube was considered, but this patient had a rather adventurous career after arriving at the hospital. She was rather bad, had become very thin, and by the time we had established the diagnosis, by giving a bismuth meal, and by an œsophagoscopy, the state of the child was such that I did not feel justified in trying to overcome the mother's wish to take her home. The frequency of the vomiting varied a good deal, as did its relationship to meals. I agree with Dr. Sutherland that one has not furthered knowledge of the ætiology of the condition by comparing it with another condition which is not understood: still, I think the comparison is a fairly true one, and that an explanation in one will give a very good clue to that in the other. I should like to acknowledge the great help I have had from Dr. Spence, house physician at Great Ormond Street, in working out the particulars of the case.

Further Note on a previously shown Case of Cirrhosis of Liver in a Child.

By Reginald C. Jewesbury, M.D.

At the last meeting of the Section I showed a child, just over 4 years of age, who was suffering from cirrhosis of the liver (p. 40). Both liver and spleen were enlarged and hard, and I obtained the definite history that the child had been dosed, commencing soon after it reached 1 year of age, with one teaspoonful of gin at frequent intervals. Some discussion took place here as to whether it was cirrhosis or not, and, if so, whether alcohol was the cause. A few weeks after I showed the case the child developed an extensive lobar pneumonia, from which it died, and I now show its liver and spleen, together with sections of these organs. I also show an adult cirrhotic liver in the sections, to compare with those of the child. I do not think one would like readily to say that the child's liver is a case of multilobular cirrhosis from the naked-eye appearance: the surface does not present the "hobnailed" appearance which is evident in the adult, the surface is smooth over large areas, and large irregular whitish raised areas can also be seen. The liver, to the naked eye, in fact, presents rather a secondary malignant appearance. However, the microscopical sections show a very typical multilobular cirrhosis, almost identical with the adult form. The section of the spleen, also, shows a large amount of fibrous tissue. In this child there was also a certain amount of fibrosis of the kidneys. No congenital heart defect was found.

Sclerodactylia with Raynaud's Phenomena and Necrosis of Nasal Bones.

By E. A. Cockayne, M.D.

D. P., aged 1 year and 2 months. The boy appeared to be normal up to the age of 3 weeks. At this age a bluish area appeared on the left hand, which was swollen. According to the mother, the centre of this came out as a "core." Shortly after this the child had an attack

of diarrhœa and sickness. When 10 weeks old he had a greenish-yellow offensive discharge from the nose, which has continued on and off ever since.

The child was seen first at the age of 5 months. The bridge of the nose was very depressed, and there was an offensive discharge. The fingers were blue and cold, with small septic areas on them, and the nails were deformed, and continued in this condition all through the summer. At present the child is fairly well nourished, but is very susceptible to cold. There is a swollen red area just in front of the right ear, and a blue cold area with a necrosed centre in the middle of the cheek. The hands are blue and cold, and the child gives evidence of a good deal of pain in them. The fingers are thin, the skin blue and glossy; the nails are deformed. Over the finger tips and nails there is much dry thickened epithelium. There is limitation of passive and active movements at the interphalangeal joints. In short, sclerodactylia is present. The legs show a blue mottling, and there are small scars on both lower legs. The toes are normal, but are cold and blue. While under observation the finger tips have been almost black on two occasions, and on the dorsum of the left hand there has been a blue area with a dark centre of necrosed tissue. The Wassermann reaction in both mother and child proved negative on two occasions, in April and in July, 1919.

One interesting point about this case is the early age of the commencement. It started at 3 weeks, apparently, on the left hand. The child is beginning to get sclerodactylia, just as do some cases of Raynaud's disease in adults, and that has developed very quickly. The child has worn woollen gloves during almost the whole of its life, and yet the condition in the hands is extremely marked. The toes seem to have escaped, a fact difficult to explain. The mental condition is normal: the peculiar facies I ascribe to collapse of the nasal bones.

DISCUSSION.

Dr. F. PARKES WEBER: I fail to see why Dr. Cockayne does not call this a case of sclerodactylia. I have repeatedly pointed out, with regard to cases of sclerodactylia, that the first symptoms of the condition are Raynaud's phenomena. I think that these cases of sclerodactylia should be separated from those of Raynaud's phenomena, in which there is no sclerodactylia. By "sclerodactylia" I mean the generalized form of sclerodermia; though not necessarily symmetrical at the commencement, cases tend to become symmetrical after a time. I group this case with the

cases Dr. Cockayne has so admirably described elsewhere, as sclerodactylia occurring at very early ages. In one case of his own the condition was congenital.[1]

Dr. G. A. SUTHERLAND: What is the internal condition of the nose? Dr. Cockayne does not say anything about that. There is one possibility which occurred to him, but from which, apparently, he has been warned off because the Wassermann test was negative. I think it is a mistake, if you incline to the view that it is specific disease, to accept a negative Wassermann test as conclusive. I have seen some cases of this nature, with those so-called Raynaud's phenomena — because there is no such entity as Raynaud's disease—which were undoubtedly specific, and improved very much under antisyphilitic treatment. If I have evidence which seems to point strongly to a case being syphilitic, on clinical grounds, I do not care what the pathologist reports as the result of the Wassermann test. If Dr. Cockayne has not already given that child mercury, I recommend him to do so.

Dr. COCKAYNE (in reply): I did not call the case sclerodactylia because of the lesions on the cheeks and legs, though I admit sclerodactylia is the most important feature of the case. I will amend the title. In answer to Dr. Sutherland, the child has had mercurial treatment for nearly the whole of the time, in spite of the negative report of the Wassermann test: but there has been no apparent improvement. It is remarkable that on the two occasions the mother and child were tested the Wassermann reaction was negative—namely, in April and July. I think it is improbable that the condition is syphilitic. I should have been glad if any line of treatment could have been suggested.

Dr. PARKES WEBER (in further comment): I think the most probable causation of sclerodactylia is some endocrine disturbance, though exactly what kind is doubtful. There is a field for experimental therapy here in the hope of doing the child good: I mean in regard to the use of thyroid or pituitary preparations.

P.S.—Since this meeting the tips of two fingers have become gangrenous, one is black and dry, and the other has become detached, with the loss of the nail.

[1] E. A. Cockayne, "Congenital and Acquired Sclerodermia in Childhood," *Brit. Journ. Child. Dis.*, Lond., 1916, xiii, pp. 225-233.

Case of Polio-encephalitis affecting the Mid-brain, showing Involvement of the Pyramidal and Extrapyramidal Systems.

By F. J. Poynton, M.D.

(Shown by Dr. J. C. Spence.)

The patient is a girl, aged 10 years, a healthy active child before the onset of this illness. The first symptoms arose on July 27, 1919, when she lost the use of her right arm. Later in the day her face became affected and appeared "to be twisted to the right." The child's mother states that there was no accompanying loss of consciousness, vomiting or signs of an acute feverish condition, and that she complained only of slight headache and loss of appetite. During the next few days she lost the use of her right leg, left leg and left arm in turn, and her speech became affected. Within a week of the onset of the symptoms she was confined to bed unable to move her limbs, which were stiff and rigid, and speaking only in almost unintelligible monosyllables. She was very quiet and depressed, though subject to frequent emotional outbursts. She lay in this condition for a month before any change appeared. This was first seen in the improvement in her speech and in the return of the power to use her left arm and was progressive up to her admission to the Hospital for Sick Children, Great Ormond Street, on October 30, 1919.

On admission to hospital she showed spastic paralysis of the right arm and leg. The arm was in a position of flexor rigidity, and there was a coarse tremor of the right hand. The tremor, absent during sleep or when the body was in a position of rest, was immediately brought into play by any emotional disturbance, attempt to speak or carry out any muscular effort. On the left side the arm could be moved, but the movements were wildly ataxic, the leg was rigid but there was slight movement at the hip and knee joints. Speech was slow, stuttering and hesitating. There was no squint nor ptosis. The pupils were equal, reacting normally to light and accommodation. There was no nystagmus, but the pupils showed a nystagmoid unsteadiness when brought to a central position. The face was expressionless, and when the child was emotionally disturbed the loss of expression became more marked on the right side; but there was no true facial nerve

paralysis. The right plantar reflex gave an extensor response, and there was an increased knee-jerk and ankle clonus on the right side. The left plantar reflex gave a more indefinite extensor response, difficult to elicit. There was no ankle clonus on the left side; the left knee-jerk was increased. No changes in sensation were detected. The fundi oculi were normal. The cerebro-spinal fluid was clear, sterile and without any increase in cells or albumin content. Wassermann's reaction was negative. No abnormal physical signs could be detected in the heart or lungs. The urine was normal.

During the past two months the clinical picture has changed a little. While the right hemiplegia has persisted, unaltered except for a slight return of movement in the right leg and right arm, there has been distinct improvement in the tremor of the right hand, the ataxia of the left arm, the speech, and the control of the emotions. The ataxia, which on admission was very wild, has now almost disappeared. She is no longer subject to the sudden and inexplicable emotional changes.

A careful look-out has been kept for any signs of ocular paralysis. In December there was slight inequality of the pupils for about two weeks. During the past three weeks there has been a fine lateral nystagmus but there have been no signs of third, fourth or sixth nerve involvement.

We shall be glad if anyone can suggest what tracts connecting the cerebellum and mid-brain are affected here. A difficult point in the case is, that if the lesion is in the mid-brain, with involvement of the red nucleus, or the connexion between the cerebellum and red nucleus, one would expect the third nerve to be involved. One can only assume there had been a third nerve palsy but it cleared up.

DISCUSSION.

Dr. H. C. CAMERON: It is not easy to appreciate all the circumstances from a recital of the history, but I make the alternative suggestion of encephalitis lethargica. I was struck by the statement that the cerebro-spinal fluid was normal, which is not what one would expect in poliomyelitis of such great severity. The onset, too, seems to have been gradual. I have now under my care a boy whose condition we regard as the result of encephalitis lethargica, and which in many respects reminds me of this case. If the possibility I have mentioned was taken into consideration, I have been wondering why it was negatived. The characteristic emotional state of this child, the facies, the spasticity involving all the limbs suggests

to me a diagnosis of this nature rather than that of acute polio-encephalitis. In view of the different prognosis the distinction seems important.

Dr. F. PARKES WEBER: These movements would, perhaps, fit in with some of those in various cases described by Dr. Farquhar Buzzard at a meeting of the Section of Neurology, showing sequelæ of supposed lethargic encephalitis.[1]

Mr. MITCHELL SMITH (Stoke-on-Trent): The points of interest which appeal to me in this case are the incidence of events which has led to the contractures and the possibilities of treatment which is a matter of vital moment to the child. We are all familiar with the deformities presented sadly too often by these hemiplegic cases later on, all of which might in a great measure, if not indeed wholly, be prevented. The incidence of events seems quite clear. The cerebral lesion which attacked the child several months ago threw certain groups of muscles into spasm. These overpulled the opposing groups and having gained the mastery have kept them overstretched with the resultant palsies. As the child lies on her couch this afternoon, she has a dropped foot, commencing flexional deformity of the hip, lordosis, a dropped wrist with adducted thumb and stiffness of the metacarpo-phalangeal joints. These deformities are not far advanced but they are present and will now have to be corrected. They could have been prevented by efficient splinting in the earlier stages. The President tells me that the child has been kept in splints but that they have been taken off two hours daily for massage and this afternoon whilst the child was brought here. I submit that that does not constitute efficient splinting. Splinting to be efficient in such cases must ensure that the palsied muscles are relaxed and kept in relaxation continuously until they are able to hold their own against their antagonizing groups. From the point of view of treatment, I suggest that the central lesion is now of secondary importance and that the indications are to concentrate on repairing the damage which has been done to the muscles. The deformities ought to be corrected, the hand and foot by correction in plaster, gradual correction if there is much resistance because there is always danger of throwing these spastic children into convulsions if too much strain is placed on a resistant muscle. Special care must be given to securing the thumb in proper position. Massage, remedial exercises and electro-therapeutics are all useful adjuncts to treatment provided that the palsied muscles are kept in relaxation whilst the treatment is carried out. The child should be encouraged to walk as soon as possible in a caliper splint, if necessary, and also encouraged to use her hand as soon as the finger joints are mobile. Stretching of the tensor fascia femoris and adductors should be attempted. Tenotomy of these will probably be required later, the result of which will depend largely on the after-care. The treatment I have outlined, is of course entirely based on the teaching of Sir Robert Jones. It

[1] E. F. Buzzard, "Encephalitis Lethargica," *Proc. Roy. Soc. Med.* (Sect. Neurol.), 1919, xii, pp. 56-64.

is a long and tedious business requiring infinite skill and patience on the part of the nurses, who should be specially trained for the work, but in the hands of expert workers the results are well worth the trouble.

Dr. J. C. SPENCE (in reply): It was two and a half months after the onset of the disease that an examination of the cerebro-spinal fluid was made; the significance of the findings after such a lapse of time is not great. I feel that much has been lost that might have helped to an accurate diagnosis of the cause of the disease, through not seeing the case in its earlier stage. It is difficult to decide whether the clinical picture is the result of an encephalitis lethargica or of a polio-encephalo-myelitis from infection from the organism of acute poliomyelitis. In this case there is no history of stupor or lethargy, and the residual paralyses are very gross and evident. Both of these facts induce me to exclude encephalitis lethargica as a possible cause. The gradual onset, followed by a period of progression of the symptoms for a few weeks and then gradual but definite improvement during the next few months is not indicative of a vascular lesion on the one hand or a brain tumour on the other. I think that the condition results from an infection by the organism of acute poliomyelitis of the mid-brain lesion with perhaps some involvement of the basal ganglion. A lesion of the optic thalami would account for the altered emotional states. I must thank Mr. Mitchell Smith for his suggestions as to the treatment of the deformity. At present the deformities are being treated by postural splinting, which is removed each day for massage and passive movement.

Section for the Study of Disease in Children.

President—Mr. J. P. Lockhart-Mummery, F.R.C.S.

DISCUSSION ON THE IMPORTANCE OF ACCESSORY FOOD FACTORS (VITAMINES) IN THE FEEDING OF INFANTS.[1]

Dr. Edward Mellanby.

It is obviously a matter of great difficulty for a laboratory worker to open a discussion concerned with the feeding of children. The task is not rendered easier by my consciousness of the exceptional difficulty in arguing from one species of animal to another when dealing with questions of diet and metabolism. Every worker on the subject of vitamines has this difficulty always before him. We must also recognize that much of the accessory food factor story has not been extended to children or tried beyond the laboratory walls. It is just for these reasons, however, that a discussion of the type we are having to-day should be of great value; because the laboratory worker and the clinician meet face to face. Each can describe his observations. The physiologist may throw new light on the points under discussion, and, still more important, the clinician may assist the investigator in evaluating his experimental results and so prevent the exaggeration of one fact at the expense of another.

We are fortunate in having the clinical side of the subject represented by Dr. Barton, Dr. Hutchison, and Dr. Pritchard. I trust I am voicing the opinions of all present when I express the wish that, not only will these gentlemen place before us the chief conditions affecting the so-called deficiency diseases, as met with in practice, but that they

[1] At a meeting of the Section, held February 27, 1920.

will also emphasize the points where it is clear the laboratory worker is going astray. Candid criticism can only do good when it is honest and based on accurate observation.

The laboratory is represented in the discussion by Dr. C. J. Martin and Professor Noël Paton. The experience of Dr. Martin, as Director of the Lister Institute, has been great as regards the accessory food factors. He has been in close touch with the work of Funk and Cooper, and in more recent years, with that of Miss Chick and Professor Harden and their colleagues. He will no doubt add his valuable experiences and conclusions to the discussion. Professor Noël Paton has not, I believe, made up his mind as to the reality of vitamines in general and my own work in particular. He will, no doubt, warn us against adopting an uncritical belief in the researches centring round deficiency disease. The statistical investigation undertaken in Glasgow by Miss Ferguson, under his auspices, has led to the conclusion that the dietetic aspect of the rickets problem is hardly worth further consideration—at least the fat content of the diet is of no importance. The Glasgow work indicates that if all children were brought up in large airy rooms instead of their slum dwellings, then rickets would disappear.

I propose to say but very few words in general introduction because most of you are no doubt familiar with the subject to some extent. In any case, it is now possible to read a general survey of the work in the book recently published by the Medical Research Committee. No doubt, the opener of a discussion ought to skim lightly over the whole subject, expound and philosophize thereon. I crave your forgiveness if, instead of doing the proper thing, I spend my time in dealing with one or two aspects of the problem which have interested me personally and leave to the other gentlemen taking part in the discussion the consideration of other portions which I shall only touch upon.

To-day we have to consider the part played in the nutrition of infants by the three known accessory food factors: (1) Antiscorbutic; (2) fat-soluble A; (3) water-soluble B or antineuritic.

From the point of view of the children of this country we can probably eliminate the water-soluble B factor. Although infantile beri-beri exists in rice eating countries and probably in other places where one-sided and limited diets are in use, the extensive distribution of this factor in food, together with its well recognized resistance to heat, would appear to make disease due to its deficiency small in western countries.

Of the antiscorbutic factor I shall say but little, and this without

authority or first hand knowledge of the subject. Dr. C. J. Martin will probably deal with this element of the normal diet. I suggest, however, that there are certain points in connexion with this factor which urgently require consideration. It would be, for instance, interesting if we could have authoritative statements on the following points:—

(1) Is the scurvy problem among children of this country one involving real danger? We know that the classical scurvy symptoms are rare, but it is not equally certain that milder symptoms indicating a deficiency short of the production of scurvy are not common.

(2) Is it essential that children feeding on dry milk compounds should take orange or swede juice?

The second problem involves an answer to two questions: (1) How much of the antiscorbutic factor is destroyed in the drying of milk? (2) How susceptible is the child as compared with experimental animals? Speaking without any particular knowledge on this point, I should think it probable that, although an infant feeding entirely on whole dried milk may not require extra antiscorbutic, yet when cereals are added to the diet, the antiscorbutic factor will be essential.

In opening this discussion, I propose with your permission to occupy the time at my disposal in dealing with what is possibly the fat-soluble A accessory factor. This I shall do for the following reasons: (1) It is the one with which I am most acquainted; (2) it is the factor which is probably most intimately related to disease in infants; (3) the discussion will largely centre about its action.

You may remember that the identification of the fat-soluble factor depended on work by McCollum in which the growth of rats was studied. The antirachitic factor on the other hand, has been so-called because of the effect of certain substances in preventing the development of rickets in dogs. Whether they are identical is unsettled, but one point of difference is so striking that it is essential to bear it in mind.

This is, that the fat-soluble factor is, according to the rat-feeding experiments, absent from all vegetable fats. In the rickets experiments, however, it seems definite that the amount of antirachitic factor is fairly high in some vegetable fats, such, for instance, as peanut, cotton-seed and coco-nut oils, and only to a small extent in palm kernel, linseed, and babassu oils.

A second point worthy of emphasis is that fat soluble A does not appear to be as necessary for growth in the case of puppies as it is in rats. It is true that all the diets of the puppies must have contained a trace of fat-soluble A, but the growth was often just as great when

the diet was very deficient in this factor as when containing it in abundance. This fact, previously described by me, has been extended to children recently by Hess and Unger. On a diet of dried separated milk, sugar, cotton-seed oil and cereal, sustained over many months, they found the growth of children practically normal. The question arises as to whether, in speaking of the fat-soluble factor in relation to child nutrition, we should still continue to call it a "growth factor." For the growth of puppies and children its influence is probably smaller than that of the other elements of the diet.

It seems most likely that both of the above described differences, viz., (1) the difference in distribution of the fat-soluble factor and the antirachitic accessory factor in vegetable fats; (2) the importance of the fat-soluble factor for the growth of rats and its probable unimportance for growth *per se* in puppies and children, can be explained by variation in the general and intermediate metabolism of these animals. These points will no doubt ultimately be solved by further experimental work. In the meantime, I would beg those who are extending the results of experimental research on rickets to children not to mix up the two types of work, namely, the growth experiments on rats and the rickets experiments on puppies. In giving cotton-seed oil to children, as recently carried out by Hess and Unger, they had in their minds undoubtedly the generally accepted fact that this oil contained no fat-soluble A. On the other hand, so far as my experiments have shown, cotton-seed oil is to be classed as one of the better vegetable oils in preventing rickets and, taken in conjunction with the other conditions of diet as described by Hess and Unger, it does not appear to me surprising that rachitic symptoms did not develop. I shall touch again on their experiments later.

From my earlier work, a short account of which has been already published, I consider that it is undoubted that something of the nature of a vitamine distributed in varying quantities in fats, plays an important part in the development of rickets. I do not wish this afternoon to deal further with this particular point. In view, however, of the erroneous and exaggerated views that are now widespread as to the action of the antirachitic factor in diet, it is essential that an effort should be made to bring such substances away from their atmosphere of mysticism, and place them side by side with other elements of the diet. If we can in some way link up their action with the known dietetic substances, so that we can discuss them in terms that are familiar, good progress will have been made along these lines. I propose, therefore, in

the remaining time at my disposal, to describe some of the more recent results of my experimental work carried out with this end in view.

The Relation of the Antirachitic Factor to Age.

It is a well-recognized clinical fact that active rickets is a rare disease in children over 2 years of age. On the other hand, arguing on the basis of my experimental results obtained with puppies, it is equally certain that, after this age, the diet of a child is often of a more rickets-producing nature, that is to say, it is frequently composed more of bread and other cereals and less of milk. The only deduction that is possible from these facts is that, after a time, a child becomes more independent of its diet from the rickets point of view. I wish to emphasize that it is only the rickets point of view I am discussing; no deduction from these results can be made as regards resistance to infection or any other pathological condition produced by diet and more particularly the anti-rachitic factor of diet at various ages. The above fact—namely, that the antirachitic accessory factor is of less importance in the older child is also brought out strikingly in puppies. It is obvious from certain well-defined facts such as the following:—

(1) After the puppy has arrived at a certain age I have been unable to produce rickets by feeding it on rickets-producing diets.

(2) On definite diets puppies develop rickets, but, if not too severe and the general condition does not become too bad, recovery at the growing ends of the bones, as indicated by radiographic examination, often takes place, although the animal may remain on the same diet and under the same conditions.

To take the first of these facts, fig. 1 is a photograph of a puppy taken when $10\frac{1}{2}$ months old. It was put on to a rickets-producing diet of separated milk, white bread, meat, orange juice and linseed oil at the age of 5 months, so that the dieting had lasted over a period of five and a half months at the time of the photograph. As a matter of fact, the dieting has now continued over eleven months, and the dog is still well and healthy and shows no signs of rickets. The radiographs seen in figs. 2 and 3 were taken of its wrist joints at the beginning and after five months of the diet. It is obvious that there is no indication of rickets having developed during this period.

The earlier treatment of this particular dog I shall refer to later, when I deal with the effect of exercise and confinement on the development of rickets.

As regards the second of the above facts, I shall now show you an instance of self-cure in a puppy when the diet and conditions were kept, as far as possible, the same as when the rickets developed. Figs. 4, 5 and 6 (p. 64) represent the changes taking place in the wrist of a puppy. The diet in this case consisted of separated milk, bread, orange juice, yeast, meat and linseed oil, and you will see that on March 28, after three and a half months of this diet, rickets had developed. It will be seen, however, from fig. 5, that one month after the first radiograph the healing process had started, and renewed calcification is evident between the epiphyses and diaphyses of the distal ends of radius and tibia. In



FIG. 1.

Puppy after three months' (second to fifth month of life) lack of exercise and living on a diet containing abundant antirachitic factor.

fig. 6 the healing process had continued to a further stage. Here, then, we have evidence of a self-curative process taking place in puppies as they grow older quite comparable to the self-cure which is well-recognized as taking place in children.

The above facts can be taken as evidence that the antirachitic accessory factor is more necessary in the diet the younger the animal, that is to say, until some essential process or secretion has developed in the young animal. Until this process has evolved, and of what its nature consists I have no idea at the present time, rickets will more readily

develop. After the necessary establishment of the new process in the young—animal or child—it is a matter of some difficulty to produce rickets. Whether all new calcification processes proceed normally after this event, even in the presence of deficient diets, still remains unknown, but further experiments will soon clear up this point. I should expect that a deficient diet will continue to act in a

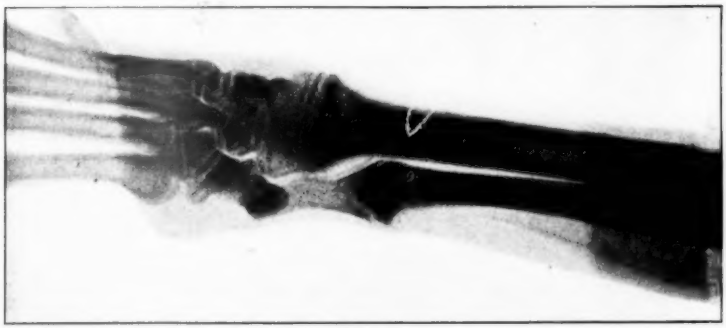


FIG. 2.

Radiograph of wrist-joint of puppy in fig. 1 after three months' confinement on a full diet and prior to five months' rickets-producing diet. The joint is normal.

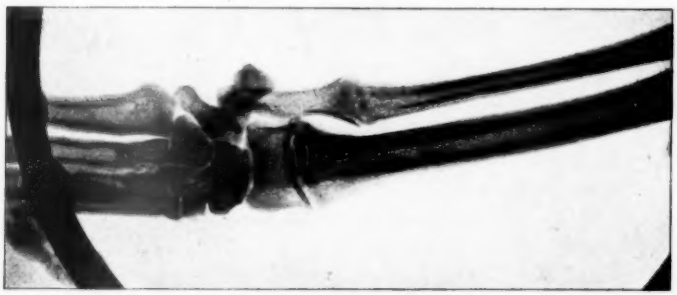


FIG. 3.

Radiograph of puppy (ten months old) in fig. 1 after five months of rickets-producing diet. The bones are normal.

manner detrimental to calcification processes, but possibly not to such a profound extent as in the younger animal and child. It is possible that the legs will continue to bend owing to poor calcification of the periosteal bone of the shafts, even after there is good evidence of a



FIG. 4.

Wrist-joint of puppy after three months' rickets-producing diet with freedom for exercise. Bones rachitic.



FIG. .

Later radiograph of puppy of fig. 4. Healing process started; diet and exercise continuing as before.



FIG. 6.

Wrist-joint of puppy of figs. 4 and 5. Self-curative process further advanced.

healing process going on at the epiphyseal ends. The recovery of calcification of the periosteal bone of the shaft appears to be a much slower process than that at the growing ends.

The Relation of the Antirachitic Accessory Factor to the Energy-bearing Portions of Diet.

In a paper already published I laid stress on the fact that, although rickets is primarily a deficiency disease of a dietetic nature, other metabolic conditions must obviously be involved because it is impossible to regard the accessory food factors as independent of the general metabolism and of the other elements of the diet. I emphasized the unity of a perfect diet, because we are becoming more and more aware that the removal or deficiency of one element in the diet involves much more than the absence of function or defective function carried out by this one element. It also means that the other parts of the diet cannot do their tasks efficiently because of the constant interplay between the chemical changes in the body and their dependence on each other. During the past eighteen months I have been endeavouring to prove that the same dependence and interrelationship holds between the vitamines and the proteins, fats and carbohydrates and, although this part of the work is still in progress, sufficient evidence has been obtained to allow the conclusion that their interaction in metabolism is of great importance so far as the development of rickets is concerned. I have explained in my earlier work how the tendency to rickets is increased by more rapid growth, using the weight of the animal as the indication of growth. I am fully aware that increase in weight is not a true indication of rate of growth because it obviously includes other factors, such as the laying on of fat. For the present, however, I shall confine myself to the relation of the development of rickets to the increase of weight. In many of the experiments the only variable part of the diet was bread, given generally to the animals *ad lib.* Therefore it is evident that, other things being equal, the greater the amount of bread eaten, the greater will be the tendency to rickets. This generality is probably true, so long as the increased bread intake results in an increased *storage* of foodstuff retained by the animal as body tissue. The same condition probably holds with oatmeal and other cereals.

In order to illustrate this point I shall show you an experiment carried out on three puppies of the same litter. The diet in each case

consisted of separated milk, white bread, orange juice, meat and palm kernel oil. In fig. 7 you see the relative rates of growth of these animals. In the curve of growth of 221, the least rapidly growing gained 1,700 grm., 223 gained 2,150 grm., and 220, the most rapidly growing, 2,830 grm. during the same period of sixty-six days. Throughout the experiment, except that the meat was raised from 5 to 10 grm. *per diem* a few days earlier in 220 than 223 and 221, the only variable was the amount of bread eaten. All other conditions as to housing were

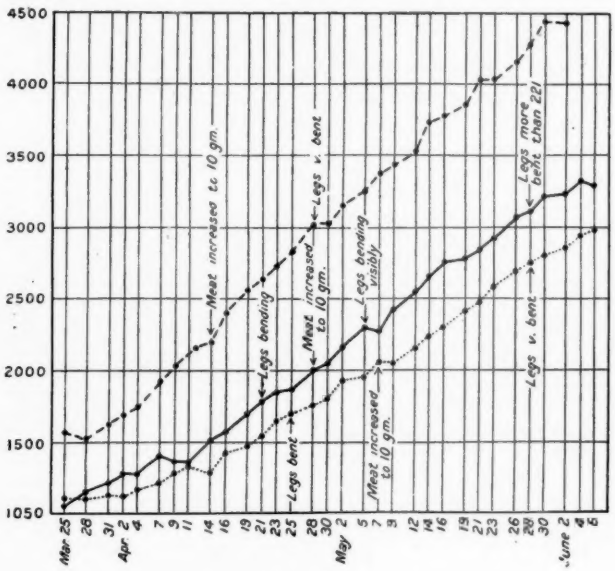


FIG. 7.

Relative rates of growth of puppies 220, 221, and 223. 220 has grown most rapidly, 221 least rapidly.

identical. If therefore any great difference regarding the development of rickets occurred, it can probably be ascribed to the variable bread intake. The radiographs of these three puppies can be seen in figs. 8, 9 and 10. It will be noticed that 221, the least rapidly growing, has very slight rickets, 220, the most rapidly growing, very severe rachitic changes, and intermediate between these two is 223, whose rate of growth is also midway between 221 and 220. Figs. 11, 12 and 13 are photographs of these three puppies, 221, 223 and 220. It will be seen



FIG. 8.

Wrist-joint of least rapidly growing puppy (221) of same family as figs. 8 and 9. Photograph of this puppy is seen in fig. 11 (p. 69). Rickets very slight.



FIG. 9.

Wrist-joint of puppy 223. Rickets fairly bad. Photograph of this puppy is seen in fig. 12 (p. 69).

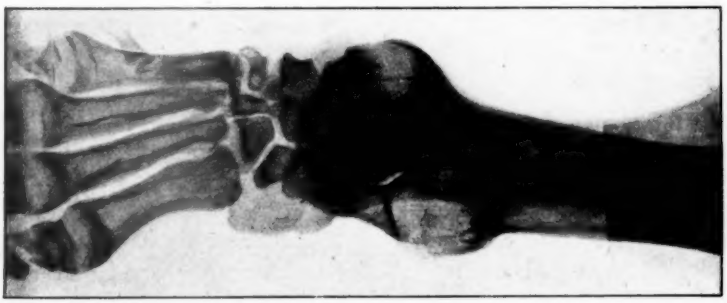


FIG. 10.

Wrist-joint of puppy 220. Rickets very bad. Photograph of this dog is seen in fig. 13 (p. 69).

that the bending and external appearances of their legs are in keeping with the radiographs of figs. 8, 9 and 10. Let me again repeat that I am only using the word growth in the sense of putting on weight. It appears then that puppies living under similar conditions show a greater tendency to develop rickets when the diet contains a larger amount of cereal, all other elements of the diet being kept constant. The question arises as to what special element of the bread is responsible for this greater tendency to rickets. This point awaits complete solution, but evidence is accumulating which indicates that the carbohydrate moiety is the offending substance. There is some evidence, on the other hand, that protein has an antagonistic action to the development of rickets. In my earlier paper, I referred to the effect of meat in making a slightly rickets-producing diet into a safe one. There is also an indication that casein has the same effect. I have been recently informed by Professor F. G. Hopkins that ordinary commercial casein, as used in these experiments, may have a large amount of the fat-soluble A accessory factor associated with it. Whether, therefore, the protein effect depends on its own action remains to be decided. If the antirachitic effect of casein is established, we will be able to comprehend one reason why milk is a better preventative of rickets than the corresponding amount of butter. I wish to make it clear that increasing the protein alone does not make a safe diet. My view is that it aids the antirachitic vitamine so that less of the latter will suffice to keep the growth normal; carbohydrate, on the contrary, especially when it results in storage of fat or makes the animal "seedy" or lethargic, appears to have a decidedly rickets-producing effect, and thereby makes additional antirachitic vitamine imperative.

It is possible that Hess and Unger's recent feeding experiments on children referred to above, in which they found that 180 grm. of a dried, separated milk powder, 30 c.c. of cotton-seed oil, 3 grm. of sucrose, 15 c.c. orange juice and 30 c.c. autolysed yeast and, later, wheat cereal *per diem*, did not produce rickets, may have, as one part of the explanation, the high protein intake of these children. I have already explained that cotton-seed oil is one of the better vegetable oils as regards its antirachitic action in puppies. 180 grm. of a dried separated milk powder must have contained about 65 grm. of protein, an amount which is abnormally high for children from 5 to 9 months old. This high protein intake, together with the antirachitic accessory factor of the cotton-seed oil, appears to me to explain satisfactorily the fact that rickets did not develop in these children. As regards their observation



FIG. 11.

Puppy 221. Very slight rickets seen in radiograph, fig. 8 (p. 67).



FIG. 12.

Puppy 223. Rachitic changes in bones seen in fig. 9 (p. 67).



FIG. 13.

Puppy 220. Very bad rickets seen in radiograph, fig. 10 (p. 67).

that the children grew fairly well in spite of the great deficiency of the fat-soluble accessory factor, this is additional evidence that it is not impossible to argue from my experimental results on puppies to children, as I have previously commented on the good growth that can be obtained in puppies in spite of great deficiency of the fat-soluble accessory factor.

I should further like to point out that the whole subject of the relationship of accessory food factors to the energy-bearing portion of the diet is one of great importance, and it is necessary that investigations on scurvy and beri-beri should also be undertaken in this connexion. It will be generally agreed that neither scurvy, nor beri-beri, nor rickets will develop as the result of starvation only. Therefore we must consider whether there is not a positive side also to these dietetic diseases. We know that in the case of scurvy and beri-beri administration of the suitable vitamine will effect a cure, but is it not also probable that the disease is brought on by some other element of the diet, since it does not develop as the result of starvation? There is some direct evidence that this is the case in avian polyneuritis, and the offending element in the diet is probably carbohydrate. Results tending to prove this have been obtained by Abderhalden, Braddon and Cooper and Funk. So, also, in the case of rickets in children, it may ultimately be established that it is excess of carbohydrate in an unbalanced diet which is largely responsible for the development of the disease. The fact that the laboratory results obtained on deficiency disease have not been entirely confirmed in practice may be the result of the omission to link up accessory food factors with carbohydrates and the other energy-bearing substances in the diet.

The Effect of Exercise on the Development of Rickets.

I have previously pointed out that, although I considered Findlay's results showing the effect of exercise on rickets to be important, yet I do not think exercise the prime factor in the ætiology of rickets but only subsidiary to diet. It is, however, obvious that the ultimate explanation of rickets must also embrace it. Whatever inhibitory effect the constant running about has in the case of puppies, we must, when considering the disease in children, discount a large part of this effect, for rickets develops in many children when only 6 months to 1 year old, and it is difficult to see how running about can play a large part at this age. The exercise obtained by children at this period

of their life consists of small movements which make up their general liveliness, and I think it will be agreed that the activity of a child depends more than anything else on the adequacy of its diet. Surely the size and number of rooms in the house where the child may carry on its movements is of subsidiary importance to its diet. If exercise and muscular contractions constitute the explanation of the ætiology of rickets, then there is clearly a simple method of eliminating the disease from Vienna, and the apostles of this gospel ought now to be preaching the glad tidings and observing its effect there. It seems to me absurd to think that the widespread and intense nature of rickets in Vienna and elsewhere in recent times has arisen primarily because of any lack of exercise or because of the more defective hygienic conditions that may have developed within the last few years.

My experiments on exercise have been made along various lines and the results obtained may be briefly stated thus:—

(1) Confinement on an adequate diet will not produce rickets.

(2) Freedom in the daytime will not prevent rickets when the diet is inadequate.

(3) Confinement will not prevent the cure of rickets, so far as the growing ends of the bones are concerned, when the diet is good.

On the other hand, complete freedom and the possibility of constant running about will oppose to some extent the rickets-producing effect of a deficient diet and may carry it over to the safe side in small slowly-growing dogs.

Fig. 1 (p. 62), which is a photograph of a dog I have previously referred to, was confined from the second to the fifth month of its life. Its diet consisted during this period of 250 c.c. whole milk, 20 grm. meat, 5 to 10 c.c. cod-liver oil, 5 c.c. orange juice and white bread. In spite of the absence of the opportunity for exercise it was a beautiful dog, showing no signs of rickets, as can be seen from the radiograph, fig. 2 (p. 63), taken after the period of confinement.

The brother of this puppy was allowed complete freedom during the daytime (with a special type of muzzle on) and developed slight rickets, which can be seen in fig. 4 (p. 64). It was eating a rickets-producing diet during this period consisting of 175 to 250 c.c. separated milk, white bread *ad lib.*, 5 to 10 grm. meat, 10 c.c. linseed oil and 5 c.c. orange juice. This animal was an instance of self cure and, while living under the same conditions, calcification of the growing ends of the bones was resumed. This point is seen in the radiographs (figs. 5 and 6, p. 64), and has been previously commented on. The rate of growth

of 199 was not very good: it increased 2,730 grm. in nineteen weeks while eating the diet.

Here, then, we have instances of two puppies (terriers) and members of the same family, the one remaining normal in spite of lack of exercise and the other developing rickets with full opportunity for exercise. The dominant factor in each case was the diet. The first dog had an abundance of antirachitic factor in its diet, which included whole milk and cod-liver oil. The second on separated milk and linseed oil was getting little or no antirachitic factor. I wish to repeat, however, that muscular contraction has some inhibitory action in the development of rickets and must be considered in the general scheme.

The points I have discussed to-day, more particularly the relation of the antirachitic accessory factor to the energy-bearing portions of the diet and to exercise, indicate plainly that rickets must be considered as a problem of general metabolism. As a working hypothesis it seems possible to formulate a general scheme in which each of the various elements already discussed can find a place. Any condition which induces a laying on of tissue seems to necessitate a greater intake of antirachitic accessory factor to prevent rickets. On the other hand, conditions which stimulate the metabolism and increase the heat loss relatively to the energy of the stored food work together with the antirachitic accessory factor and make a less amount necessary for normal growth. For instance, excessive carbohydrates in the diet often brings about a condition of laying on of fat associated with lethargy. The diet under these conditions must have much antirachitic accessory factor, otherwise rickets develops. Proteins and exercise are stimulants to the metabolism and, when the diet has a relatively good protein content and the animal is active, less antirachitic accessory factor is necessary.

It is upon this hypothesis that my present investigations on rickets are being continued, and, while it is freely admitted that it is not based on complete experimental evidence, a general conception is always useful to an investigator and can do no harm so long as it is regarded by others in its true light. The generalization may not explain all conditions under which rickets develops, but we are in the satisfactory position of knowing that its proof or disproof can easily be tested by further experiment.

The Importance of the Antirachitic Accessory Factor in the Feeding of Children.

Up to the present I have dealt entirely with the effect of the antirachitic vitamine in the case of puppies, and I think you would probably find it interesting if I attempted to supply some evidence which supported the animal results and showed that, in the case of children also, this substance is of great importance. I shall deal with two investigations[1] carried out at a time when the presence of the antirachitic accessory factor in food was undreamt of.

In examining school children at Leeds, Dr. William Hall was much interested in the great difference in general nutrition, and more especially in the teeth of Jewish and Gentile children, the financial position and housing of whose families were comparable. The results of his investigations were described at a Health Congress in Leeds in 1902, and his general observations and conclusions are so concordant with my experimental results that I should like to record them. The following table represents a few of these results :—

	Rickets	Bad or backward teeth
Good district Gentile school ...	8 per cent. ...	38 per cent.
,, ,, Jewish ,, ...	5 ,, ...	11 ,,
Poor ,, Gentile ,, ...	50 ,, ...	60 ,,
,, ,, Jewish ,, ...	7 ,, ...	25 ,,
Country school ... ... (Ripon Cathedral School)	11 ,, ...	33 ,,

The great difference between these two classes of people, more especially in the poor schools, is very striking. It will be further observed that the condition of the poorest Jewish children is better than that of the country children.

Hall then proceeded to investigate the cause of the above described differences and finally decided that only the dieting could be held responsible. From his analysis of the diets, which he found very different in the two classes of the community examined, I take the following points :—

(1) The Jewish families used large quantities of oil in cooking—even in making bread. Fish were generally fried in oil. Potatoes were not boiled in water; if boiled, it was usually in milk; otherwise cooked in oil. In making broth, butter and oil were added to it.

[1] These instances were kindly brought to my notice by Dr. Scurfield, Medical Officer of Health for Sheffield.

(2) The normal beverage was cocoa made with milk. This was usually drunk three times a day, except on the days when meat was eaten.

(3) An analysis of the eggs eaten by the Jewish families showed the large number consumed.

(4) Fruit and vegetables were used abundantly.

(5) The Jewish mothers combined to buy large quantities of the cheaper fish in the market. Herrings were commonly eaten.

Dr. Hall's conclusions as to the inferior physique of the Gentile families were: (1) That it did not depend on heredity or on city life; (2) that it was purely a dietetic problem.

My researches on animals are in complete agreement with the outcome of these investigations. The diet is certainly the key to the physical defects so common among urban and city inhabitants. It is also remarkable how closely the diets of the Jewish community, as observed by Hall, follow the animal experimental results. Nearly all the substances mentioned by Hall contain an abundance of antirachitic accessory factor.

The next subject connected with diet and child nutrition to which I wish to refer concerns the inhabitants of the Island of Lewis in the Hebrides. The facts I shall mention are taken from the Carnegie Report of the Physical Welfare of Mothers and Children of Scotland. Many of the inhabitants of these islands live in what are called "Black Houses." A photograph of a typical "Black House" is shown in fig. 14. They are constructed of turf and stones with a thatched roof. There is often no chimney to the house, and since the peat fires are kept constantly burning and there is no exit for smoke, except through the door, the condition of the atmosphere of the house can be well imagined. The walls are often as much as 5 ft. thick. Cattle often live under the same roof, the byre adjoining the house, and it is sometimes necessary to pass through the byre to enter or leave the building. The chicken have full run of the house and go on to the beds, tables and dressers. Altogether the hygienic conditions are dreadful. What about the children? The statements are made in the report: "The children are not taken out until they can walk;" "never taken out till they can walk, except possibly for a few minutes on a fine day in the summer time." It is quite clear, therefore, that, if bad hygiene and lack of exercise are responsible for rickets and ill-health, then the infants of Lewis ought to succumb at a great rate.

What are the facts? The main fact is that the death-rate of these

infants under 1 year is about the lowest in the British Isles, and has occasionally fallen to as low a figure as 40 per 1,000. This death-rate compares very favourably with the infantile mortality of 100 to 300 per 1,000 usually found in the towns of this country, in spite of the efforts made to improve the general hygiene. It is remarked that rickets is almost unknown in the island, and the most striking feature in the adult population is their beautiful teeth—a testimony to the absence of rickets in infancy. Parenthetically, I regret that lack of time has prevented me from showing on the lantern some of the beautiful specimens of my wife's work on teeth—work that has shown that the formation of perfect and imperfect teeth and jaws is affected by diet in a similar way to the development of the normal and rachitic condition of bones.



FIG. 14.

A typical "Black house" in Lewis (Hebrides) where the teeth of the people are excellent and there is no rickets, in spite of bad hygienic conditions.

If we now examine the diet of these people we find results in close agreement with expectations based on the above described experimental work on puppies. In the first place practically all the children are breast fed. Again an analysis of the foods eaten show that the staple articles of diet are fish, oatmeal and eggs. It is true that milk is scarce, except in the summer, but against this we have breast feeding, affording the opportunity to children to get milk at the most important period of their life. As regards fish we find that the liver (i.e., the best source of the antirachitic accessory factor) is regarded as the favourite dish. It

is described as being mixed with oatmeal and milk and cooked in cods' heads, each member of the family being provided with a cod's head. It is almost certain that the breast feeding of the children, together with the high protein and antirachitic accessory factor content of the adult diet, are responsible for the absence of rickets, the formation of beautiful teeth and the very low infant mortality found in these islands and, that this is the case, in spite of the dreadful hygienic conditions of many of the houses. For the benefit of those interested in the antiscorbutic factor, I may mention that vegetables are almost unknown in these parts. Evidence is accumulating from various sources, showing the effect of the diet of the mother on the accessory food factor content of milk, so that it is likely that the milk of the Lewis women is particularly rich in the antirachitic accessory factor. I do not wish, however, to discuss this point to-day. If we follow up the mortality and health history of the Lewis children after the age of 1 year, additional facts of importance can be observed. From 1 to 5 years the death-rate among the children is high when considered in relation to the infantile mortality under 1 year. The housing conditions are such that epidemics spread rapidly and fatally, and phthisis in 1914 caused a death-rate double that on the mainland. When the children begin to go to school, they fall off rapidly until they attain an age when they can look after themselves. This is because they generally have to go to school before their parents are out of bed, and so often do not get a real meal until midday. When the children have arrived at an age at which they can fend for themselves and satisfy their appetite by seizing food, if need be, then they develop well and ultimately become the fine stock so well recognized as coming from these islands.

I have dwelt on this particular point because it seems to afford an opportunity of realizing to some extent the relative importance of diet and housing at the different ages. It is difficult to avoid the conclusion that diet is everything to infants under 1 year, and, so long as this is good, bad hygienic conditions are of small significance. After the first year, however, when the child becomes susceptible to measles, broncho-pneumonia, and other infections, then clearly the housing and hygienic factors in addition to the diet are of great importance. If these suggestions could be definitely proved and accepted, we should have gone a long way towards the solution of the problem of race decadence. It is reasonable to accept as facts that where there is low infant mortality, there is an almost complete absence of rickets and the teeth of the people are good; also that the production of rickets depends on a

relative insufficiency in the diet of the antirachitic accessory factor, the best sources of which are fish oils, animal fats, except lard, milk, eggs and some of the vegetable fats. It is also probable that anything which stimulates metabolism, such as high protein in the diet and exercise, aids the antirachitic accessory factor; while excess of carbohydrate, especially if it leads to a laying on of fat or a production of lethargy, works in a way antagonistic to the vitamine and makes it necessary to have a greater amount in the diet to ensure normal growth of bone, formation of sound teeth and jaws, and good general health.

My investigation on rickets is still in progress and the results remain incomplete. The mode of production of the disease is obviously complex, otherwise it would have long been solved, but by considering and altering each factor in turn, the animal experimental method will certainly in time clear up all the points of difficulty.

I feel I owe you an apology for having introduced the discussion in such a one-sided way, but, if I have succeeded in increasiug your interest in a new aspect of the problem, which I hope will prove of great practical value, I shall probably have earned your forgiveness.

Professor Noël Paton, F.R.S.

It is curious and interesting that a discussion of this kind should be opened by one physiologist and followed by another. I take it as a welcome admission by the clinician that we are brethren who can and should help each other. The physiologist will always be ready to acknowledge his indebtedness to the clinician whose work and observations have suggested to him so many of his most profitable lines of work and he is, now, at least trying to repay the debt by applying some part of his time to the solution of problems of importance to the clinician—witness the investigations carried out on wound shock and gassing during the late war. In doing this I think that he must recognize that it is his duty to present for the acceptance of his clinical brother only those results which have been thoroughly tested, and that he should remember that the man in practice is just as able as he is to assess the value of the evidence presented and that he should not insult his intelligence by asking his acceptance of statements without that evidence being given. This is just what I object to in the attitude of many physiologists to the present craze for vitamines—a horrible name since we do not know that, even if they exist, they are amines, or what

they have to do with life. What we do know on the subject may very briefly be stated :—

(1) Hopkins and Willcock, and Osborne and Mendel have shown that minimal quantities of certain amino-acids are necessary for growth. They are limiting factors for growth.

(2) The same has been shown by McCollum and others for inorganic substances, chiefly calcium and phosphorus.

(3) In various animals it has been shown that the absence of some water-soluble food constituent or constituents leads to arrest of growth and death, or to the onset of scurvy or beri-beri.

(4) In rats and mice it has been shown that minimal quantities of certain fatty mixtures—e.g., of milk fat or cod-liver oil—are necessary constituents of the diet for growth. With a deficient supply of these growth is arrested in these animals. In dogs, according to Mellanby, growth is not arrested, but rickets is produced. Of their effect upon other animals we know nothing. What of kittens, piglings, chicks, and what of infants?

On such a basis have we any right to draw any far-reaching conclusions?

Why do not clinicians arrange a series of experiments in institutions to study the effects of these substances, instead of being satisfied with personal experience or remaining content to argue from the divergent results in rats and pups to the human subject? I am, of course, aware that such clinical work has been done and I shall presently deal with the observations of Hess and Unger.

I do not propose to speak further upon the general question of vitamines. My work has been done on rickets and I shall confine myself to the evidence that this is a disease due to want of vitamines. In doing so, I must refer to the recently published report of the Vitamines Sub-committee of the Medical Research Committee. The part of the report which deals with rickets is certainly a request to "open your mouth and shut your eyes"—to accept statements unsupported by evidence. But what we have to do is to estimate the value of the evidence adduced. What evidence have we at present as regards the cause of rickets?

I purposely refrain from touching upon the older work on rickets, an excellent résumé of which is given by Findlay in the introduction to Miss Ferguson's report. Von Hansemann's "domestication" may mean anything—confinement, feeding or filth, and it is needless to discuss such an abstraction.

First, we have some evidence upon the possibility of rickets being associated with abnormal conditions of the thymus. The observations of Friedleben, Von Basch, Klose and Vogt and Matti seemed to give support to this view. The experiments of Renton and Madge Robertson in this laboratory show quite clearly that rickets occurs as readily in animals with the thymus as in animals without it. They found that however well fed pups kept in a laboratory may be, there is a great tendency to the development of rickets, a tendency more marked in some breeds than in others. The recent paper by Park and McClure in the *American Journal of the Diseases of Children*, November, 1919, supports the conclusions of Renton and Robertson that rickets is not due to thymus insufficiency.

Leonard Findlay, in 1908, reviewed the existing evidence and recorded a series of experiments upon sixteen puppies which he considered proof that confinement and not diet is the determining factor in causing rickets. He found that puppies fed on bread and water simply wasted and died without developing rickets, in the same way as rats wanting the fat-soluble substance waste and die. All the puppies kept confined (eight) developed rickets; the five not confined to the same diet of porridge and milk did not do so. Mellanby criticizes the results on the supposition that Findlay gave only 175 c.c. of milk, a purely gratuitous assumption. They had an abundance of milk. He further suggests that the diet was not a healthy one because two of the pups died. It is the diet on which most of the pups in Scotland are reared. How many of Mellanby's 200 dogs died? My criticism of Findlay's experiments is that only in one case were the confined pups and the checks of the same litter and it is unjustifiable to argue from such results, obtained from different litters, since the susceptibility of different strains is different.

The report of the Vitamine Committee is avowedly based upon Mellanby's results. We can judge only of its value from the results which have been published. These appeared in the *Lancet* of March 15, 1919, pp. 407-412. He states that he cleared the way for his dietary experiments by a series of experiments to show that other factors were not operative. These experiments he does not record and we cannot estimate their value. We have thus Findlay's recorded results on the one side, and Mellanby's statement on the other, neither of which justifies any definite conclusion.

As to the influence of the dietary factors, he has recorded observations upon thirty-three pups, ten normal and thirteen which developed

rickets. Presumably the experiments selected for publication were not the least convincing of his series. When the records are examined it is found that the dogs grouped together *were not always of the same litter.* Now, to contrast animals of different litters is, as already stated, wholly unjustifiable, since the susceptibility of different strains to rickets is notoriously different. Further, in his groups the pups were not always killed at the same age, and yet the calcium content of the bones is compared, although, as everyone knows, this varies with age. The same puppy is included in different groups.

In this small series of observations no less than thirteen substances were tested and conclusions drawn as to which of these allows and which prevents rickets. In the Report of the Vitamine Committee a list of thirteen substances which do not prevent and of eleven substances which do prevent rickets is given upon page 83, with no published evidence except that of Mellanby's paper. Certainly on such a basis it is ridiculous to ask any reasonable man to accept the conclusions :—

(i) That rickets is a deficiency disease due to the absence of an accessory factor probably identical with the fat-soluble substance.

(ii) That certain specified articles of food prevent its onset while others do not prevent it.

Our observations recorded in the *British Medical Journal* of December, 1918, were made upon seventeen puppies of two litters. The main object was to get rickety pups and sound pups of the same litter for chemical examination. But a test was also afforded of the relative influence of diet and open air life. From each litter two puppies were sent to the country, kept in a large yard and fed upon oatmeal porridge and skimmed milk, so that each had 2·8 grm. of milk fat per day. Those kept in the laboratory had whole milk containing during the first period of their growth, 3 grm. of milk fat per day, and during the later period 6 grm. In addition to this, three of the puppies had butter in an amount to yield, along with the milk, 14 grm. of milk fat. The puppies kept in the country on the lower fat intake remained absolutely normal. All the puppies kept in the laboratory developed rickets, one of those receiving butter became most distinctly affected. It has been suggested that puppies kept in the country get their fat-soluble substance by eating grass. Unfortunately for this theory, the carnivorous alimentary canal does not digest grass, even when swallowed. It has further been argued that confinement may so depress digestion that the fat-soluble substance is not absorbed from the food, but there was no indication of disturbed digestion and

Mellanby has stated that confinement does not cause rickets. In the last experiment the pups were given a very large amount of oatmeal, and the energy intake was superabundant. Watson, during the present winter, has made observations upon two litters of puppies with a more limited energy supply in the food. No final conclusions can be based upon them. At the first glance they seem to support the view that milk fat has an influence in retarding the onset of rickets, but the fact that at least two of the pups on 250 c.c. of whole milk developed rickets, and the further fact that those on skim milk had an energy intake so much lower than the others, render definite conclusions impossible.

In the Vitamine Committee's report no reference is made to Bull's interesting investigations of an epidemic of rickets among foxhound puppies at Adelaide in 1912.[1] The kennel had previously been free of rickets. The epidemic, when it broke out, affected practically all the puppies. The animals had been fed on boiled meat and pollard, the only change being that before the outbreak the pollard had been substituted for wheaten meal. He found that the most potent factor in preventing and curing the condition was to remove the pups from the yard in which they had been kept. He found, for instance, that two pups, when taken away and kept upon their ordinary food, did not develop rickets. He reported that affected puppies which had a supply of milk improved most rapidly. A litter of two pups and their mother were moved to a specially prepared yard, given the same food, but remained free of rickets. His conclusion is: "It was found that almost any change in the environment or diet would at once modify the occurrence and intensity of the disease. When the animals were kept in the same yard that had been used for some years for confining the puppies, but the diet was altered, a modification was produced—namely, the disease became less severe in its manifestations and not all the animals were affected. *When removed from the kennels animals developing the disease showed improvement and sometimes complete recovery irrespective of the nature of the diet.*

"Undoubtedly the food the animals were receiving was very unsuitable and closely associated with the production of the disease, but it could not be regarded as the essential cause. *It was not until a new yard was prepared for the puppies that the occurrence of the disease was prevented, even when the old diet was continued.*

[1] *Journ. Comp. Pathol.*, xxxi, 1918.

"It would appear, therefore, that at least two conditions were associated with the occurrence of the disease—namely, environment and food. But it has been shown that the particular food used for some years at the kennels would not produce the disease when other conditions were altered. On the other hand, it was found impossible to prevent the occurrence of the disease completely when the diet only was altered, although the evidence in this regard is inconclusive; and one is inclined to believe that, had more opportunity occurred of improving the diet, the disease would probably have been completely controlled.

"In contrast with this, more or less satisfactory improvement following change of diet is the most marked success obtained by change of environment. This stands out as a positive fact, that the same diet had consistently been associated with the disease being given, the disease could not be produced in puppies, even of the same breed, when fed away from the kennels or on some other part of the property that had never been used for similar purposes before."

Some returns which I procured from the huntsmen of some of the larger hunts upon the incidence of rickets in the foxhound whelps give little support to the idea that deficiency in a fat-soluble substance is a causal factor in the onset of the condition.

TABLE.

Number of whelps	Food	Exercise	Rickets	Age of incidence	Remarks
(I) 120 to 140	Oatmeal, biscuits, meat, milk (skimmed and scalded)	Grass run, fresh ground daily	Occasional	2 to 3 months	Exercise
(II) 70 ... (Peace, 200)	No milk, ordinary kennel food	Big field	After distemper	8 to 10 months	—
(III) 40 to 60 (Peace, 170)	Oatmeal, biscuit, milk, half pint (best boiled)	Large kennel in fresh field	Yes, if kept confined or overfed	9 to 10 weeks	Get them away for liberty
(IV) 160 to 190	Food given to old hounds; do not use milk	Good large pen	. Yes	All ages	Puppies sent off at 8 to 9 weeks
(V) 40 to 60 ...	Cow's milk, new, half pint	Pen, six yards square, on grass	No, except in bad mothered pups or worms	3 to 9 weeks	—

Investigations on Children.—The series of investigations which have been given the most prominence is that of Hess and Unger upon negro children in New York. According to the authors, about 90 per cent. of these develop rickets. From observations upon thirty-two children,

they state that the administration of 54 oz. of cod-liver oil for six months prevented the onset of rickets in 93 per cent. of the cases. Their Table I has been largely used by the champions of the vitamine theory, but it would have been well had they read the paper. A reference to Table III would have shown them that ten out of the thirty-three children treated had rickets before the oil was administered and that they had merely improved during the six months under oil. As a matter of fact, in the Table III, six are noted as unchanged and two progressed—became worse. This hardly justifies the statement in Table I as regards the prophylactic action of oil. Miss Ferguson, under the direction of Leonard Findlay, has repeated these observations upon sixty-two children, of from 1½ to 12 months, with cod-liver oil to about 2 oz. per week, that is about the same amount as given by Hess and Unger, was given for about six months. The cases were examined before starting treatment and were all free from rickets. They were examined again at the end of six months by Dr. Findlay, who reports: out of twenty-seven oil and thirty check cases present at the examination there were :—

	Oil	Check
Non-rachitic ...	23 = 85 per cent.	... 10 = 33 per cent.
Rachitic	3 = 11 ,,	... 18 = 60 ,,
Doubtful	1 = 4 ,,	... 2 = 7 ,,

Note on Teething.—On an average, the controls are behind those receiving oil by about one tooth a month from seven months onward.

Such an observation lends support to the view that cod-liver oil has a favourable action, but whether this is due to a vitamine or to some other action is not indicated. The investigation is still in progress.

Miss Ferguson, in 1917, undertook an investigation of the diets and social conditions of families in which rickets occurred and in which rickets had not occurred—forty-seven families in all—of the same social class in the city of Glasgow where rickets is very prevalent. She found no marked difference in the dietary habits: both sets of families used very small quantities of milk, the average in the non-rachitic families being 6·7 oz. per person per day, and in the rachitic families 4·7 oz. Of twenty-seven rachitic families, twenty—i.e., 74 per cent., and of twenty non-rachitic families, twelve—i.e., 60 per cent. had less than 30 c.c. milk, expressed as per man per day. In quite a number of the non-rachitic families the milk consumed was as small in amount as in any of the rachitic. Mellanby criticizes these results, on the ground that (1) such family studies throw no light on the feeding of the infants,

and (2) they show that in the families with rickets the children got less of "substances delaying or preventing rickets." One cannot argue in both ways.

Here let me point out that the criticism of family diets made in the report of the Sub-committee that they give the amount of the proximate principles but not the actual foods used is untrue, so far as the Glasgow studies are concerned.

Lastly, I must refer to the most recent publication by Hess and Unger on "The Clinical Rôle of the Fat-soluble Vitamine; its Relation to Rickets."[1] It is a most cogent criticism of the findings of the Vitamine Sub-committee, supported by a valuable series of observations. They show that the bone changes in scurvy may simulate those of rickets. They find that rickets develops notwithstanding an abundance of fresh air. They record a series of experiments to show that infants getting abundance of milk fat become rickety, while others kept upon a fat-poor diet remain almost free from the disease. They conclude: "It would lead too far afield to discuss the various theories that have been advanced to account for the occurrence of rickets, and, moreover, it would not be profitable at the present time as the data are inadequate. There seem to be several causes at work, rendering the unravelling of the problem so difficult that there is a difference of opinion, not only as to the particular dietary factor that is at fault, but even as to whether rickets is to be considered a disorder of dietetic origin. It should not be lost sight of that there is a pre-natal factor involved. The fact that the negro infant, living side by side with the white in the larger cities, and obtaining milk from the same source, develops rickets so frequently and so markedly, indicates that there are important influences to be reckoned with in addition to the food."

In considering the diet a most important question is whether the recent theory as to the vitamine origin of this disorder can be maintained, and more particularly whether rickets should be attributed to a lack of the fat-soluble factor. We can obtain the clearest understanding of this aspect by comparing this disease to the well-recognized and established deficiency diseases, scurvy and beri-beri. What does the comparison show? In the first place, these two disorders are commonly accompanied by weakness and malnutrition; we do not encounter the strong, apparently healthy, babies met with in

[1] *Journ. Amer. Med. Assoc.*, January 24, 1920.

rickets. But of far greater moment is the fact that neither can be brought about by overfeeding. Rickets, as I have already emphasized, frequently develops in infants receiving too much milk rich fats, protein and salts. It seems impossible to bring this fact into consonance with a deficiency disease, whatever may be its nature, using this term in the commonly accepted sense. Our study shows that the fat-soluble vitamine is not the controlling influence; that infants develop rickets while receiving a full amount of this principle, and that they do not manifest signs, although deprived of this vitamine for many months, at the most vulnerable period of their lives. It is impossible to interpret the contrary conclusion to which Mellanby came as the result of his pioneer experiments on dogs or to accept the term "fat-soluble vitamine" as synonymous with "antirachitic factor" as Hopkins and Chick would have us do. Clinical tests carried out with care must be accorded fully as much weight as laboratory investigations. The two methods of approach should be carried out side by side, and even the most thorough study on animals must be made to harmonize before it can be accepted as holding good for man.

Finally, this work seems to show that the danger to infants of a diet deficient in fat-soluble vitamine is slight, provided it includes sufficient calories, and otherwise is complete. They can maintain their health and vigour, despite amounts of fat-soluble vitamine so small as rarely to be encountered in times of peace. In spite of the fact, therefore, that this vitamine is not widely distributed in nature, a disorder that may be termed "fat-soluble deficiency—marasmus or xerophthalmia—is hardly to be apprehended from a clinical standpoint." They also state: "There is a growing danger of attributing every unexplained growth impulse to the new, attractive but ill-defined vitamines: of their sharing with the secretions of the endocrine glands the fate of becoming the dumping ground for every unidentified factor. It should be borne in mind that there are other little understood factors and food reactions."

These results are in conformity with those of von Pirquet ("System die Ernährung") that "A fat minimum does not exist for domestic animals. Fat can entirely be replaced by carbohydrates. This may happen through a lifetime where the natural nourishment, milk, affords a rich fat content; in the weaning of calves sour 'magermilch' is used, to which fat-rich seeds or cake (linseed, or linseed cake, or coco-nut cake) may be added, but also, just as well, meal or bran. I have made experiments in this direction upon sucklings in which I replaced the

usual milk mixture with an entirely centrifuged de-fatted milk, the calorie value of which was brought up to the normal by cane sugar. In spite of the fact that the fat-less diet was suddenly given without any gradual transference, no influence was noticed on the course of the weight curves of the children and only once was any influence noticed on the stools."

The periods over which these fat-less diets were given were too short to allow of conclusions being drawn as to the influence on the onset of rickets.

I agree with Hess and Unger that we are not yet in a position to discuss the ætiology of rickets until we have done a great deal more work. Certainly it has not yet been proved to be a deficiency disease due to the lack of some fat-soluble substance. Nor do I think it has been proved to be due to the lack of exercise. But it is directly correlated with overcrowding and confinement in insanitary houses. The possibility that it is primarily an infective disease has never been disproved. It might be due to non-specific micro-organisms, possibly of fæcal origin, in the same way as goitre has been shown by McCarrison to be due to such infections. Certainly the more scrupulously clean the kennels are kept, the less seems to be the tendency for pups to develop rickets. This is indicated by Bull's observations.

What we want is a combined co-ordinated study of the disease from the clinical and from the experimental side. Rickets is so prevalent and is of such enormous importance from the economic standpoint, that it is well worthy of the most serious study. In the interests of the nation, I trust that the Medical Research Committee will be able to institute such a comprehensive investigation.

Dr. Robert Hutchison.

The causation of rickets has always been a puzzle to clinicans, but up to the present the advent of the experimental pathologist in the field has only served to darken counsel. In my opinion Dr. Mellanby has certainly made out a *prima facie* case for the view that rickets is the result of the absence of some accessory substance from the diet although it does not appear to be the ordinary fat-soluble A. On the other hand Professor Noël Paton's experiments have led to an opposite conclusion, nor, apparently do rats develop rickets when fed on a diet devoid of the growth vitamine. It should further be remembered that Herter[1]

[1] *Journ. Exper. Med.*, 1898, iii, 293.

fed young pigs on a fat-free diet for prolonged periods, and although their nutrition suffered they did not develop anything even suggesting rickets. If the results of experiment differ in rats, puppies and pigs, how can we apply them to babies?

Clinical evidence is in favour of the disease being a dietetic disease, for it can be cured by altering the diet; the other factor is in the environment remaining as before. The dietetic fault is certainly not an all-round deficiency, for rickets is, perhaps, commoner in over-fed than in under-fed children. It seems rather to be caused by an ill-balanced diet and particularly by one which contains a relative excess of carbohydrates. How this causes rickets it is impossible to say, but then Dr. Mellanby is unable to say how the absence of a "vitamine" causes it either. If an accessory substance plays any part at all it must be that it is required to counteract some other defect in the diet and not merely that it supplies in itself something that is essential and without which rickets will develop, no matter what the composition of the diet is as a whole. The same is true of scurvy. Infants rarely develop scurvy on a diet of condensed milk alone, but if a starchy food were added to the condensed milk, scurvy is very apt to appear. The antiscorbutic accessory substance must in some way actively counteract the bad qualities of food and diet, and not merely supply something lacking, for it is equally lacking in the condensed milk alone. It may be necessary to assume this kind of action on the part of the accessory substances, but it is attributing to them rather a different rôle from that with which earlier investigations have credited them.

I am of opinion that as regards rickets, at least we want more evidence before calling in the action of a hypothetical accessory substance at all. It must be remembered that vitamines are the latest dietetic "stunt," and we must be careful not to push their significance too far and not to draw sweeping practical conclusions from our at present very inadequate knowledge of them.

Professor C. J. Martin, F.R.S.

I listened with the very greatest interest to Dr. Mellanby's opening of this discussion, and it struck me as not only being excellently served up, but also as being very moderate in the conclusions drawn.

Dr. Mellanby brought before you some facts, and those facts have to be explained. The importance of them to me is their orientating value. You have, ultimately, to test any interpretation of the essential

causation of rickets, whether it be in children or in animals, by studying the disease in children and in animals as it occurs. I would not wish for a moment to depreciate the value of clinical observations, or the physician's capacity for making such observations or weighing the value of the evidence. Scientific work is scientific work, whether it be done at the bedside or in the laboratory workshop. It is the method, not the material, that matters. But, in scientific procedure, one must have some idea to guide one. You may have patients and see things going on, and you may make careful observations: but you do not know what particularly to observe and record unless you have an idea as to what is happening to test.

I can add but little to the discussion on rickets, but it struck me that Professor Noël Paton's contribution was not a serious assault upon the opener's position. From an epidemiological point of view, surely there is no doubt now that rickets is a dietetic disease, nor can one doubt it from the results obtained by altering the diet. That rickets may be a complicated problem, and that Dr. Mellanby has not finished his work, are obvious facts.

I would like to emphasize the warning which was uttered by Dr. Mellanby, and by Professor Paton too, that you must not apply quantitatively results obtained in the case of one kind of animal to other kinds of animals, or even to the same kind of animal of a different age. The danger of doing so will be seen from certain facts which I shall bring before you as regards the antiscorbutic factor.

What is wanted in this vitamine business is to put the matter upon a quantitative basis. That is more or less impossible at present for rickets—knowledge has not advanced far enough. It is most desirable, however, as regards the other accessory food factors, the action of which is more precisely known. Until such a quantitative basis is reached, not only shall we differ in matters of general principle—as to whether the growth factor is or is not present in one thing or in another, or whether it is or is not destroyed by heat—but it will be impossible to ascertain the minimal quantities required for even one kind of animal. Further, quantitative information is essential to determine the content of these accessory food factors in different food stuffs, and to apply our knowledge intelligently for therapeutic purposes to human beings.

For many years at the Lister Institute, beginning in 1907, many of my colleagues, and other workers there (Cooper, Chick, Funk, Harden, Zilva, Hume, Delf and others), have been striving to place the subject

upon a quantitative basis as regards the water-soluble vitamines, lack of which causes beri-beri and scurvy. Unfortunately you cannot estimate these properties other than by laborious experiments on animals, experiments which last from three to six months. When you ask a question and want to know the answer before proceeding the next step, and you have to wait weeks or months for it, it is a tedious matter. This kind of work needs people of very high character, and although, in 1907, I began to work at the subject of beri-beri, I was soon glad to pass it on to Messrs. Funk and Cooper, at that time working in my laboratory, well recognizing my unsuitability for work making such demands.

I will just bring before you now what have been the results obtained as regards the minimum requirements of the antiscorbutic accessory factor for different kinds of animals, and as to the amount of antiscorbutic vitamine in some common foodstuffs. If I write the figures on the board they will serve as a text.

Not being able to separate this vitamine in a pure form, it is necessary to find a substance which is fairly uniform and contains roughly the same amount as a unit. Lemon juice has been adopted by Dr. Chick for scurvy. We will express it in cubic centimetres per kilo weight of the animal. For rats the amount is negligible. Dr. Harden and Dr. Zilva have reared rats without antiscorbutic substance, and they have bred, and the two generations together have existed over a year and are in very good health, and the only noticeable deficiency in the second generation was, that they would not breed with one another. On the other hand, in the case of the guinea-pig, the amount required per day is as much as 4·5 c.c. per kilo, and for the monkey 0·6 per kilo.

With regard to man, what shall we say? The evidence of clinicians appears to be that something like 1 dr. per day for a child 6 months old is not more than a minimum. Dr. Still tells me he encounters cases of infantile scurvy which have had much larger quantities than that. But if it were 1 dr. per day, that would come out at 0·64 per kilo as a minimum, which makes it very similar to that for the monkey. The amount required by rats is so small that if one were working with these animals one would conclude there was no such thing as an antiscorbutic vitamine, and that indeed was the conclusion come to by McCollum.

Having, by working quantitatively, arrived at the minimum require-

ment of one species of animal, it is possible to evaluate various foodstuffs, and this for practical purposes is very important. That has been done by Dr. Chick and her comrades, and a summary of their various results was published by her in the *Wiener klinische Wochenschrift* in December last, for the benefit of the profession in Vienna. Dr. Chick took lemon juice as a unit, and called the value of 1 grm. of it 100. Cabbage juice and orange juice are about equal, and also 100. Swede juice is good, being represented by 60 on this scale. Green French beans have the value 30. Then we drop away. The carrot, beetroot, beef juice, and lightly cooked potatoes are about the same, with a value of 7·5. Fresh milk is about 1 to 1·5.

The experimental error is rather large—50 per cent. plus—or minus —largely owing to idiosyncrasy in the animals. Nevertheless the results have taken years to obtain. In the same way, having attained a quantitative basis, the effect of the treatment which the food commonly undergoes before consumption, the most obvious of which are heating and preserving, has been ascertained by Dr. Chick, Miss Hume and Miss Delf and Dr. Tozer. If food is heated long enough, all the antiscorbutic property disappears. In some cases it disappears much more readily than in others. In the case of such foods as milk, potatoes, and cabbage the deterioration in antiscorbutic value has been quantitatively shown by Miss Hume, and by Miss Delf and their colleagues, as also in the case of acid fruits and fruit juices, which turn out to be much more resistant to heat. Surely these facts must be of use when you come to deal with your little patients.

Dr. Barton.

I wish to show you this afternoon two substances which I have found of very considerable value at University College Hospital. During the war, when the air raids, combined with deficient food, put nursing mothers off their milk, and hundreds of breast-fed infants in the district had to be artificially fed, we were hard put to it, without milk often, and cream always, to feed the multitude. The best substitute I found was dried milk, and infants made weight on it better than they did on anything else. Still, one felt it was a devitaminized diet, and in view of the investigations at the Lister Institute, it was clear we were not doing our duty to these infants unless we added what was lacking.

Therefore I went down to see Miss Chick on the subject, and she suggested I should use an orange juice in which the rind was minced with the pulp, thereby adding to the juice the preservative effect of the essential oil of orange present only in the rind. She showed me a specimen of such juice many months old, but this contained an amount of the pulp undesirable in the feeds of very young infants. In hot weather I could not get the juice to keep sound over a fortnight. This juice did well, and never disagreed, but a time came when oranges were scarce and juiceless, and I had the temerity to try juices expressed from fresh vegetables, notably lettuce, but my experience leads me not to recommend them. The juices of fresh vegetables seem to be a potent culture medium for every kind of organism, and they smelt badly after a day or two, and as diarrhœa seemed to be caused by such juice I quickly abandoned it. If anyone could give us an efficient preservative for fresh vegetable or fruit juices we should be very grateful.

The other substance I wish to bring to your notice is a cream. When cream could not be obtained the need for something of the kind became urgent. For a year I used Marylebone cream, but felt again that, in view of Dr. Mellanby's researches, the addition of fat-soluble A was wise. Marylebone cream, being composed of linseed oil, contains practically none. I therefore asked Mr. Hampshire to devise a cream of beef-suet, and here it is. I have used it for the last eighteen months, and it answers well. The difficulty in making suet into an emulsion lay in its high melting point, but by the addition of 2 per cent. olive oil we managed it, and you will see some under the microscope. The percentage of suet in the cream is 25, about that of skimmed cream; it keeps for months, is free of tubercle, and is quite palatable; it is not expensive, and is made commercially as "University cream."

Dr. Eric Pritchard.

I have a rival cream to Dr. Barton's, and I would like to hand it round for examination. He mentioned Marylebone cream, which I introduced many years ago, and used for some time in combination with separated dry milk. Infants fed for six months on this food did not do very well, so I tried a different sort of fat. I thought I had not succeeded with this cream because it was made with linseed oil and did

not contain the same neutral fats and in the same proportion as exist in breast milk. I therefore asked our dispenser to make a formula which should contain the various fats in exactly the same proportion as exist in breast milk; with this modified Marylebone cream I was highly successful. One of the fats used in this cream was Hugon's suet. We have now obtained a still better cream, in which all the fats are in the same proportion as in human butter fat. It should contain also a large percentage of the fat-soluble vitamine, since it is made from fat which is reputed to contain the largest amount—except, perhaps, cod-liver oil —of any known fat, namely, the depot fat round the kidney of the ox, commercially known as oleo-fat. It is at present difficult to procure, as it is largely used for margarine. This fat from around the kidneys is submitted to hydraulic pressure at a temperature of 120° F., an operation which squeezes out most of the stearin, which is used for the manufacture of soap. The remainder, which is a fairly liquid fat at low temperatures, is highly charged with the fat-soluble vitamine. To bring this mixture to a proper composition, it is mixed with various other oils, chiefly nut oil.

Referring to some of the more general points in this discussion, I feel, with Dr. Hutchison, that we need not fear lest these views are not taken seriously enough: the danger rather is that some will take them too seriously. I have evidence on all hands that the lay public is beginning to think that rickets can be cured or prevented by a teaspoonful of cod-liver oil. My experience of rickets dates a long time back. The first cases I examined critically were those in which, so far from there being a deficiency of butter fat, the latter was used in great excess. Rickets I regard as one of the terminal results of bad feeding, resulting in defective nutrition, and I think there is real danger in attaching the term "antirachitic" to any particular vitamine. It tends to obscure the real pathology. It arises from many causes, chiefly from food causes, but not entirely so. I think the great value of this meeting will be that it will give clinicians an opportunity of explaining their difficulties to the experimental pathologists, and asking for their help in particular directions; we are not in a position ourselves to experiment on our patients and thus elucidate the difficulties of the problem for ourselves. I attach very great importance to these accessory food bodies, and the more we learn about them from experimental work, and the more we apply knowledge thus gained to our patients, the better will be our results. My results have been much better since I have

known more about the special properties of the accessory factors. We want to know, however, what are the particular symptoms of malnutrition which follow the deprivation of these accessory factors. For my part, however, I believe that the majority of the cases of malnutrition with which we have to deal are due not to deficiency, but to excess of some element or elements of food, and there may be symptoms due both to the one and to the other.

I would like to know, for instance, what are the symptoms of malnutrition due to the following dietetic errors: those due to excess of any or all elements, those due to deficiency of any or all, and those in which there is a combination of the two defects. The term "scurvy rickets," which I think originated at University College Hospital, still survives, as if scurvy rickets was a definite disease entirely. There are curious cases—now less common than they were—due to want not alone of the antiscorbutic factor, but due to want of the antineuritic factor as well. Those are the cases, *par excellence*, which have been fed on Allenbury's foods. In such cases there is a mixture of symptoms, due to the want of both the antiscorbutic and the antineuritic factor. They are cases of fat children, showing, more or less, general non-pitting œdema, and in them there is as a rule a marked pseudo-paralysis.

I think also we occasionally meet with another group, due to want of the fat-soluble factor affecting the growth and the susceptibility of the child to infectious disease. Many of the cases of rickets and deficiency disease are complicated by infections, and I think it is infection which may explain the cases Professor Noël Paton referred to, in which the animals could not recover until they were removed to new kennels.

Dr. W. M. Feldman.

I have listened with very great interest to all the speeches and have felt particularly interested in the work of Professor Noël Paton. He attributes rickets to confinement in a close atmosphere. My own clinical observations have shown that confinment *alone* will not produce it, because one finds some children in poor neighbourhoods who are closely confined to the house and yet do not get rickets. On the other hand, I have seen cases of rickets in some children of well-to-do parents, who were brought up on the most hygienic principles as regards air, food and exercise. These facts are not only against Professor Noël Paton's

theory but are also not in accordance with Dr. Mellanby's vitamine theory. It appears to me that amongst the various factors which combine to produce rickets some regard must be paid to the hereditary element. In those children of good families who showed signs of rickets I found some evidence of past rickets in one or other of the parents.

Dr. Lawson Dick.

I wish to join other speakers in complimenting the openers on the way in which they have brought this subject before us. One or two points make it difficult to accept absence of fat-soluble A as the essential cause of rickets. In the East End of London in the L.C.C. schools and dealing chiefly with Jewish children, it was found that: 81·4 per cent. of mothers suckled their children, whilst 18·6 per cent. used cows' milk.

Jewish mothers seldom use condensed milk or artificial foods. The feeding of these infants was excellent, yet 80 per cent. up to 12 years of age presented definite signs of having suffered from rickets. These facts were brought before the Society by me some time ago in speaking of the teeth in rickets. Those who know the state of nutrition of Jewish mothers are aware that they make excellent mothers—their general health is good and their fat nutrition is excellent. Prolonged lactation is blamed in these mothers. In China and India and the East generally prolonged lactation is the rule. Mothers frequently suckle their children for two or three years, even continuing during pregnancy until two children are being suckled at the same time, yet rickets never results.

Another point brought out is the absence of fat-soluble A from lard or at least its great deficiency. If fat-soluble A is so important to the nutrition of the child this is a somewhat peculiar result. Clinically, bacon fat is an excellent way of giving fat to the very young child.

Again, rickety children by no means always suffer from malnutrition. Frequently they are very fat, especially when fed on sweetened condensed milk. The child may be so fat that it is difficult to feel the ribs or the epiphyses of the long bones and yet the child is distinctly rickety.

There is the geographical aspect. We ought to look at rickets from a much wider survey. From 40° to 58° N.—say from the middle of Italy to the North of Scotland, and extending across Europe and

America—will include the whole belt in which rickets is rife. It is a temperate zone, a zone of deciduous trees, and a wheat bearing zone. But, above all, it is a great industrial zone, the great industrial zone of the world. It is the zone of great cities and dense populations. Only in China perhaps, is there anything to compare with it in this respect, and there it is a rice-fed population. It appears to me that vitamines have not been proved to be a serious factor in producing disease in this country. We live in the wealthiest portion of the world, where the people live on the most varied diet, and yet we find these deficient factors in food blamed for causing rickets. It is, I should imagine, very difficult to exclude a sufficient quantity of vitamines from any child's diet in this country. In China and Japan it is different. Professor Castellani, in his book on tropical diseases, says 400 millions of Malays, Chinese and Indians, live on rice, and there you have the ideal conditions favouring diseases due to deficient vitamines, and so, on this view, you would expect to find those diseases. Yet in these countries for all practical purposes rickets is never seen. The question of the occurrence of rickets in dogs is a very difficult one. They are prone to rickets. Stoeltzner pointed out long ago the tendency they show to pseudo-rachitic osteitis. I have had the opportunity of seeing some of the specimens of Dr. Mellanby, and they are most excellent. I am sure this work is well worth pursuing. The teeth changes produced are different in dogs to what we see in children: there is an expanded cavity in Mrs. Mellanby's cases, and no enamel, quite different from the hypoplastic type seen in young children, in whom the dentine is very dense; it is remarkable how well these hypoplastic teeth frequently last.

Dr. MELLANBY (in reply).

Most of the speakers would apparently be whole-hearted believers in the importance of accessory food factors in child nutrition were it not that they are blessed with the name "vitamines." The word "vitamine" is unfortunate, and it may not be satisfactory to all to speak of the "antirachitic accessory factor," but I have worked at rickets for some time and, for my purpose, at any rate, it suits. We must give a name to the substances, and the great need is to get the facts linked up. It is useless to tell us to take broader views; when we take broad views,

we never seem to get to the point. Rickets is a disease whose source we can elucidate, and although it is necessary to start an investigation with broad views, the experimental method allows us to eliminate one factor after another until we can get the issue narrowed down to the closest limits. The subject is of tremendous importance and does not end with rickets, because in these cases the whole vitality of the child is lowered and it may succumb to one of many diseases. One speaker mentioned non-resistance to infection. There is no doubt that this is a fact and the low resistance of my puppies to infection has always been a source of great trouble. Three weeks ago, I got an outbreak of distemper and, as a matter of experience, I knew it was of little use continuing a comparative investigation with them, so in a few days the whole lot were killed and a fresh beginning made.

Dr. Lawson Dick said in the discussion that the dogs' teeth in my wife's work, which he has had the opportunity of seeing, are not comparable with those of children seen in the East End schools. That is a matter of opinion. Some medical men and dentists have expressed the view that the teeth changes are comparable in the two cases. Naturally, because of the necessity of producing rapid results, the diets of the dogs are often so deficient as to produce results more exaggerated than are met with in children.

The most emphatic criticism which we have heard in this discussion has been that of Professor Noël Paton. It is surprising how unanimous speakers have been in regarding these vitamines of importance and how closely the animal and clinical results have agreed. Professor Paton is hardly prepared to admit there are such things, and, in any case, he thinks we are making far too much of them. On behalf of those who are working in the laboratory on the subject, I disclaim any blame for people taking this excessive view of things. That has been largely due to the subject having been written up in the press, and those who have done it have not always properly followed what we have said and have often given their imagination too much licence. The laboratory worker has, on the whole, been careful in making claims, and has usually done so with a full sense of responsibility. In the opening remarks of this discussion, I spent most of the time dealing with the relation of the antirachitic factor to the other elements of the diet, and I think this is probably the first time this relationship has thus been emphasized. That people write extravagantly about vitamines is not our fault.

So far as I could follow, Dr. Hutchison's remarks as regards children agreed very well with what I said about rickets in puppies. He took the attitude of the canny Scot, but agreed with my facts. If the facts are accepted, I can conceive of no other course than the whole-hearted acceptance of the vitamine hypothesis.

It is difficult for me to say anything about Professor Paton's criticism. He is a member of the Medical Research Committee, for which body all this work has been carried out. Anyone can see my work at any time. It is only a question of asking me and I shall be glad to go over the results with anybody who wishes it. It would be much easier and more satisfactory for a member of the Medical Research Committee to examine my results at close hand than to come here and condemn my work by saying I have produced insufficient evidence. My research on rickets has lasted about five years, the last three of which I have concentrated on the vitamine hypothesis. I was led to this explanation solely by my experimental results and against my inclinations. That the results have not been published in full means that the problem has not yet been solved; what has been published simply indicates the type of work being done at the time and the trend of the results. It has given the opportunity to other workers to join in, repeat and extend the work.

Professor Paton made a great deal of the fact that in my lectures on rickets, in 1918, I did not take the dogs in families. According to him, only comparable results on dogs of the same families are of any value. I have, as a matter of fact, made many comparative experiments on families, but the problem to be solved is not one of rickets in families but rickets all over the country. It would be easier to work on dogs of the same breed and age, but that condition I have not been able to secure. I have had to work on any puppy I could get hold of. Moreover, if the problem can be solved with dogs of different families and breeds and all the facts made to fit it, the work will obviously be the more convincing.

Professor Paton also criticized me for having pointed out that several of Findlay's dogs died during the experiment, whereas I did not mention how many of my own died. Findlay set out to prove and claimed to have proved that rickets is due to lack of exercise. Therefore, I think he should have taken the precaution to see that his puppies had a perfect diet. If some of his animals died, it is evidence to me that they did not have a perfect diet. My animals die, especially if any infection

gains access to the kennels, because the majority get an unbalanced diet. Therefore he misses the whole point of my criticism, because all my results point to diet being the prime cause of illness and death in early life.

If we assume, as has been suggested, that the hereditary element explains the occurrence of rickets, we shall become involved in a hopeless kind of problem. I do not mean that heredity has nothing to do with its occurrence, but I shall only fall back on that hypothesis when all other means have failed. Some animals are more susceptible to lack of vitamine than are others, but I suggest to you that this is largely due to the variability in rate of their growth and metabolism of the animals.

Section for the Study of Disease in Children.

President—Mr. J. P. Lockhart-Mummery, F.R.C.S.

Case of Hemi-hypertrophy.[1]

By H. H. Chodak Gregory, M.D.

Patient, a female, aged 2½ years, with marked asymmetry of the body and limbs. The mother thinks that the disproportion noticed at birth is the same now as then. The father and mother are normal and the other children are said to be symmetrically developed. The left leg and foot, the left arm and hand, the left side of the abdomen, chest and tongue are all obviously larger than on the right. But the right side of the head is slightly larger than the left. Judging by the size and shape the right side appears to be the normal, the left being abnormally developed. The actual measurements are: Arm—acromial process to end of thumb, right 11½ in., left 13½ in.; circumference, middle of upper arm, right 6 in., left 7 in. Legs—anterior superior spine to internal malleolus, right 14¼ in., left 15¼ in.; circumference, middle of thigh, right 10 in., left 11½ in. Abdomen—spine to umbilicus, right 10 in., left 11¼ in. Thorax—spine to mid-sternum, right 9½ in., left 11 in. Skull—greatest semicircumference, right 6½ in., left 5½ in. The tongue appears to be larger on the left side; the left side of the face is also a little bigger.

The condition is not hereditary, and is rarely so. It was noticed at birth: the mother said that on the first day she saw the right side of the child, and thought what a nice baby she was, but it was not until she saw the left side that she noticed how small it was. The right side of the skull is the larger. As the hypertrophy is on the left side as far as the limbs and the trunk are concerned, it suggests that the right hemisphere of the brain is larger than the left.

[1] At a meeting of the Section, held March 26, 1920.

Cases of hemi-hypertrophy are not easily looked up in the literature, because of the varied titles under which they are recorded. Broca reported a case of the kind in a boy aged 11 years. That was a hypertrophy on the left side, and was noticed in the first month of life. It involved the arm, hand, thorax, head, face and tongue. Hearing was said to be more acute on the left side than on the right, but sight was not so good on that side as on the other. Dr. Hutchison reported two cases in 1904 and 1916, and they were particularly interesting because there were post-mortem examinations. One of those children was aged 4 months, the other 5 months. Both died of acute diseases: one of broncho-pneumonia, the other of pyelitis. In both those, the limbs were affected, but not the skull or face, and in both the internal organs were asymmetrical. The left kidney was twice the weight of the right one, and the left testicle five times the weight of its fellow. The left lobe of the thymus was larger than the right lobe. In the second case the right suprarenal body was three times as large as the left; there were three accessory suprarenal bodies, and the right testicle was larger than the left. These are the only cases of which I have found records when looking up the subject.

The ætiology of this condition is unknown. Garson measured seventy skeletons, and found that only 10 per cent. were truly symmetrical, so that some degree of asymmetry may be regarded as the rule, and I suppose that hemi-hypertrophy must be looked upon as a freak production, that is, a mere exaggeration of the normal asymmetry.

It will be a matter of some interest to see how much, if any, disability this child experiences as she grows up. At present she seems to be quite healthy. Although her right leg is longer than the left one, she tilts her pelvis when walking, so that very little limping is noticeable.

DISCUSSION.

Dr. COCKAYNE: Purves Stewart,[1] in his book, describes and figures a case of hemi-hypertrophy of the left side, in which the right side of the cranium was larger than the left. The bones of the face and of the rest of the body were larger on the left side.

Dr. F. PARKES WEBER: I cannot see, in a case like this, which is indeed a case of great interest, that one is justified in speaking of it either as hemi-hypertrophy or as hemi-atrophy. The limbs on both sides are quite

[1] Purves Stewart, "Diseases of the Nervous System," 4th ed., 1916, p. 308.

shapely at present. When the child grows up there may be a lack of shapeliness on one side which may show that that is the side which is abnormally developed. At present, however, I do not see how we can say more than that there is asymmetry between the two sides, a volumetric asymmetry. It seems really only natural that occasionally, in both lower animals and human beings, one side of the body should be, for some reason, larger than the other. I quite admit that so much difference in size between the two sides of the body is rare, but a certain amount of asymmetry between the two sides of the face is very common, and it is customary for people who are particularly concerned about their looks to take care that photographs of them present the more pleasing side.

Dr. CHODAK GREGORY (in reply): I do not agree with Dr. Parkes Weber about the shapeliness of this child's limbs on the left side. On the right side the limbs are much like those of the ordinary child, but on the other side they are thicker and more stumpy-looking.

Rheumatoid Arthritis with Nodules.

By D. H. PATERSON, M.D.

I. P., FEMALE, aged 8 years. Admitted February 25, 1920. Complaint: Swollen joints, stiffness of the neck. Onset gradual. Duration, twelve months.

Family history: The third child of the family. Father alive and well except for bronchitis, aged 46 years. Mother alive and well, aged 35 years. Brother alive and well, aged 3 years. Four sisters alive and well, aged 10, 7, 5 and 1 year respectively. One died at four days, after a difficult labour. There were three miscarriages distributed among the other pregnancies.

Previous health: Full-term child, normal labour. Breast fed for three months, then cow's milk and water half and half. She cut her first tooth at the age of 6 months, sat up at 9 months, walked at 13 months, talked at 14 months.

Hygienic surroundings: Family of eight live in two rooms at the top of the house on high ground, but the rooms are damp. The district is Upper Holloway. Whooping-cough at the age of 3 years. Scarlet fever at 4 years. Chicken-pox at 5 years.

Present illness: It was first noticed that the wrists were swelling about twelve months ago. She was treated at another hospital for sprains. She was brought to Great Ormond Street in May, 1919.

She was admitted, and there were definite nodules on her elbows, and painful wrists and ankles were noted at the time. There was no heart lesion. Because of the nodules her condition was thought to be acute at that time and treated as such. She was however discharged with a large nodule on her right elbow as she showed no other symptom of acute rheumatism. Practically all the joints of her body have been affected since this time. She has never been quite free or fit. It was only a week before admission that her neck became affected.

Present condition: On admission patient was seen to be a rather thin, hollow-eyed child with auburn hair. Both wrists were thickened



A nodule may be seen on the right elbow. The characteristic hands of rheumatoid arthritis are well shown.

and the backs of the hands were swollen with atrophy of the interossei. There was a large nodule on the left elbow as well. The epitrochlear glands were enlarged on both sides and there were shotty glands along the posterior border of the sternomastoid muscle on the right side also. The neck was stiff and sore when moved. The other joints did not seem involved. Circulatory system: Heart regular; no bruits; cardiac dullness not increased. Respiratory system: Chest rather flat; percussion note resonant throughout; breath sounds vesicular. Digestive system: Tongue clean; teeth good; tonsils have been removed; abdomen well formed; liver and spleen not palpable; no areas of rigidity or tenderness. Nervous system: Pupils react to light and

accommodation ; abdominal reflex present ; knee-jerks present ; plantars flexor ; temperature has been normal since admission. Occasionally she gets pain in her wrists. The radiographer reports general bony changes.

DISCUSSION.

Dr. F. PARKES WEBER : I think the nodules are true rheumatic nodules, and that the chief point of interest is the absence of any sign of valvular disease of the heart. But I would like to ask whether there is not an adult type of genuine rheumatic nodules unaccompanied by any sign of valvular disease.

Dr. F. LANGMEAD : This case suggests a border-line between acute rheumatism and rheumatoid arthritis in children. The enlarged joint seems to be very characteristic of rheumatoid arthritis, and the association of enlarged glands, especially enlarged epitrochlear glands, incline me to agree with the diagnosis. The occurrence of true rheumatic nodules in rheumatoid arthritis is the point of greatest interest. Some believe that the same infection is capable of producing both acute rheumatism and rheumatoid arthritis, and this case appears to favour that contention. On the other hand, the clinical pictures of the two conditions are to my mind very different. It might be worth while in this exceptional case to remove one of the glands. Such glands have been removed in cases of rheumatoid arthritis, not infrequently, in the hope of ascertaining the nature of the infection, but, as a rule, their contents have proved sterile. These nodules are of a different kind from those usually seen in rheumatoid arthritis in adults, and sometimes in children, which are flat nodules or plaques, found in the same situations as in this case, in the tendons at the olecranon process and elsewhere, but they have not the characters of the rheumatic nodule nor do they appear and disappear rapidly, though they may disappear slowly. Rheumatoid arthritis, a symptom-complex, and not a disease, is capable of being produced by the toxins of various infections, and amongst them, in view of this case, must perhaps be added the rheumatic infection, though, I think, it provides only a very small proportion.

Cases of Anæmia with Enlarged Spleen.

By D. H. PATERSON, M.D.

CASE I.

R. C., aged 4 months. Admitted March 23, 1920, under the care of Dr. Poynton. Complaint, a lump in the abdomen. Duration of illness, a fortnight.

Family history: Father aged 32 years; mother aged 33 years; both quite healthy. Married ten years. Three children, no miscarriages. Two girls, aged 13 and 7 years respectively, both apparently healthy. No jaundice in the family.

Previous health: Full-term child, normal labour. Breast-fed since birth, and up to admission. He was not jaundiced at birth. The child thrived well for two weeks. He reached 12 lb. at that age, but since then he has lost weight. He was always a pale baby.

Present illness: For two months he has been losing weight rapidly. A month ago he was treated by a doctor for bronchitis, but he did not improve. A fortnight ago it was noticed that the abdomen was becoming larger, and the doctor found a lump there. There has been no vomiting. He has had no fits and he feeds well. During the past fortnight he has had diarrhœa, passing loose green stools immediately after feeding. He has been peevish and restless for the past two months.

Present condition: A fat baby in poor flabby condition. He is active and cries loudly with plenty of tears. There are no bony deformities. His head measures $15\frac{3}{4}$ in. in circumference and is square. The fontanelle is open wide, $1\frac{1}{2}$ in. across. No cranio-tabes. The child inspires with a snore as if there were some pharyngeal obstruction. Nothing abnormal palpable in the posterior pharyngeal wall. There is a slight napkin rash on the buttocks. Cardio-vascular system: Pulse palpable at the wrist, normal tension, 94 per minute; apex beat palpable in the nipple line; no apparent increase in the area of cardiac dullness. Respiratory system: Respiration 30 per minute; no dyspnœa nor distress; air entry into both lungs is good; breath sounds vesicular throughout. Alimentary system: Abdomen prominent, circumference $17\frac{1}{2}$ in.; a small umbilical hernia is present; the abdomen yields on palpation; the left flank is occupied by an enlarged mass which seems to be the spleen; it has a hard edge and extends towards the mid-line within 1 in. of the umbilicus; the liver is enlarged also, there is no free fluid; the liver margin is one and a half fingers' breadth below the costal margin at the ninth rib. Nervous system: He is a bright baby; the pupils are normal and react to light; the knee-jerks are present; there is an active plantar response; the legs move quite freely; there is no enlargement of the lymphatic glands; the stools are normal in colour and consistency; the urine is normal. Blood count: Red blood cells, 3,440,000; white blood cells, 18,000; hæmoglobin, 30 per cent,; colour index, 0·4. Differential count: Polymorphonuclears, 29 per cent.; small

lymphocytes, 35 per cent.; large lymphocytes, 16 per cent.; transitional cells, 1 per cent.; neutrophil myelocytes, 12 per cent.; basophil myelocytes, 3 per cent.; normoblasts, 2 per cent.; megaloblasts, 12 per cent. Polychromatophilia. Wassermann reaction negative.

CASE II.

J. D., aged 5 years. Admitted March 5, 1920, under the care of Dr. Robert Hutchison. Complaint: Does not eat, prolapse of the bowel. Onset: Eighteen months; has not wanted her food for a fortnight.

Family history: Patient is the sixth child; father alive and well; mother alive and well, aged 39 years; four brothers, aged 16, 14, 11 and 1½ years respectively, all alive and well; three sisters, aged 17 years, 10 years, and 4 months, all alive and well; no miscarriages.

Hygienic surroundings. Family of eight live in a six-roomed house; it is dry and well ventilated.

Previous health: Full term child; natural labour; breast-fed until the age of 5 months, was then fed on cow's milk and water. Was not backward in any way; had measles at the age of 2 years.

Present illness: Child was quite well until eighteen months ago, when prolapsed bowel first appeared; since then she has been pale and has had a cough; for the past fortnight she has been off her food: she often eats the cinders from the fireplace and picks the mortar from between the bricks and eats it.

Present condition: Patient is a pale, rather puffy-faced anæmic-looking child; she is small but well nourished, her muscles are however very soft and flabby; her head, nose and eyes are well formed; the superficial lymphatic glands are not enlarged anywhere; her back and limbs are straight; the sclerotics are pale; there is beading of the costochondral junctions. Digestive system: The tongue is pale, moist and clean; the teeth are good; the mucous membrane of the gums and fauces is very pale; the tonsils are not enlarged; the abdomen is uniformly distended; the umbilicus protrudes; the liver is palpable one finger's breadth below the costal margin; the spleen is palpable more than one finger's breadth below the costal margin; there are no areas of tenderness; no other tumours are felt. Circulatory system: No abnormalities noted. Respiratory system: No abnormalities noted. Nervous system: No abnormalities noted.

Blood count, March, 7, 1920: Red blood cells, 3,680,000; white blood

cells, 7,200; hæmoglobin, 32 per cent.; colour index, 0·40. Differential count: polymorphonuclears, 46 per cent.; small lymphocytes, 32 per cent.; large lymphocytes, 19 per cent.; large mononuclears, 2 per cent.; myelocytes, 1 per cent.; stools normal; urine normal; a soft systolic murmur now heard over the præcordia.

March 14, 1920: Spleen enlarging; it is now down more than two finger breadths below the costal margin; liver is becoming more palpable also. Blood count: Red blood cells, 2,640,000; white blood cells, 14,000; hæmoglobin, 25 per cent.; colour index, 0·4. Differential count: Polymorphonuclears, 49 per cent.; small lymphocytes, 21 per cent.; large lymphocytes, 18 per cent.; large mononuclears, 4 per cent.; transitionals, 1 per cent.; neutrophil myelocytes, 2 per cent.; basophil myelocytes, 1 per cent.; megaloblasts, 3 per cent.; normoblasts, 1 per cent. Both poikilocytosis and polychromatophilia were noted. Wassermann reaction negative.

May 23, 1920: On this date the liver and spleen were no longer palpable.

Dr. F. PARKES WEBER: The first of these two cases, that is, the child 4 months old, appears to be a very remarkable one, since perhaps the spleen was large at birth. The younger the child, the more disturbance one expects to find in the blood-picture, because the younger the child, the more actively the hæmopoietic functions of the bone marrow tend to react to any toxic agency. Therefore one is not surprised that there are so many myelocytes and nucleated red cells in the circulating blood in this infant. I think the diagnosis is certainly justified. In the other case, also, I think the diagnosis which has been made is correct, that is, as far as our knowledge at present goes: I think our knowledge, altogether, is still very incomplete as regards the splenic anæmias or pseudo-leukæmias in infants.

Section for the Study of Disease in Children.

President—Mr. J. P. Lockhart-Mummery, F.R.C.S.

Three Cases of Renal Dwarfism associated with Curious Bony Changes.[1]

By D. H. Paterson, M.D.

For a great many years the condition of chronic interstitial nephritis in children has been well recognized. Dr. Leonard Guthrie's [1] series of cases in 1897 were particularly complete, and later Sawyer in 1903 [2] and 1906 [3] published articles on this subject. Hirsch [4] in 1904 also published a series of cases. In March, 1911, Dr. Morley Fletcher [5] showed the first case of chronic interstitial nephritis with infantilism before this Section of the Society. The child had a double genu valgum, but very little comment was made on this point at that time. The trouble dated from birth apparently, and growth had been very slow. At 6 years of age he was wearing the clothes that had been made for him when he was 2 years old. His intelligence was good, and his Wassermann reaction was negative. Parsons [6] published a case in the same year, and speculated on the ætiology of this condition. During the same year cases were reported by Reginald Miller [7], who noted an existing genu valgum and mental deficiency. Dr. G. A. Sutherland's case [8] of extreme rickets with infantilism might possibly have fallen into this class of case, as he notes that the child had marked polydipsia, and the urinary report would suggest chronic interstitial nephritis.

In the following year Dr. Parkes Weber [9] under the heading "Remarks on Polyuria in Children and Infantilism," discussed the whole question, and contrasted this type of case with those accompanied

[1] At a meeting of the Section, held April 23, 1920.

by diabetes insipidus. He referred to the cases published by Fletcher, Parsons, and Miller. Dr. Frederick Langmead noted two cases he had seen, and Dr. Naish [10] commented on cases he had seen and later published. Miller and Parsons [11], however, published a very complete paper shortly after this, and commented on the name of "renal infantilism" given the condition by Dr. O. May; one part of the paper dealing with infantilism with organic renal disease (chronic interstitial nephritis) and the other with infantilism without organic renal disease (diabetes insipidus). The first group of cases due to chronic interstitial nephritis they further subdivided into those with congenital symptoms and those in which the symptoms came on after birth. They specially noted the genu valgum present in some of these cases.

A typical case was also reported by V. Hutinel [12], who considered the bony changes in this condition midway between those of rickets and osteomalacia. Mr. Davies-Colley's case [17] of osteomalacia in a child aged 13 years, reported many years ago, showed extensive bony changes, but the bony condition and the condition of the kidneys were not the same. It is interesting, however, to note the effort to associate these two conditions. In 1913 [13], 1918 [14], and 1920 [15] Dr. Hugh Barber, of Derby, however, published seven cases, five of them having post-mortem findings and five of them showing bony deformities. In some of these the condition had been present since birth, and in others it had appeared later. To this he gives the name "renal dwarfism." He showed a radiogram of the wrist of one of his cases, and considers the bony changes similar to rickets.

The histories of my three cases are as follows :—

Case I was that of a boy, aged 11 years, who was admitted to the Hospital for Sick Children, Great Ormond Street, in January, 1917, for deformities of the bones of the legs and arms and inability to walk because of knock-knee. The condition had been present for four years. He was healthy at birth and breast-fed for nine months, but was afterwards kept on fluid diet longer than usual because of his dislike for solids. He had always been fond of fruit and was a great drinker of water. The father and mother and the four other children of the family were all healthy and showed no bony deformities, nor was there any family history of kidney disease. Until the age of 6 years he had been quite well, but then he had an illness which started acutely and was accompanied by feverishness and vomiting. After this he did not seem quite so well, and at the age of 7 years it was noticed that his legs were bent and he became progressively more knock-kneed. He

was treated for rickets by splints, but at 7½ years old was admitted to the local hospital, where it was found that he had kidney disease. The bones of his legs were broken and reset, and from that time on he had not been able to walk. When admitted to the Hospital for Sick Children, Great Ormond Street, he was found to be a very small, pale and very thin child. The legs were distinctly deformed, with bowing outward and forward of the femora and tibiæ; there was also marked muscular wasting. An operation for double osteotomy of the femora was done, and this was followed by uræmia which proved



FIG. 1 (Case I).

Femur showing a kinking-in of the shaft rather than a fracture. Callus has been thrown out. This deformity was bilateral.

fatal. The urine examined after operation contained albumin and casts. The X-ray examination revealed bony deformities of unusual character in the femur (fig. 1). This shows a fracture with callus thrown out at the damaged site, but it also suggests that there has been such thinning of the bone substance that the bone has kinked in like a tube of paper, rather than a breaking of the bony substance.

Post mortem the kidneys were extremely small and showed some

cystic and fibrotic changes. The only other organ which was found abnormal was the heart, which was dilated.

Case II was that of a boy, aged 1 year and 9 months, admitted to hospital in September, 1919, with deformities of the ankles and wrists, which had been present for ten months. There was no other deformity in the family. The boy had been breast-fed until 11 months old and had never been able to take any solid food. When a year old he was fat and could stand, but was small. He seemed to lose power in his legs, and later it was noticed that his arms gave way also and that he had no power in them. They afterwards became permanently flexed. After an illness, in March, 1919, associated with constipation and vomiting, he steadily lost weight. When admitted he was seen to be a very much under-developed and under-nourished child, and weighed 10 lb. 8 oz. He seemed to be bright and intelligent but did not speak. The thorax was long and narrow in shape and there were present parietal and frontal bossing, an open fontanelle, beaded ribs and Harrison's sulcus. The liver and spleen were both enlarged. The back was straight, but the limbs were extremely deformed; the femora were bowed and the patellæ rotated outward; the tibiæ were very bowed and the radii and ulnæ showed a like deformity. The hands were held in the accoucheur's position, but there was no suspicion of tetany. The respiratory, circulatory and nervous systems were normal. Ten days after admission the child's diet was altered to solid food, including minced meat and potato. He then developed uræmic convulsions and died. The Wassermann reaction was negative.

Post mortem, the left kidney was found to be extremely small and cirrhotic. The right kidney was slightly larger, but was also abnormal, its condition suggesting old inflammation and its capsule stripping with difficulty. Their weight was 7 dr. each. Both suprarenals appeared relatively much enlarged. Microscopically the sections of the kidney showed a condition of fibrosis to a marked degree, the kidney substance being replaced entirely in some areas by fibrous tissue. The arteries were greatly thickened, and many of the glomeruli were obliterated. In some places the tubules showed enlargement as though in an early stage of cyst formation.

Microscopically a section through the radius shows the following features (fig. 2):—

(1) There is a straight line across the epiphysis sharply marking off ordinary hyaline cartilage from a layer composed almost entirely of

cartilage cells arranged in columns, and with no visible intervening matrix. In this layer there are signs of normal calcification in isolated places. There is a tendency to cleavage between the two cartilaginous zones. The deep zone of cartilage cells passes somewhat abruptly into a close connective tissue in which there are abundant signs of cell proliferation.



FIG. 2 (Case II).

Radius; photomicrograph. The epiphyseal line **X** is seen to be straight. There is a complete absence of lime salts in the connective-tissue trabeculæ. (× 20.)

(2) The most striking feature of the section consists in the numerous trabeculæ, having the appearance of non-laminated bone-trabeculæ, but showing a complete absence of deposited lime salts and hardly anything in the nature of bone-cells. These trabeculæ were surrounded more or less completely with a closely applied layer of flattened cells, and from the increasing closeness of the texture of the connective tissue surrounding them it was evident that they had arisen from a metaplasia of the connective tissue medulla.

(3) Some distance from the epiphyseal area the spaces between the non-calcified trabeculæ begin to be occupied by a more normal fat marrow.

(4) Close to the line of fusion between the zone of cartilage cells and the medullary connective tissue are to be seen in one spot one or two very small bone trabeculæ (cells and lime salts) and these are evidently being formed like the others from the fibrous-looking



FIG. 3 (Case II).



FIG. 4 (Case II).

Fig. 3.—Showing fractures on the diaphyseal side of the epiphyseal line of the lower end of the femur and tibia.

Fig. 4.—Showing stippled appearance of the lower end of the radius and ulna with a callus thrown out.

connective tissue. The direction of the fibres is horizontal, or parallel to the edge of the cartilaginous zone, between the trabeculæ and the cartilage.

The X-rays in this case show multiple fractures on the diaphyseal side of the epiphyseal line (fig. 3). There is general osteoporosis of all the bones. The wrists show a characteristic stippled appearance (fig. 4). The actual shafts of the long bones show little or no bowing, which is also unlike rickets.

Case III was that of a girl, aged 7½ years, admitted for contracted legs and bent wrists, which had been present since birth. There was no family history of a similar condition. She was a premature child, born at the seventh month of intra-uterine life, and had been breast fed for six weeks. After that time she was given cow's milk and seemed to thrive quite well. She had never walked. At the age of 3 years a tenotomy had been done, as her feet were being drawn up. On admission she was 34 in. in length. Her head was square and bossed. The shoulders were sloping and rounded and the clavicles curved antero-posteriorly. There was possibly an old fracture of the right clavicle. Flexion, extension and abduction at the shoulder-joint were all limited. The humeri were very thin. The radius and ulna on each side showed great enlargement and deformity at the lower ends, giving the impression of old fractures which had united in bad positions. The thorax was long and narrow and pigeon-chested and the ribs were beaded. The liver reached two fingers' breadths below the costal margin; the spleen was just palpable. The legs would not fully extend at the hips, and there was considerable genu valgum, with rotation of the patellæ outward, the knees being incapable of full extension. The tibiæ were small and sabre-shaped, and the lower ends of the tibiæ and fibulæ were much enlarged. Dorsiflexion of the feet beyond a right angle was impossible. The back was straight. The urine contained a trace of albumin but no casts. Mentally the child was very backward and seldom spoke. The phenol-sulpho-phthalein test indicated a great retardation in the secretions of the kidney. The Wassermann reaction was negative. An attempt was made to correct her bony deformities, but the left leg was re-fractured while being manipulated. Two days before her death she developed fine tremors of her hands and a large amount of albumin was present in her urine. The urea was 0·3 per cent. and the nitrogen excreted as ammonia was to that excreted as urea as 1 is to 10. Creatinin equalled 8 mg. in 10 c.c. of urine. The

child became comatose and died, passing no urine during the last twenty-four hours of her life. The cerebro-spinal fluid was normal, but for a great increase in the urea present. The kidneys post mortem weighed 12 dr. each and the suprarenals 2 dr. each. The kidneys were cirrhotic, their capsules stripping with great difficulty, and on section several small cystic spaces could be seen. Microscopically there was marked increase of fibrous tissue throughout the kidneys and a tendency to

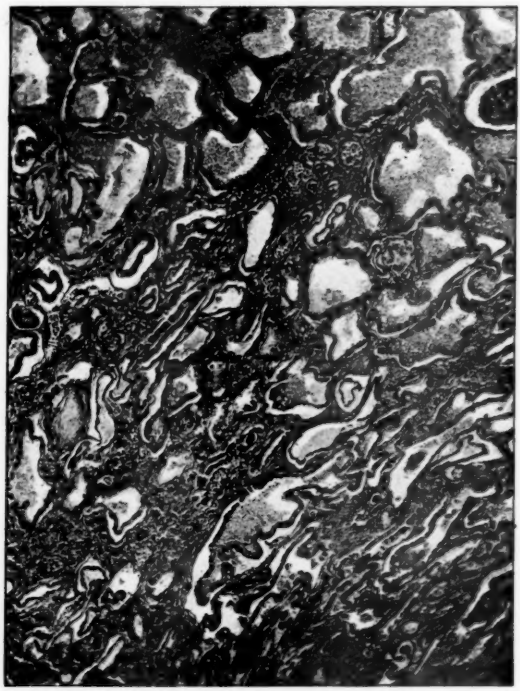


FIG. 5 (Case III).

Photomicrograph showing well-marked interstitial nephritis. Note the tendency to cyst formation. (× 100.)

cyst formation, the tubules being greatly enlarged and filled with colloidal material. The glomeruli were fibrosed in places (fig. 5).

The sections through the femur (figs. 6 and 7) showed the following points:—

(1) The epiphyseal line is in the main fairly straight, though downgrowths of cartilage cells do occur in places.

(2) The bone trabeculæ are sparse and non-laminated.

(3) The medulla is abundant between the sparse trabeculæ. It is formed of loose connective tissue and does not show much activity in cell-production.

(4) Isolated masses of cartilage cells have penetrated into this medullary tissue to a considerable distance from the epiphyseal line.



FIG. 6 (Case III).

Femur. The epiphyseal line, marked X, is in the main fairly straight. The bone trabeculæ are sparse and non-laminated. (× 25.)

(5) There is an absence of bone-forming activity. There are no osteoblasts arranged against many of the trabeculæ and when they are present they are few and ill formed. There is practically no sign of active bone formation or absorption.

The radiographs in this case (figs. 8 and 9) are almost identical with those of Case II, especially the wrists.

The radiographs in these cases showed multiple fractures, the site of which were on the diaphyseal side of the epiphyseal line. The epiphyseal line is unlike what is seen in rickets, since it is straight. The stippled appearance of the wrists suggest that there has been an attempt on the part of nature to repair the fractures and to throw out callus at this point. There was also general osteoporosis, especially at the growing points.

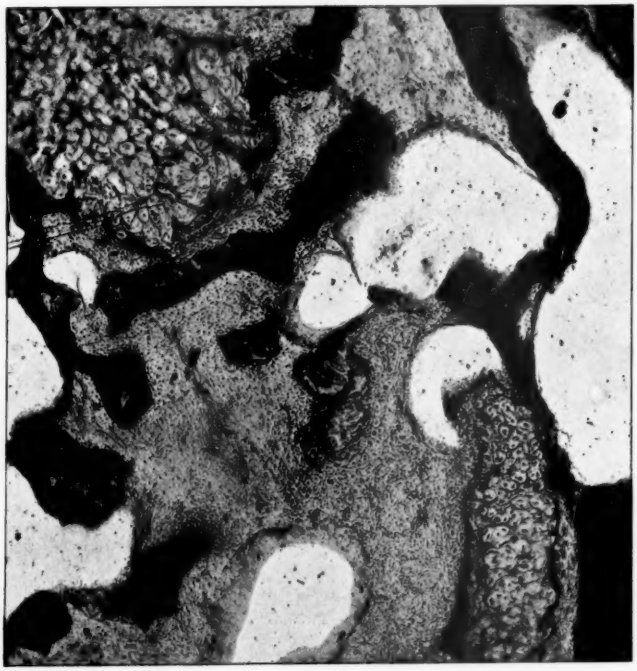


FIG. 7 (Case III).

Femur. The medulla is in large abundance between the sparse trabeculæ. It is formed of loose connective tissue. Isolated masses of cartilage cells have penetrated into the medullary tissue. There is practically no sign of actual bone formation or absorption. (× 100.)

Microscopically there is a resemblance to rickets in the following points:—

(1) In the excessive proliferation of the cartilage cells; the columns forming four or five cells abreast.

(2) There is one instance seen in which metaplasia of cartilage cells into bone is taking place.

On the other hand there is a complete absence of the osteoid tissue which is such a marked feature in rickets, in which the central portion of a trabecula shows deposited lime salts, and the peripheral portion is devoid of them.



FIG. 8 (Case III).

The lower ends of the radius and ulna show the same stippled appearance as in Case II.

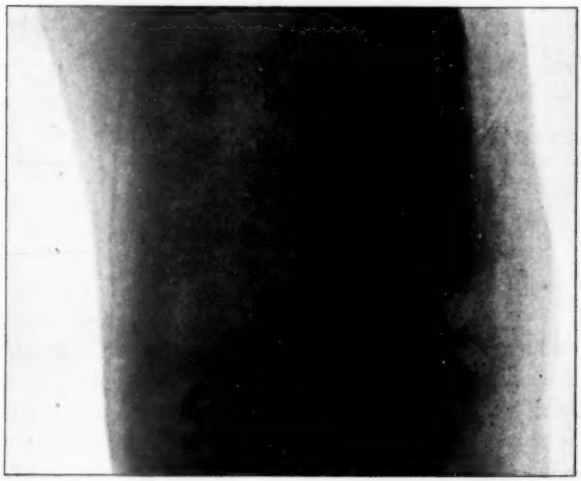


FIG. 9 (Case III).

The fracture seen at the lower end of the femur is bilateral and similar to that seen in Case II.

The inference from these sections is that active development of abnormal tissue is going on, but that there is an almost complete failure in one essential element of the osteogenetic process, namely, the deposit of lime salts in the trabeculæ. It is quite easy to understand how a very slight strain might lead to a breach of continuity through such soft tissue. The changes in all these sections are in the direction of the diminution of the osteogenetic function. I would suggest that these bony changes are nutritional in origin.

In conclusion, I wish to thank the honorary staff of the Hospital for Sick Children, Great Ormond Street, for allowing me to present these cases.

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Case of Renal Infantilism.

By H. Morley Fletcher, M.D.

The patient, H. K., a girl aged 14 years, has been weak and backward in development since infancy, and has suffered for years with severe frontal headache. Micturition has been frequent (two or three times nightly for many months). There has been no constipation nor diarrhœa. She has become much weaker and paler the last few months and for the last three days has been vomiting after food.

Past history: She had measles in infancy; scarlet fever two years ago. Her condition was not affected by these illnesses.

Family history: The father and mother, and seven brothers and sisters are stated to be in good health.

She was admitted to St. Bartholomew's Hospital on March 17, 1920.

Present condition: Very small for her age; weight, only 35½ lb. Face pallid. Is very poorly nourished. Intelligence very good. In spite of constant ill-health she was in Standard VII. There are no signs of advancing puberty, the mammæ are not developed, and there is no pubic hair. Her appearance generally is that of a child of between 5 and 6 years. The radial artery is small and wiry. Systolic blood-pressure 240, diastolic 190 mm. Hg. Left ventricle moderately hypertrophied, aortic second sound accentuated. Liver and spleen not palpable. Osseous system: Except for a slight degree of genu valgum, there is no obvious curvature or deformity of bone. Urine, slightly turbid; specific gravity 1010, acid, albumin 0·3 per cent.; no sugar. The centrifugalized deposit contains red and white blood cells, a few granular casts, and a few bladder epithelial cells; no crystals. Bacteriological examination showed the presence of a long-chained streptococcus, probably *Streptococcus fæcalis*, in all dilutions. Eyes: Mr. Foster Moore reports as follows: "The disks are much obscured by œdema, the disks themselves are somewhat pale, the arteries and veins are diminished in size, the veins are much cut into where the arteries cross them. Hæmorrhages are scarce and small. There is probably subretinal exudate, especially below the disk. In each eye there is a particularly well developed star figure, amongst which is a good deal of pigment proliferation, especially in the right eye. In a few places well-marked sclerosis of the choroidal vessels is visible—e.g., some distance vertically below the right disk. From the pallor and absence of marked swelling of the disk, the small retinal vessels, the absence of hæmorrhages and the pigmentary changes, I should judge retinal changes have been present for some considerable time—e.g., many months. There is no retinal detachment."

For the first few days after admission, the child suffered intensely from headache and occasional vomiting, but this gradually ceased and she improved considerably in condition. The systolic blood-pressure varied while she has been in hospital from 175 to 240 and the diastolic from 135 to 170 mm. Hg. Blood urea was determined by Dr. Canti as 0·09 per cent.

Dr. Mackenzie Wallis reported that the diastatic activity of the urine was 10 units, which may be regarded as normal. He found that

the urine contained a considerable amount of globulin as well as albumin. Urea concentration test gave the following result: 7 a.m., before the test, urea = 1 per cent.; 8 a.m., one hour after administration of 5 grm. of urea, urea = 0·09 per cent.; 9 a.m., two hours after, urea = 0·9 per cent. The administration of 5 grm. of urea did not produce any increase in the urea concentration of the urine. The blood gave a negative Wassermann reaction.

Since admission the child has remained in much the same condition, but during the last few days she has become rather more drowsy and apathetic, and the urine now contains visible blood.

There are one or two remarks I would like to make, which may be taken as having a bearing on both Dr. Paterson's cases and my own patient. The first of these is, that I think it will be agreed that the degree of dwarfism which is seen in cases of this type of renal disease in children is much more marked than perhaps any other form of infantilism which we come across. Some members may recollect the first case I showed, that of a small boy aged 8 years, who weighed only 21½ lb., and in whom polyuria was of such marked degree that he passed each day as urine about one-third of his body weight. Yet, though the bodily infantilism is so marked, it is very striking to note that these cases show quite a high order of intelligence. That is well exemplified in the girl I show to-day; at 13 years of age she was in Standard VII at school, which, as you know, is quite high.

An important point in these cases is as to the significance of globulin in the urine. In two cases which have been under my observation there was a large amount of it at one time, and it is present in the girl I show to-day. I would like to hear from those who know more about the chemistry of the urine than I do, what the explanation of that is; whether it bears any relationship to changes taking place in bones or other tissues.

The most obscure point in these cases is the association of this renal disease with the deformity of bone which seems so general, and is often of such extreme degree. In the little girl I have brought to-day it is not so marked: there is only slight genu valgum.

In a large proportion of these cases it is probable that the renal changes begin in uterine life or in early infancy.

As to the cause of these changes, I think syphilis can be absolutely excluded, and in nearly every recorded case syphilis has been mentioned as having played no part. It is probable that some infection of the urinary tract may be an important factor in the kidney changes: some

infection either primarily renal or one which has ascended from some bladder infection. That, however, is a point on which many more investigations are needed, especially in early cases of this type. I shall be glad to know whether Dr. Paterson had a bacteriological examination made of the urine in his cases, and if so, what was the result.

The duration of the condition is very variable, and this duration largely depends on the care which is exercised in looking after the patient. I fear my little patient is nearing the termination of her trouble by uræmia.

DISCUSSION.

Dr. H. C. CAMERON: In Dr. Paterson's account of the literature on this subject, with regard to a reference to a very early case which was described before the Pathological Society in 1883,[1] by Mr. Davies-Colley, the bones of which are in Guy's Hospital Museum. That case showed extreme dwarfism and deformity. In support of what Dr. Morley Fletcher has said in regard to the congenital origin of the kidney disease, I showed a case here, some years ago, in which the chronic interstitial nephritis was combined with a horse-shoe deformity of the kidney. I looked through the literature at that time, but I could not see any other instance in which there had been any gross anatomical congenital defect of the sort. But it suggests that, in that case at least, the nephritis was connected with a deformity. On the other hand, I am in doubt about one case which I have been watching at the present time. I think we should all agree that the appearance of patients with this condition is a highly characteristic one. The case of which I speak seems to me almost certainly to be one in which the bony changes are such as we have had demonstrated to us this afternoon. Moreover, the X-ray picture is typical. I have carefully examined the urine many times, I have had the urea content of his blood estimated, and the amount of urine he has passed ascertained, and I have not been able to find any evidence that this boy has chronic interstitial nephritis. There is no arteriosclerosis nor hypertrophy of the heart. And there the matter rests. I see the patient at the hospital at long intervals. In X-ray pictures I have not recognized the extreme number of fractures which Dr. Paterson's cases show. I have seen such fractures at times, and I have seen them with the characteristic disposition of callus, which Dr. Paterson's cases show so well. But then, on the other hand, my patients have been older—14, 15 and 16 years. I think it is most unusual to have this condition recognized, even post mortem, at so early an age: I do not remember the record of a case so early as 22 months. Therefore I think it is possible to associate the frequency of the fractures in these particular examples with the early age of at least two of them. Dr. Barber, of Derby, has published five of these cases, and has sent

[1] *Trans. Path. Soc.*, 1883, xxxv, p. 285.

me his paper on the subject, and he has alluded to a point which has come under my notice also—namely, the danger of operative interference in these cases. I think most of the cases of the kind which come under my care have been admitted to the hospital or to the orthopædic out-patient department, and have been sent up with the idea that some form of osteotomy should be performed to reduce the very striking deformities. Osteotomy is an easy thing to do in these cases, because the bone could be cut with a penknife, it is so soft. But in two cases in my experience uræmia has followed the giving of the anæsthetic, and I think that was also the history in two or three of Dr. Barber's cases.

Dr. F. PARKES WEBER: I would like to point out that there is a class of cases of children and young adults in whom diabetes insipidus is associated with some degree of infantilism. These differ very much from the class of cases which have been described by Dr. Paterson this evening, and by Dr. H. Morley Fletcher. In the first place, the degree of infantilism is not very great. In the second place, there is clinical evidence of the absence of organic disease of the kidneys. In the third place, there is no associated malformation or disease of bones. In the fourth place, the faulty organ seems to be the pituitary gland, probably the posterior lobe. In the fifth place, these cases may show a positive Wassermann reaction, and probably congenital syphilis sometimes or always plays a part in the ætiology. I suggest that in these cases, to which I am alluding, a slight syphilitic lesion, early in life, affecting the posterior lobe of the pituitary body, has something to do with the associated conditions of diabetes insipidus and infantilism. In regard to the cases which have been discussed to-day, I want to ask a question with reference to the "horseshoe" kidney mentioned by Dr. Cameron. Can Dr. Cameron tell me in what proportion of cases of horseshoe-kidney actual disease of the kidney substance is associated with the congenital abnormality? A few days ago I was shown a boy who had signs of chronic interstitial nephritis and died of uræmia. At the post-mortem examination a horseshoe-kidney was found, in which there were chronic interstitial changes, associated apparently with an acute exacerbation of parenchymatous nephritis. There was a history in the case of scarlatinal nephritis many years ago, but, on the other hand, one must remember that most patients get over scarlatinal nephritis completely, without any obvious chronic nephritis being left.

Dr. H. C. CAMERON (in reply to Dr. Weber): I do not know. I have had the experience of finding chronic interstitial nephritis in a horseshoe-kidney before more than once, and I have the general impression which I mentioned. I have tried to find out the frequency but I have no detailed series of observations or any evidence. I think it possible that in my case also it was a coincidence that the nephritis occurred. It seemed to me at the time that there might be other cases, too, in which renal infantilism was associated with interstitial nephritis occurring in a horseshoe-kidney, but I did not find any such records.

Dr. H. L. TIDY: Dr. Cameron has referred to a case in which he found no albumin in the urine, on repeated tests. I think that was so also in one of Dr. Barber's cases which he published, and that was of about the same age—12 to 15 years. In that case, too, there was no sign of thickening of arteries, and no increased blood-pressure, and when, finally, the boy died, interstitial nephritis was found to be present. Speaking from memory on this point, I think there was an interval of some length, before the death of the boy, during which the urine was not examined, owing to Dr. Barber's absence abroad. I have recently seen a somewhat similar case, which leads me to ask one question, applying especially to cases of later age: it is as to whether in those cases the blood has been examined. In the case to which I refer, joint changes commenced at 4 years of age, and the boy is now just over 14. About a year ago, just after he had gone to a public school, the relatives received a telegram from the school requesting them to fetch the boy home at once as he was suffering from pernicious anæmia. The spleen was three or four fingers' breadth down, and the blood picture corresponded with that of pernicious anæmia: there were about two million red cells, 45 per cent. of hæmoglobin, and a collection of normoblasts, megaloblasts and myelocytes, with occasional myeloblasts. Now, that is to say a year later, the boy is much better, and the spleen is much smaller. He has, however, only about three million red cells, and 65 per cent. hæmoglobin. After a prolonged search over a full slide one can find an occasional normoblast and megaloblast, with here and there a myelocyte. The interpretation I was inclined to put on that case, in which no albumin was present, is that this was an error in the bone marrow, leading to these peculiar changes in the joint and to the error in the blood, but I should be glad to know whether, in later cases, the blood has been examined.

Suprarenal Sarcoma of the Robert Hutchison Type.

By F. PARKES WEBER, M.D.

THE patient, D. L. S., aged 5 years, a cachectic and emaciated English girl, was admitted to hospital on March 4, 1920. The liver was enormously enlarged, and the eyelids of both eyes were symmetrically swollen and ecchymotic, of a blackish blue or purple colour (*see* fig. 1). The liver, of firm consistence, could be felt occupying most of the front of the abdomen; the right lobe extended down to the pelvis, and the left lobe nearly so; the surface of the organ was smooth and even; the gall-bladder could not be distinguished by palpation. The spleen was likewise not made out. There was slight ascites, enough to give a fluctuation-thrill. There was no enlargement of any superficial lymphatic glands. By Röntgen-ray examination of the thorax (Dr.

James Metcalfe) some hilus lymph-glands were seen. By ophthalmoscopic examination (Dr. C. Markus) the optic disks were found to be pale and to have blurred edges, indicating a condition of post-neuritic atrophy of the optic nerves; there were no retinal hæmorrhages. The child was nearly blind with both eyes. The superficial veins over the front of the abdomen were much distended, and the veins on the forehead were also abnormally noticeable. The whole face had a swollen, "puffy" appearance. There was moderate œdema of the lower part of the trunk and the lower extremities. There was no fever; the pulse was 104 to 140 per minute; respiration 28 to 36. Brachial systolic blood-pressure, 108 mm. Hg. The urine was of specific gravity 1027, acid, loaded with urates, free from sugar, and showing only a slight trace of albumin.

The history obtained was that the child had "influenza" in April, 1919, but that the present illness commenced at the beginning of November, 1919, with various pains. The belly had been enlarged for the last month, and the child had been nearly blind for the last five days. The ecchymotic swollen condition of the eyelids had been present during the last fortnight, but had for a time been previously observed—namely, for six weeks or so about the end of 1919. During the last week there had been occasional vomiting.

The father, aged 28 years, and the mother, aged 29 years, were both of them healthy-looking and intelligent, and in both of them the Wassermann reaction (March 8, 1920) was completely negative. The patient was their second child. The mother had had two other children (both living); no miscarriages.

By the advice of a doctor the patient had been given a teaspoonful of brandy four times daily, and this was continued from the beginning of November till the beginning of December, 1919. Since that time a teaspoonful of brandy had been administered occasionally (not often). This history made one at first think of the possibility of the child having alcoholic "hypertrophic" cirrhosis of the liver. If the brandy was of a bad quality, adulterated with methyl-alcohol, the optic nerve disease might also be accounted for. But it was ascertained that one of the finest obtainable qualities of cognac had been used. The whole illness could, therefore, scarcely be attributed to the brandy.

Chloroma or any form of leukæmia was negatived by examination of the patient's blood; which showed anæmia, but no great excess of white cells: nor was the differential count of white cells in favour of

eukæmia. On March 5 the red cells were counted as 1,700,000, and the white cells as 13,000 to the cubic millimetre of blood. On March 12 the red cells numbered 1,130,000, and the white cells 14,600 per cubic millimetre of blood. The differential count of white cells (March 7), according to Mr. H. McLean, gave the following figures: Small lymphocytes, 24 per cent.; large lymphocytes, 4 per cent.;



FIG. 1.

From a photograph (for which I am indebted to Dr. Fankhauser taken on March 14, 1920, not long before the patient's death, showing the remarkable ecchymotic swelling of the eyelids.

polymorphonuclear leucocytes, 72 per cent.; no eosinophils and no mast-cells were observed during the count of 300 white cells. The red cells varied in size, and there was a little poikilocytosis and polychromasia; no abnormal cells were found.

The existence of some kind of tumour at the back of the eyes

seemed probable, to account for the blindness (post-neuritic optic nerve atrophy) and the ecchymotic swelling of the eyelids. The remarkable ecchymotic discoloration and swelling gradually increased in the hospital and involved not only the eyelids but also some of the forehead and cheeks. The œdema of the lower extremities and trunk became more marked, and the hands likewise became œdematous, before the child's death, on March 15, 1920.

Necropsy and Microscopical Examination.

In the abdomen a tumour, as large as a big orange, but of irregular shape, was found attached to the upper pole of the right kidney, which appeared very anæmic, but otherwise normal. The tumour was curiously lobulated, and in parts was very sanguineous. It showed actively growing nodules, excessively hæmorrhagic portions, and likewise necrotic areas, which last were brownish or nearly white. Microscopically the tumour had the appearance of a round-celled sarcoma. It doubtless originated in the medullary portion of the right suprarenal gland, and remains of suprarenal cortical tissue could be made out by microscopical examination in the layer of compressed capsule-like tissue enclosing the neoplasm.

The left suprarenal gland seemed normal. The left kidney was pale (like the right one), but otherwise normal (confirmed by microscopical examination).

The liver was very greatly and almost symmetrically enlarged, weighing 91 oz. It was of light brown colour on the surface. By cutting into it and by microscopical examination it was found to be infiltrated with pale, not sanguineous, new growth; in some parts there was relatively less new growth than in others. The tumour-cells resembled those of the right suprarenal tumour. They are shown in the accompanying figure (fig. 2), separating the columns of liver cells and apparently situated in the blood-capillaries. Mingled with them are lymphocytes.

The spleen (weight, $2\frac{1}{2}$ oz.), pancreas, heart, and right lung showed nothing special, but old pleuritic adhesion was observed about the left lung. There was no fluid effusion in any of the serous cavities at the post-mortem examination. The ovaries (of the size of olives) were both enlarged with very sanguineous new growth, similar to the right suprarenal tumour (confirmed by microscopical examination of one ovary).

There were abdominal lymphatic glands similarly infiltrated with new growth; but some caseous lymphatic glands at the roots of the lungs and in the mediastinum (one of them was examined microscopically) were doubtless tuberculous.

The orbits and skull were more or less symmetrically involved by metastatic, very sanguineous areas of new growth, the cells of which resembled those of the suprarenal tumour. In the cranial cavity the growths were flat and diffuse, and were situated between the dura mater and the inner table of the skull. Osteophytic spicules pro-



FIG. 2.

Microscopic drawing (magnification, 320) from a section of the liver, to show the diffuse sarcomatous infiltration of the liver. The tumour cells distend the blood-capillaries between the columns of hepatic glandular cells, and mingled with them are lymphocytes.

jected from the inner table into these metastatic deposits. The brain itself seemed normal. The ecchymotic swelling of the eyelids, which constituted such a notable clinical feature of the case, was doubtless due to blood-extravasation connected with the diffuse tumour-infiltration of the periosteum of the orbits, &c. The optic neuritis and blindness may have been partly due to actual compression of the optic nerves at their passage through the optic foramina. The structure of the tumour in

the right suprarenal mass, the abdominal lymphatic glands, the ovaries, the dura mater and cranial bones, was obviously all of the same kind. The diffuse growth in the liver, as I have already stated, differed somewhat from the growths elsewhere, but only inasmuch as it contained relatively little blood.

I have to thank Professor S. G. Shattock for his kind help in the examination of the pathological specimens from the case, and for superintending the drawing from a microscopical section of the liver (fig. 2).

Remarks.

The case is evidently one of primary malignant disease of the medullary portion of the suprarenal gland, in a child, with secondary, multiple, diffuse, more or less symmetrical, metastases in the cranium—of the type of which Robert Hutchison, in 1907, collected ten examples, six of which were as yet at that time unpublished. In 1908 W. Tileston and S. B. Wolbach reported a fresh example of adrenal sarcoma in a child, of the Robert Hutchison type. For certain reasons (though, I think, wrongly) they rejected one of Hutchison's ten cases (that of Ogden and Mathews in 1906), but they added four further cases which they had discovered in their search through the literature. Thus they brought the total number of known cases up to fourteen, including the one described by themselves. They said that the tumour cells resembled the undifferentiated cells of the bone-marrow, the so-called premyelocytes or myeloblasts.

I see no reason for refusing to call the tumours in these cases sarcomata, though apparently R. S. Frew (1911) would call them carcinomata. Bulloch and Sequeira (1905) concluded that cases of primary "blastomata" of the suprarenal glands not associated with precocity in sexual development were usually lympho-sarcomata. That is perhaps practically equivalent to saying that the tumour-cells resembled the undifferentiated cells of the bone-marrow. But I do not think it advisable to call the growths lympho-sarcomata.

Clinically, owing to the involvement of the orbital region of the skull and the presence in some cases of decided exophthalmos, the children may at first sight be regarded as suffering from chloroma or some form of acute or subacute leukæmia, but this error could be avoided in the majority of cases, as it was in the present case, by a careful examination of the blood. Nevertheless, even at the post-mortem examination in the present case, the spicules of bone projecting from

the inner table of the cranium into the deposits of new growth on the dura mater somewhat resembled a condition met with in chloroma, a condition which constituted a striking feature of Firth and Ledingham's case of "atypical chloroma," described about nine years ago. Moreover, microscopic examination of some portions of the liver in the present case might at first sight be supposed to show diffuse leukæmic infiltration of the organ.

Scurvy, with hæmorrhage in the orbits or eyelids, could not even be suggested as a possible explanation of the clinical manifestations in the child under consideration, though in some cases of infantile scurvy proptosis (especially of the left eye), due to orbital hæmorrhage, has been observed.

A remarkable feature of the present case was the enormous nearly uniform enlargement of the whole liver, which during life not only completely masked and concealed the presence of the suprarenal tumour, but also, when considered in association with the history of the medicinal use of brandy, gave rise at one time to the suspicion of alcoholic hypertrophic hepatic cirrhosis. It was suggested that the child's blindness might be due to the brandy having been adulterated with methyl-alcohol. But this suggestion, as I have already stated, was found to be wrong, when the remainder of the brandy which had been used was produced by the parents.

The excessive involvement of the liver in the present instance makes one think also of the cases, recorded by W. Pepper (1901), of sarcoma involving the liver and suprarenal gland in new-born children, but his cases were congenital and come under a separate category.

Perhaps the two most valuable diagnostic signs during life in the Hutchison group of cases are (when present), first the dark ecchymotic swelling of the eyelids on one or (more usually) on both sides, with or without actual exophthalmos, and, secondly, the optic neuritis or post-neuritic atrophy. In the absence of both these clinical signs one ought to hesitate in regard to the diagnosis. These signs were both absent in the boy, aged 2½ years, shown by Dr. E. Cautley on June 20, 1919, and the case turned out to be one of sarcoma not involving either of the suprarenal glands.

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Section for the Study of Disease in Children.

President—Mr. J. P. Lockhart-Mummery, F.R.C.S.

Dislocation of the Thumb in a Child, aged 9 Months.[1]

By B. Whitchurch Howell, F.R.C.S.

A. H., aged 9 months, born naturally, with *no* deformities. Left thumb said to have been injured about six weeks ago; treated by a medical man, who stated the child was suffering from a dislocated thumb, which he reduced. Six days later the trouble recurred, and he was brought to me at the Queen's Hospital for Children, for an opinion on the present deformity. The thumb is held out flat, in abduction, with the terminal joint flexed to 70°. The head of the metacarpal bone can be felt projecting prominently into the palm. The first phalanx is dorsally dislocated, and the second phalanx is flexed to 70°, and cannot be straightened. The tendon of the flexor longus pollicis seems contracted. The child uses the thumb well, but always in the abnormal position.

I regret I have not a skiagram of *both* hands to show, but I think the clinical evidence warrants the diagnosis.

I should be grateful for the opinion of members present, with suggestions as to the line of treatment.

The President: The case is a very interesting one. I agree with the diagnosis, namely, that it is an accidental dislocation. I suggest that Mr. Howell should try to reduce it under an anæsthetic, fix the thumb in an extended position, and retain it there with plaster. In tiny babies it is difficult to get fixation of such a small part as the thumb without producing sloughing of the skin.

P.S.—The deformity was reduced without difficulty under an anæsthetic, and a special malleable iron splint applied.

[1] At a meeting of the Section, held May 28, 1920.

Case for Diagnosis.

By E. A. COCKAYNE, M.D.

A. G., MALE child, aged 9½ years. Admitted to the Hospital for Sick Children, Great Ormond Street, for loss of weight and incon-



FIG. 1.

tinence of urine and fæces of five years' duration. Mother dead; no miscarriages; father and two elder children aged 13½ and 12 years respectively alive and well.

The child had scarlet fever at 14 months; no other illnesses; walked between 18 months and 2 years of age; quite well until five years ago. Since then he has had looseness of the bowels and occasional aching in the legs with progressive loss of flesh. He is a thin undersized child, mentally dull; skin dry and of delicate texture;



FIG. 2.

general loss of subcutaneous fat, including that of mammary and pubic regions; limbs long and slender in proportion to height. Measurements: Symphysis pubis to ground, 23 in.; to vertex, 24½ in.; anterior superior spine to internal malleolus, 24 in.; to internal condyle of femur,

14 in.; maximum circumference of thigh, 9 in.; of calf, 7 in.; length of foot from os calcis to end of hallux, 6½ in.; circumference midway between these points, 5¼ in.; length of arm from acromion process to styloid process of radius, 14¾ in.; of hand from styloid process of ulna to tip of index finger, 4¼ in.; length of middle finger, 2½ in. Thus the hands and feet are very slender, but not unduly long. Limbs straight; no genu valgum; joints normal: some hypotonus at ankles and wrists; marked bilateral flat foot; teeth crowded, but well formed; skull normal in shape, 20¾ in. in circumference. X-rays: Marked thickening of frontal bone; sutures not ossified; bones at base of skull dense; pituitary fossa normal in size; both femora show marked irregular thickening with formation of new dense bone throughout their length. There is a similar condition of osteitis in both humeri, especially in lower part; small local areas of thickening are present in both tibiæ and in one fibula; bones of forearms, hands and feet are normal. Eyes prominent. Mr. McMullen reports that the fundi are normal; no scars round mouth; nose well formed; small shotty glands in neck, axillæ and groins. Heart and lungs normal; pulse 100; temperature normal; abdomen prominent; liver and spleen not palpable; reflexes normal; no incontinence of urine or fæces; urine normal; thyroid gland not enlarged; sugar tolerance high; no glycosuria after 200 grm. of dextrose, but present after 250 grm. Wassermann reaction in blood negative and remaining negative after provocative dose of 0·3 grm. novarsenobenzol. Cerebro-spinal fluid, examined by Dr. Nabarro: Clear; not under pressure; albumin, 0·025 per cent. by Aufrecht; some immediate reduction of Fehling's solution; an occasional cell only seen; Wassermann reaction negative; father's blood, Wassermann reaction negative.

DISCUSSION.

Dr. F. PARKES WEBER: I think it is clear that the diagnosis of congenital syphilitic osteitis will not account for the whole of the features of this rather remarkable case. In addition to the abnormalities in the long bones, the boy has a "lanky" appearance, thin tapering fingers, and long thin extremities, with a looseness about the ankles which has given rise to flat-foot on both sides, a general condition or build which one might associate with disturbance of endocrine glands. For the condition in children which has been termed "arachno-dactylia," or "spider-fingers," a disturbance in the pituitary gland has been suggested as the most likely cause. The fingers in this boy are not sufficiently thin and tapering to justify one in calling it arachno-dactylia, but I regard it as a case belonging to that group. Supposing that the abnormality in the long bones is due to congenital syphilis, can one in any way associate

it with the general condition ætiologically? I think one can, for, in congenital syphilis, it is becoming more generally recognized that the neighbourhood of the pituitary gland may be affected at an early age, and that this may give rise to symptoms of diabetes insipidus, and so forth: and it is justifiable to suspect that there may be congenital syphilis in the present case, in spite of the negative Wassermann report. In what particular way the pituitary gland is affected (if it is affected at all) in cases of arachno-dactylia is at present certainly not known. In the present case, one will perhaps get further knowledge: for instance, when the boy is older it will be more obvious whether the bones are affected with congenital syphilitic osteitis: there may be bowing of the tibiæ finally, giving rise to a more typical picture of what I should prefer to call "congenital syphilitic osteitis deformans"[1]—a condition simulating the clinical picture of osteitis deformans in older patients who are not subjects of syphilis. When this boy grows up one would expect him (on account of his build) to have extreme gastroptosis with general visceroptosis. I suppose Dr. Cockayne will cautiously try antisyphilitic treatment.

The PRESIDENT: It is a very interesting case, and I am inclined to regard it as congenital syphilis, because it always seems to me one is so much more likely to be dealing with an abnormal manifestation of a common disease, than with something which is extraordinarily rare. There are remarkable conditions from quite common causes. I can think of no other disease which would produce the conditions which we have in this case—namely, an obviously chronic inflammatory condition of bone, without any serious pain. Moreover, this condition is so very symmetrical, which is characteristic of congenital syphilis. I regard this as an abnormal case of congenital syphilitic osteitis. I think the additional points mentioned by Dr. Parkes Weber can easily be accounted for by secondary changes. For instance, with such a marked inflammatory condition of the long bones as there is here you would expect increased growth at the epiphyses, even though they might not be diseased at their ends: and that would account for the extra length of the bones in this boy. It would be interesting if Dr. Cockayne would tell us, at a subsequent meeting of the Section, what has been the effect of treatment with antisyphilitic remedies.

Dr. COCKAYNE (in reply): I am very much obliged for the remarks which have been made on the case. With regard to arachno-dactylia, I think there are many things which are against that view. This boy's fingers and toes are not long, there has never been any bending of the spine, and he has not the shape of heel which has been described and figured in connexion with that condition. Before I brought him here I had the idea that there was some endocrine disturbance, but his case does not seem to me to fit into any definite group. The mental dullness, high sugar tolerance, the slender hands and feet, and the small testicles are in favour of hypopitu-

[1] Cf. F. P. Weber, "A Note on Congenital Syphilitic Osteitis Deformans," *Brit. Journ. Child. Dis.*, Lond., 1908, v, p. 83.

itarism, but the child is very thin, and there is no deposit of fat even in the mammary or pubic regions. If it is due to some endocrine disturbance, it is probably due to the same condition as the osteitis. We have been treating him on the lines suggested in the discussion, and I will report the result later on.

Case of Oxycephaly.

By THEODORE THOMPSON, M.D.

THIS extraordinary looking child is about 12 months old. It was a case of breech presentation, and he presents all the features of oxycephaly, or "tower skull." It is a very flat head from before



FIG. 1.

back, and comes to a point on top. He also has marked exophthalmos, and the typical facies. These children generally grow up with good mental capacity, but as the skull closes they generally become blind, apparently on account of increased intracranial pressure. Skiagrams show that there is a deep series of depressions on the inner side of the skull, the result of increased pressure in the brain. This particular case is interesting, because the skull has not yet closed, the fontanelle is still open, and there is a marked bulging. I have brought the case forward because it seems to show that the increased intracranial pressure exists before closure of the fontanelles takes place.

I am not very familiar with the condition. The best paper on the subject is Sir Morley Fletcher's, in the *Quarterly Journal of Medicine*, and Dr. Robert Hutchison has described a case too. This case is one of the youngest I have seen or heard described. Apparently the child is of quite average intelligence for its age: there is no ocular defect, the fundi are normal, and he sees easily. He is the mother's only child.



FIG. 2.

DISCUSSION.

Dr. COCKAYNE: I have, in the other room, a skiagram of a child with a much milder degree of oxycephaly. He was, I think, eight years of age, and at that time he definitely showed the marks which were due to increased intracranial pressure. Yet, in spite of that pressure, the skull bones had closed except for a small area in the occipital region, where he had a small meningocele. He was intelligent, but was nearly blind in consequence of optic atrophy. In this case the question which arises is whether any treatment will succeed in checking the blindness, whether measures for the relief of the intracranial pressure will do good. There is the further question whether the blindness is due to the local pressure on the optic nerves, or to the general increase of pressure. I am afraid the local pressure on the optic nerves is the more probable cause and that the hope of benefit by operation is small.

Dr. F. PARKES WEBER: I think a favourable feature in this case is the wide separation of the bones of the skull. The frontal bones are slightly separated from each other, and there is an enormous anterior fontanelle which

is practically continuous with a very large posterior fontanelle. In that way there is at present an outlet for the pressure. It is, of course, to be hoped that the sutures will take a long time to close. It did momentarily occur to me that this case might be allied to cleido-cranial dysostosis (without the clavicles being affected), but I have given up that idea.

Dr. THEODORE THOMPSON (in reply): I think the other theory in regard to these cases, apart from the theory of intracranial pressure, is that the condition is due to a defect in the growth of the bones of the skull, with a local increase of pressure on the optic nerves. In this case, in which there is the oxycephaly without the closure of skull bones, that seems to be the more likely theory.

Two Cases of Congenital Heart Disease.

By D. H. PATERSON, M.D.

Case I.—A male child, aged 10 months, admitted to the Hospital for Sick Children, Great Ormond Street, with general pulmonary catarrh. He ran a high swinging temperature. No definite signs of consolidation were ever found but broncho-pneumonia was suspected. Contrary to the rule the dyspnœa seemed less in the supine position than when the child was propped up. The pulse was over 160 and often uncountable. There was no definite bruit. Death occurred on the fourth day. Before admission to hospital he had been feverish for a day or two. Blueness had never been noticed but there had been previous attacks of breathlessness. Post mortem the lungs were found to be congested but there was no evidence of pneumonia. The heart weighed 2 oz. 14 dr. Immediately below the aortic cusps there was a hole in the interventricular septum. The aortic, pulmonary and mitral valves were all large and distorted, with fleshy thickening, but the endothelium was glistening and showed no evidence of inflammation, an observation which was corroborated by microscopic examination. Both the left and right ventricles were hypertrophied, the left more so than the right. The ductus arteriosus was closed.

Case II.—A well developed and well nourished male infant, aged 8 weeks, admitted to the Children's Hospital, Great Ormond Street, for cough and dyspnœa of one day's duration. There were moist sounds in the lungs. The dyspnœa was urgent, and it was found necessary to support the chest and to allow the head to fall back, in order to enable the child to breathe. The respirations were sometimes rhythmical.

The child was extremely pale. No cardiac mumurs were heard. Death occurred in a few days, the condition remaining unchanged while in hospital. Post mortem the lungs were found to be merely congested. The heart was transversely placed within the thorax, and weighed 2 oz. 6 dr. The right auricle was distended and thin walled,

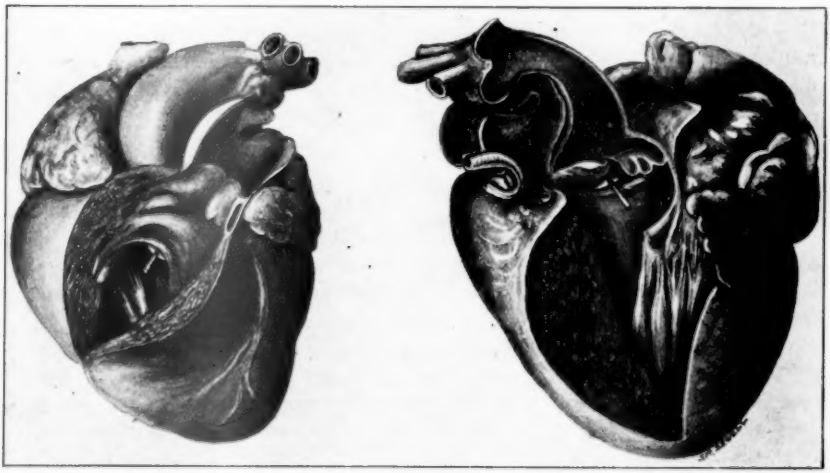


FIG. 1 (Case I).

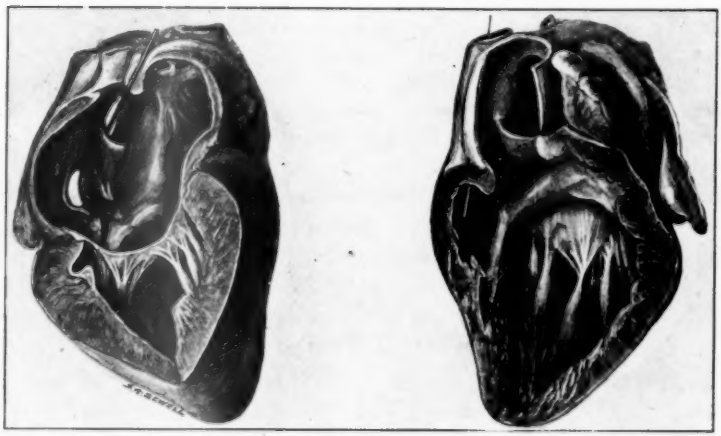


FIG. 2 (Case II).

and the left auricle was small and rudimentary. A patent foramen ovale that admitted the tip of the little finger communicated between the two auricles. A completely stenosed mitral orifice was situated in the floor of the left auricle. The right auriculo-ventricular valve consisted of two cusps only, and the corresponding orifice led into a large thick-walled chamber, from which both the aorta and pulmonary artery proceeded. The valves of these vessels were well formed. Below the origin of the pulmonary artery was a dilated space, the bulbus cordis, which, according to Professor Keith, corresponds developmentally to the third ventricle of the fish. The ductus arteriosus was not patent.

DISCUSSION.

Dr. F. PARKES WEBER: I take it that both these patients died from an infection, and that certainly the first patient, as far as the heart condition was concerned, might otherwise have lived to adult life. In both cases there appear to have been recurrent attacks of dyspnœa, with, I take it, some cyanosis. Certain cases of congenital cardiac malformation have been described by Variot, in Paris, under the term "paroxysmal cyanosis." Some of his cases died, and in some of them very extensive congenital heart disease was found (not only pulmonary stenosis); in one or two of the cases there was, I believe, only one ventricle.

Dr. COCKAYNE: These are both extremely interesting hearts. The whole question of the causation of cyanosis in congenital heart disease seems to me to be unsatisfactorily explained at present. With regard to paroxysmal cyanosis, I remember three cases coming into the Middlesex Hospital, all during one year, and they were published in the *Archives*. They were all in young babies, all being apparently normal, except that they became cyanosed at intervals, and finally died in an attack. All three had a patent ductus arteriosus, but no other lesion.

Section for the Study of Disease in Children.

President—Mr. J. P. Lockhart-Mummery, F.R.C.S.

Testing and Grading of Cardio-respiratory Power among School Children.[1]

By A. A. Mumford, M.D., Manchester (introduced).

Several circumstances are forcing upon us the need for better standards of health than we now possess. Medical inspection of school children as well as medical certificates for industrial employment under the age of 14 years are largely negative and based on the absence of discovery of recognizable defects or diseases.

With the need for higher efficiency and fuller training there has also arisen the need for greater fitness for work and for training. Hitherto we have asked, "Is the child fit to attend school?" and when we have excluded certain physical and mental defects, we could only seek further enlightenment by asking, "Is the child adequately nourished?"

The application of the spirometer and the mercurial gauge to the testing of the physical powers of candidates for the Flying Corps to sustain life and capacity in rarefied high altitudes, has directed attention to the fact that in these instruments we possess the means of measuring vitality and health in terms of the exercise of such important organs as the heart and the lungs. The same applies to the study of the soldier suffering from disordered action of the heart.

In applying these tests to the detection of disease or to the measurement of the vital power in children, certain difficulties occur which may be of interest to others beside school doctors.

[1] At the Provincial Meeting of the Section, held at Manchester, June 18 and 19, 1920.

As regards respiration, we have first to notice that in the adult breathing is a highly complex process in which several factors have been co-ordinated and the unity of which may readily be disintegrated.

The ordinary "resting" breathing of young children is practically entirely abdominal, being constituted by movements of the abdominal walls and spine as well as by movements of the diaphragm. The breathing of effort in childhood is a compound partly of an increase of abdominal breathing and partly of an upper chest movement. This upper chest movement is at first largely confined to the lift of the sternum but with the development of the shoulders and outgrowth of the upper ribs, which occur about the age of 6 years, a lateral expansive movement occurs. Muscular efforts of varying range of amplitude and force produce varying degrees of pressure of air in the lungs. By measuring respiratory pressure, then, we see the influence of age and of vital powers. With children under 8 years old, or with those who are undeveloped owing to the presence of rickets and tuberculosis, it is difficult to use the spirometer. By getting the child to breathe slowly into an ordinary U-tube mercurial manometer, it is rare that we fail to obtain some measure of respiratory power. Two tests should be taken; one of respiratory *range*, by noting the height to which the mercury will ascend, and the other, of respiratory *power*, by measuring the duration of time in seconds in which it can be sustained at a low pressure. The deficient respiratory power that occurs in rickets, as well as in tuberculosis and uncompensated heart disease, is readily revealed. This simple test may also be used to record fatigue, for it will be noticed that there is a great difference in the results obtained in ailing children after active exercise and after rest.

The second respiratory test available for children over 6 years old who are still too immature or too weakly to use the spirometer, is that of the calipers. As the abdominal breathing is the primary one, measurements should be taken from the back at the level of the tenth rib. In delicate children, or in those suffering from nasal obstruction or from root lung disease, there will be no appreciable increase in the lateral diameter. When the increase amounts to two or more centimetres, then whatever the physical signs of the lungs, we have the satisfaction of knowing that active metabolism is going on in the body, which will be a potent factor in deciding the result of the effort made towards recovery. So valuable and constant is this sign of free lateral movement at the level of the tenth rib, that it has been regularly used as a deciding point to determine whether the child

should be allowed to be in the open-air recovery home or attend ordinary school.

Lastly, we come to the use of the spirometer. This is available for children who manifest full control over their voluntary muscles. There are several types of spirometer: the original one invented by John Hutchinson in 1844; the wet revolving drum, known as the "wet gas meter"; the dry air meter, as supplied by Messrs. C. Bouillet (late Verdin) of Paris; and, lastly, the air meter and maximum lung pressure apparatus constructed by Professor Brame for the Air Force and manufactured by Messrs. Baird and Tatlock, Hatton Garden, London. Hutchinson's spirometer and the ordinary gas meter are too bulky for carrying about, but are very reliable. The dry meter requires watching and re-testing from time to time, but it is very handy. The air meter is a portable and combined instrument which registers lung pressure as well as vital capacity. If it is used for young children as well as for adults it will need double calibration, for the amount of air which passes through varies with the pressure. I have found it of great value in measuring the respiratory power of boys entered for athletic contests, &c., but at present not very reliable for testing young children.

The cardiac tests consist in stating the effort of exercise: First, on shortness of breathing; secondly, on increase of pulse-rates; and, thirdly, on length of time taken to return to normal.

If combined with ordinary methods of physical examination, these heart tests are very illustrative and serve to distinguish the functional from the organic and toxæmic heart in carditis. For their bearing on school life, I am indebted to Sir James Mackenzie, who directed my attention to them some ten years ago. I have found them most helpful.

Nasopharyngeal Toxæmia and the Examination of Swabs of the Nasopharynx.

By A. Sellers, M.D. (introduced), and C. P. Lapage, M.D., Manchester.

Dr. C. P. Lapage.

I am strongly of the opinion that nasopharyngeal toxæmia is a very important disease in children and that it causes the following troubles: (*a*) Anæmia and debility, with lassitude and general lowered health;

(*b*) loss of appetite; (*c*) headache; (*d*) frequent colds; (*e*) heart symptoms and signs of which the condition known as "irritable heart," which corresponds to that of "D.A.H." in soldiers, is the most frequent; (*f*) symptoms and signs which are usually put down to rheumatism of which growing pains and tic or habit spasm are the chief; (*g*) nervousness and mental depression.

Since most of these conditions can be produced in the early stages of tuberculosis and since many of them can be produced by bad feeding, loss of sleep, and unhygienic conditions, the question of diagnosis of the cause of the trouble is often a difficult thing. Perhaps in Manchester, where we have a smoky atmosphere, we see an unusually large proportion of cases of nasopharyngeal trouble; but whatever the cause we do see very large numbers of children with chronic nasal catarrh and with accompanying lowered general health and symptoms of toxæmia. Therefore about a year ago I spoke to Dr. Sellers and we arranged that he should examine swabs from the nasopharynx in cases of children who had signs and symptoms of toxæmia and in whom I could find no other cause for these signs than the nasopharyngeal trouble.

Dr. Sellers will explain what was done and what he found and I shall content myself with two brief remarks on the clinical side of the question. One of our results was that the number of organisms was increased in many of the cases and this points to the idea that a vicious cycle may be established by some illness such as influenza which has affected the nasopharynx. The lowered general health allows of more organisms and the larger number of organisms lowers the general health.

With regard to the question of vaccine treatment the results, as Dr. Sellers will explain, point to the conclusion that it is so difficult to isolate definitely from ordinary nasal swabs organisms which give a clear indication for a specific vaccine even when that swab is taken in the laboratory of the pathologist making the examination.

Results of the Bacteriological Examination of Nasopharyngeal Swabs from Sixty-two of the Cases described by Dr. Lapage.

By A. Sellers, M.D.

The richness of the bacterial flora of the nasopharynx has rendered the work difficult. For various reasons it was decided that the work should be done solely on clinical lines and that only the most simple methods should be employed.

The swabs were collected by myself at the laboratory. The form of swab used was an ordinary diphtheria outfit but made with a stem of slightly flexible wire, and armed with a pledget of cotton wool rather smaller than usual. It was passed through the anterior nares, without a guard, after cleansing the nasal orifice with a weak solution of lysol. Plate cultures were made by spreading 8 to 10 drops of a saline emulsion on the surface of one plate by means of a bent glass rod. The plates were incubated overnight at 37° C. and examined with the naked eye and a low power lens. Film preparations were made from the various types of colonies. The stain used for these and also for direct film preparations from the swabs was Gram's followed by weak carbol-fuchsin.

The culture medium used for the plates was defibrinated blood-agar supplemented in about one-third of the cases by slopes of Loeffler's serum or other media. Some doubtful organisms were isolated in pure culture and fermentation or inoculation tests were carried out.

The direct film preparations were found of but little value. They were merely useful in disclosing the presence of blood or pus but afforded no reliable bacteriological information.

The methods used were insufficient for the identification of the organisms, but they enabled them to be classified into certain easily recognized groups. In all 103 swabs were examined. In thirty-seven instances two swabs were taken from the same patient at intervals varying from one to several weeks. In two instances three swabs were taken. The results are summarized in the following table :—

Examination of Swabs.

Number of		Strepto-cocci	Lanc. cocci	Diplococci		Staphylococci		Diph-theroid	Various bacteria
Cases	Swabs			+Gram	-Gram	+Gram	-Gram		
62	103	1	24	1	61	66	2	21	Not classified
37	74	1	16	3	44	49	0	9	Not classified
2	6								

The results do not show that the bacteriology of the cases differs very materially from that of the nasopharynx in a state of health or slight catarrh, judging by the results obtained in examining swabs from a very large number of contacts of cases of cerebrospinal fever during the past five years.

There is no definite evidence that the clinical group of cases described by Dr. Lapage is associated with the presence of any particular organism, but the frequency of the group Gram-negative

diplococci and also of diphtheroids is of some significance. On the whole there is evidence of more copious bacterial flora in the cases under consideration than in the normal or nearly normal nasopharynx, and in the cases in which more than one swab was taken there is some evidence of a reduction in the number of bacteria as a result of treatment.

The meningococcus was not found in any instance, though on several occasions suspicious colonies were present and tested in pure culture. Serological tests were not applied.

Influenza bacilli were found in four cases. In four instances an organism closely resembling a diphtheroid was present but it differed in being constantly Gram-negative.

On testing by inoculation (guinea-pig) it was found to be non-pathogenic. Five organisms belonging to the diphtheroid group were tested by inoculation into guinea-pigs but none of them proved to be pathogenic. No diphtheria bacilli were found. Organisms probably derived from inhaled dust were often present.

On comparing the results of two swab examinations from the same case in twenty-one instances there was found to be a substantial agreement; in sixteen instances there was disagreement.

In conclusion, I think that simple clinical methods are insufficient and cannot be relied upon to give an indication of the bacteriology in any given case. I hope to continue the work, using better methods for the collection of the swabs and taking two or more swabs at intervals, increasing the number of plates, varying the culture media and endeavouring to identify the organism present. With regard to vaccine treatment, the chief point I would insist upon is the inadequacy of simple bacteriological methods and the necessity of repeated examinations before one proceeds to carry out the treatment.

Œsophageal Obstruction in Young Children.

By Hugh T. Ashby, M.D.

Lately I have come across five cases of obstruction of the lower end of the œsophagus, which I think may be of interest in many ways. The condition is generally first noticed at the age of 10 to 12 months, when solid food should be started. At this age solids are vomited, while liquids, as before, are kept down. This vomiting of solid food is

at first thought to be a habit and due to the child taking to solid food badly. After a time the vomiting of solids becomes more marked and the condition is thought more seriously of by the parents and the doctor.

The vomiting of solids is not continuous at first, but later on it may be repeated at short intervals. Liquids are generally kept down well, except in well-established cases. Both sexes seem to be equally affected. In all my cases the site of the stricture is about 1 in. above the stomach.

Diagnosis.

X-rays with bismuth are useful and conclusive, showing how the bismuth collects in the dilated œsophagus above the obstruction. The vomit also does not reach the stomach.

Illustrative Case.

Girl, under observation for three years; aged 4 years at death. The mother states that she had always vomited more or less after solid food and had become worse in this lately (three weeks before death). She had two sisters and one brother. No history of tuberculosis or syphilis in the family; Wassermann reaction negative. First admitted to hospital at the age of 1½ years, when the condition was first diagnosed with the help of X-rays. Treatment consisted of careful feeding and dilatation of the œsophagus at short intervals. The child continued vomiting solids, but was relieved by the passage of bougies at regular intervals until admitted to hospital one and a half years later (April, 1920), where she died. She had remained stationary in weight, with some variations for the last one and a half years.

Post-mortem.—There was no evidence of mediastinal inflammation. The œsophagus was easily removed from the trachea. There was a well-marked thick fibrous stricture in the œsophagus 1 in. in length and about 1 in. above the cardiac end of the stomach. The œsophagus above this stricture was slightly pouched and has thick walls. The lumen of the stricture just allowed a fine probe to pass through. A section of the stricture shows much hypertrophy of a muscle with an excess of fibrous tissue.

Treatment.

Treatment consists in keeping the food as liquid and soft as possible during the time that the spasm is severe and in feeding up well during the quiescent period. The stricture should be dilated up carefully with œsophageal bougies at regular intervals and with this the children generally, and anyhow for a time, keep fairly well. Directly the dilatation is left for more than two to three weeks the vomiting starts again.

The parents get to know this and become very regular in bringing their children to hospital. Often however the stricture becomes so tight that an anæsthetic is needed to pass the bougies but under an anæsthetic the bougies pass easily.

In future, with well-marked cases, I rather think that it would be well to do an early gastrostomy and to feed the child entirely by this way for some weeks so as to give the stricture an entire rest from the irritation of food.

PATHOLOGY.

I think it is likely that the condition is primarily spasmodic in origin and later becomes fibrous as in the case just recorded. The œsophagus is the narrowest and most muscular part of the intestinal tract, with the exception of the pylorus. This being so it is natural to put the condition on a par with hypertrophic stenosis of the pylorus. In favour of this is the fact that the stricture is always in the same place about 1 in. above the stomach. The symptoms resemble hypertrophic stenosis of the pylorus in that food is often kept down for a short time, then later on is regurgitated.

There is no history of swallowing caustics or of injury and it is unlikely that an ulcer could cause the condition and be always in the same part of the œsophagus.

Psycho-therapy in Early Childhood.

By A. DINGWALL FORDYCE, M.D., Edinburgh (introduced).

PSYCHO-THERAPISTS have developed a complicated science of the subject. They often hypnotize themselves by novel and strange terms, and display a tendency to prefer a complicated and elaborate explanation of symptoms and method of treatment to the more obvious. But, as every general practitioner has long known, psycho-therapy is one of the most valuable means of treatment he possesses—the silent power he possesses of developing by various means the patient's cheerfulness, hopefulness, pluck, and faith.

But, it may be suggested, are such effects attainable in "early childhood"? Not only are they attainable, but this is *the* time *par excellence* when they are of the greatest value. Much of the relationship between the doctor and child patient has to do with the borderland

between health and disease; much concerns the general regulation of life and prevention of illness.

Children have natural desires for playing in dirt, mud and water; they are consumed with curiosity, and they develop an interest in questions of sex sooner or later. They show conspicuously the characteristic of negativism. On the other hand, they have to pass through a modern system of education, and at present I do not intend to discuss at all the actual physical conditions as apart from, and as determined by, mental conditions relating to education.

Shortly, I take it that what we contend for in children is, first, health. We wish, in addition, to obtain obedience along with the greatest possible degree of self-development. We wish to develop the power of social pleasure and of business success. We desire that as age advances there will be no undue dependence on parents, but that self-reliance will exist along with a sane individual outlook on the problems of life. On the other hand, we wish to avoid undue haste in development, undue repression of natural childish desires, and consequently undue forcing to uniformity.

The attempt at influence is no easy one, particularly when we remember that we are largely influenced in opinion by our own early training, and also if we bear in mind that for school children, and particularly boarding-school children, the marked and probably beneficial tendency to uniformity at school is with advantage accompanied by encouragement of individualistic characteristics at home.

Further, we have to bear in mind that the more or less normal adult psychic atmosphere of the home may be extremely injurious to the child, and specially the delicate child, and requires conscious control by the parents or guardians (e.g. fear, care).

Psycho-therapy in Health.

At the present time Child Welfare schemes and Child Welfare methods are fashionable. They were much needed and they do much good. But they have developed to such an extent that they now, I think, deepen the darkness in the minds of the public and of many of the medical profession regarding the hygienic care, as a whole, of early childhood. For dirty, starving, ill-used children the first necessities obviously are cleanliness, food and kindness. Given these, and we see wonderful improvement. But such treatment is only half treatment. For the children of the better-off it is fortunately unnecessary. But

for nearly all children direction in psychic guidance is of the greatest importance, and very often is more needed in better-class homes than in the poorer.

Superficial knowledge—pseudo-scientific knowledge—of the dietetic needs of young children is common, the tenets of Freud, Froebel, and Montessori are widely studied, and highly-trained scientific nurses are common as the supreme authority in the nursery. Whether she be mother or nurse, we want in charge of the young child a woman who, in addition to ordinary knowledge of, and capacity for, dealing with children, is also capable of responding beneficially to the individual mentality of the child. She must understand the inherent desires of childhood and the natural tendency to negativism, and she must be capable, without exercising undue influence or admitting undue licence, so to make use of those characteristics as to render them aids in her welfare for the child. For instance, she must avoid laying undue stress upon the necessity for carrying out any ordinary function of the body—eating, sleeping, urination, movement of the bowels. Undue emphasis often arouses the negativistic trait and defeats its own object, when a less obvious insistence results in satisfactory execution.

It is extremely important that children should not develop bad habits, and especially the habit of developing habits. Of course, a weakly physique is the common *fons et origo* of the development of bad habits, but, even so, the physical condition not infrequently is due to ill-informed and improper management in the nursery. The child says he likes this and he dislikes that—it may be food, cot, hour of sleep, &c. Quite often the idea is definitely suggested to him; in other cases it is the natural negativistic reaction to tactless domination.

Likes and dislikes there are bound to be, but they can be immensely diminished in number—particularly dislikes—by suitable care. It should be taken for granted that the child will like what is for his good. This is particularly important in connexion with all that relates to digestion and also to the evacuation of the bowels. From the earliest period care must be taken to avoid elevating these functions to a too superior plane so as to avoid the possible development of difficulties, and thus to prevent the development of the habit of constipation.

Psycho-therapy of Sick Children.

In the case of a sick child such guidance as has been above outlined is of more immediate importance than it is during health. Good

"mothering" consists in the careful application in minute detail of informed knowledge along with such sympathy and wisdom as enables the mother or nurse to utilize the patient's mental processes for his own good. We all know the extreme value of a good nurse. But it is not at all uncommon to find that in cases of sick little children a good nurse is not a successful nurse, or it may be is not a successful nurse in some types of cases. One sees instances of this with the youngest babies, both in acute and chronic disorders, and also with older children. The nurse is unable to exert such an influence over the patients as to help them unconsciously to help themselves.

One of the commonest of the minor ailments of childhood—enuresis—is almost always neurotic in nature, and can be successfully treated by means of a suitable psychic atmosphere. Cases vary, but among better-class children one often finds that undue emphasis has been laid on the dirtiness of the habit, the disgrace, &c., and the child continues the habit from the very fear of repeating it. Make light of the accident, expect better things soon, and the child improves. The same is very often true of constipation. Defæcation is made an important ordeal which has to be undergone daily, with the natural result and consequent indication for treatment. In cases of indigestion, of cyclic vomiting, and, in older children, of migraine, much is gained by suitable "psycho-therapy" associated with other treatment.

But, as an example of how much psycho-therapy is often unavailing unless otherwise suitably supplemented, let us take a case of infantile masturbation. If treatment consists alone in explaining to the parents and nurse that it is only a bad habit which must be prevented and stopped, and that everything depends on *their* watchfulness and care, they may feel and prove unequal to the task, whereas the addition of other simple methods of treatment proves effective. Psycho-therapy is applicable in practically all cases of illness in childhood, and we get good results when it is suitably applied and suitably combined with other methods.

The whole subject is vastly interesting and vastly important, but how little we really know about it. Recently I had under my charge a little girl of 7 years suffering from multiple tuberculosis. She was in a weakly state, but did not appear to be in a serious condition, and certainly did not appear to be in a dangerous one. One day she told the nurse she felt she was going to die soon, and arranged who was to get her various trinkets after death. For a week she remained in apparently certain knowledge of oncoming death, although at times

she was fairly cheerful. At the end of a week she suddenly fell down dead in the ward. At the post-mortem examination no immediate cause whatever for death could be discovered.

Primary Sarcoma of Small Intestine causing Unusual Symptoms.

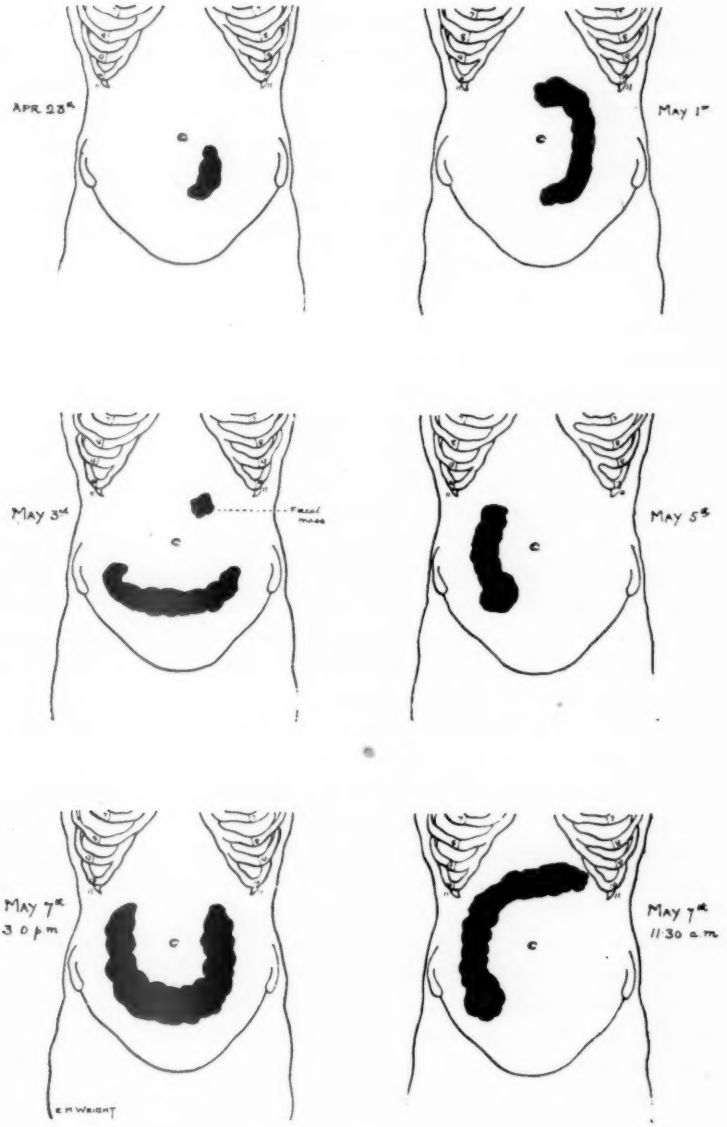
By W. Maxwell Telling, M.D., Leeds.

The specimen shown is that of a small polypoid sarcoma, primary in the small bowel, in a boy aged 3½ years. The clinical history and physical signs were of such interest, and the variety of bowel growth is so unusual, that I think the case is worth putting on record.

The child was sent to me by Dr. Humphrys, of Retford, on May 3 last, with a history of intermittent attacks of abdominal colic since the previous October—about seven months. At first the attacks of colic had come on about every three or four days, and had lasted from half an hour to one hour. The pain was of some severity, causing the child to cry and to be very restless. The attacks increased in frequency and duration, till they were occurring daily and lasting for several hours. He was in an attack when I first saw him and it had then lasted five and a half hours. For about fourteen days preceding this, vomiting had usually occurred with the attacks. The child had always been constipated and this had not been unusually worse since the colic had been present, as it was easily controlled by simple aperients. No blood had ever been noted in the stools. The boy was healthy looking and had not lost much in weight. His doctor had noted a sausage-shaped tumour during some of the attacks; this disappeared in the intervals when he would be perfectly well and cheerful.

On May 3 a considerable sausage-shaped tumour was felt to be extending tranversely across the hypogastrium from one iliac fossa to the other. Its consistence varied with palpation and it was obviously a contracting coil of gut. Handling it did not appear to increase his pain and discomfort, which persisted throughout an attack but was not more than to make him fretful and whimpering. A small nodular mass was felt in the left hypochondriac region and was thought to be fæcal; its disappearance after an enema appeared to confirm this; it was not noted again.

I at once admitted the boy to hospital, where daily enemata for a



Positions assumed by abdominal tumour.

few days seemed to make him more comfortable, the duration and severity of the attacks diminishing, but they increased again before long. During this period of nine days before operation the peristaltic tumour was observed in a variety of positions, and under conditions of varying thickness and size. When present there was never the slightest rigidity, the abdomen being palpable with the greatest ease and the tumour clearly definable and peristaltic. It was sometimes present when the child was not in obvious pain, but when the abdomen was quite normal to palpation, as it not infrequently was, his manner became alert and vivacious; he was then a happy and apparently quite healthy child.

The tumour was 2 to 3 in. in thickness, and from 6 to 15 in. long, usually a little curved, and typically sausaged-shaped. It was visible by inspection. When the tumour was palpated we at once thought the condition was an intussusception; indeed, the child was shown at a meeting of the Leeds and West Riding Medico-Chirurgical Society, and the almost universal diagnosis was chronic intussusception. The diagrams shown record some of the positions in which the tumour actually occurred.

The stools were normal throughout. An X-ray examination carried out by Dr. Scargill proved practically negative. Six hours after a barium meal the barium filled the terminal 9 in. of the ileum and was present in the cæcum, ascending and transverse colon. A small amount of barium was noted in a loop of small intestine just above and to the left of the umbilicus; subsequent findings at operations would lead me to suppose that this was at the site of and had been "held up" by the growth. A barium enema immediately reached the cæcum and gave a perfectly normal presentation of the whole of the large intestines.

I was not satisfied with the favoured diagnosis of chronic intussusception to explain all the observed conditions, regarding it, as well, as inconsistent with the absence of certain signs and symptoms. The tumour was clearly a contracting coil of bowel; from the variability of position it was equally clearly a contracting coil of the small bowel. This was also confirmed by the absolute normality of the large bowel by X-ray examination. From the periodic and complete disappearance of the tumour, if an intussusception, it would obviously have had to be of the "in-and-out" variety and a lesion of this duration transcends my experience. A further difficulty on this hypothesis would seem to be the occasional great length of the tumour (as much as 15 in. being noted). The variability of the site of the tumour pointed to its position

in such a part of the small bowel as was free to move and was therefore not at the beginning or end portions of that tube. It was therefore fairly obvious that this tumour-forming loop of small intestine must *carry its lesion with it*—that is, the lesion must be in the wall or lumen of the gut, and was not a lesion imposed on it from without for that would have been inconsistent with the wide range of mobility observed.

It was also fairly clear that the obstruction was never more than partial, but beyond this point it was difficult to arrive at certainty. From the complete intermission of both tumour formation and symptoms it was difficult to posit a lesion confined to the *wall* of the gut with a hypertrophied coil proximal to it. The stenosis would have had to be considerable, and to produce such a considerable degree of gut contraction behind it, it would surely have been visible by X-ray examination and a cause of notable delay in the downward passage of the meal. This was not the case. I therefore thought the *lumen* was probably encroached upon by a tumour which was capable of varying its position, that it was probably something in the nature of a pedunculated and polypoid growth. One would naturally suppose this to be non-malignant on the grounds of probability. A tuberculous lesion seemed entirely unlikely.

My final diagnosis, therefore, was a tumour, probably polypoid, pedunculated and non-malignant, projecting into the lumen of the small intestine at some distance from its fixed ends, and by some degree of mobility within the lumen causing recurrent partial obstruction, with spasm of the gut immediately proximal to the lesion.

Laparotomy was performed on May 12, by my colleague, Mr. Coupland. A mesial incision was made and the loop of the bowel containing the lesion presented immediately. It was felt to be some kind of stricture and a few small glands were noted on the surface. It was regarded at the moment as tuberculous and enlarged glands in the adjacent mesentery were carefully sought for but not found. Some 3 in. of bowel were excised. Recovery was uneventful.

Later when I saw the specimen I was convinced from external examination that it was not tuberculous, and on laying open the bowel the small polypoid mass was revealed. It was oval, about $\frac{3}{4}$ in. by $1\frac{1}{4}$ in. in diameter, and projecting $\frac{3}{8}$ in. into the lumen; a typical "button" growth and obviously malignant, the small glands on the external surface being infiltrated. Histological examination was made by Professor Stewart, who reported that it was a spindle-celled sarcoma.

In reviewing the case the diagnosis, though not completely accurate, was an approximation to accuracy. The malignant nature of the growth was surprising and remarkable, but not more so than the variable but considerable degree of obstruction which it caused. To explain this I had postulated a growth movable in the lumen; this was not the case, and one can only suppose, therefore, that there occurred a kinking or angulation at the site of the tumour. Otherwise the obstructive phenomena are inexplicable.

The long history of the case and the apparent solitariness of the growth point to a low degree of malignancy. This was further supported by the absence of detectable enlargement of the mesenteric glands. It is rather too much to expect a radical cure in this case, but that time will prove.

Sarcoma of the small intestine is a rare lesion and is usually lympho-sarcoma. Further, its growth is usually rapid, with considerable tumour formation and cachexia; stenosis and obstructive symptoms are very exceptional. Apart from a congenital case I have not found an example recorded under 4 years of age, but I have not exhaustively searched the literature. In all these respects, therefore, this case is very exceptional.

Epileptic Convulsions in Children, with Marked Intestinal Abnormalities.

By J. H. Watson, F.R.C.S. (Burnley).

Epilepsy has been described by medical writers from the earliest times. It is now broadly defined as a progressive disorder characterized by recurrent abrupt attacks of loss of consciousness with or without convulsions and often accompanied by a physical and mental deterioration, but as to its exact ætiology and pathology the medical profession is, and has always been baffled. We talk glibly of the influence of heredity and the sins of the parents, disorders of the endocrine glands, acidosis, increased intracranial pressure, and gastro-intestinal disorders. Many of these conditions are not as yet well understood, but speaking as a surgeon I want more definite proof that the phenomenon is purely an abnormal mental reaction. It is to me, personally, an odious task to inform parents that their child is an epileptic, without offering at the same time some hope of amelioration, if not of cure. For many years I have carefully read any literature

within my reach bearing on this subject from the surgical aspect, and the stimulus aroused by Sir Arbuthnot Lane's writings on gastro-intestinal stasis has directed my attention particularly to notice any possible gastro-intestinal symptoms in these early epileptics, such as evidence of dyspepsia, constipation, or attacks of abdominal pain. So far the radiological findings in children have not been of much help to me. This I confess has been chiefly by reason of their incompleteness, and local difficulties. However, if we are going to make any progress with the treatment of this disorder it must be with the young, for it is only with the young that we can expect favourable results and as a rule parents are willing to accept any form of treatment that will give the slightest hope of betterment. It is not my intention here to offer any theory as to why gastro-intestinal disturbances should cause epileptiform convulsions. Whether it is due purely to a mechanical stasis, the absorption of toxic substances, as Crile's experiments seem to show, the influence of some particular organism, or excessive stimulation of the sympathetic, does not matter, except that good results follow in some instances from rectification of some pathological condition at the ileo-colic angle by one or other surgical procedure.

It has been my lot to witness many and various operations for the relief of epilepsy. At one time efforts were made on the basis of the old reflex hypothesis to rescue these unfortunates. Children were circumcized, women were deprived of their ovaries, a nerve in the hand was resected. When cerebral localization was established cortical excision was practised by Horsley and others but with indifferent and even disastrous results. It is only in recent years that the surgeon has extended his efforts for the relief of epilepsy to the abdominal cavity, in some cases with decided benefit and even cure, as the following cases I quote show. Others I have had at later ages with varying results, but taking them as a whole there has been decided improvement.

Case I.—W. W. I saw this boy in consultation with Dr. Wright in 1911, on account of fits which were becoming more numerous. Thread worms had been seen in the stools and the routine treatment adopted, so that, at the time I saw him, there had been no recent evidence of the presence of the worms. He was carefully watched and dieted and bromides administered, but the fits continued so that two physicians in Liverpool were consulted and the parents were strongly urged to send him to the Institution for Epileptics at Maghull. The parents declined this advice, and I heard of him from time to time, that he was still having fits. Soon after my return from Salonica in 1917, I was called to see him in consultation, with a generalized peritonitis, the history suggesting perforation of the appendix some four days previously.

At the operation the abdomen was found full of pus, the intestines dilated and paretic. A huge retro-cæcal appendix, fixed by adhesions and perforated near the base, was found and removed. The boy made a tardy recovery but during the last three years he has been practically free from attacks.

Case II.—S. M., aged 11 years. Seen in June, 1918. History of fits during last five or six years, constipation and occasional abdominal pains. Operative findings: Cæcum large, appendix swollen at tip, congested and adherent to last few inches of under surface of ileum, marked ilio-pelvic band causing angulation, sigmoid distended and kinked by bands which envelop left ovary. Operative treatment: removal of appendix, division of bands, with careful adjustment of cut serosa, plication of cæcum. This girl has done exceedingly well and has had no fits since the operation.

Case III.—J. C., boy, aged 8 years. Case of Dr. Pozzi. History of epilepsy for two years, a voracious feeder, with constipation and attacks of abdominal pains. A very irritable child, first seen in January, 1920. Operation, March, 1920: Cæcum found mobile and prolapsed in the pelvis: a large retrocæcal appendix, swollen with recent adhesions; marked ilio-pelvic band, hepatic flexure low and free ascending colon (non-fusion). Treatment: appendectomy, plication of cæcum, and fixation to parietal peritoneum; ileum freed. The boy had two slight fits in hospital after operation and others on his return home, but on the whole he is much better than formerly.

Two Cases of Melæna Neonatorum treated by Injection of Fresh Citrated Blood.

By C. P. Lapage, M.D. (Manchester).

In the maternity wards of the St. Mary's Hospital for Women and Children I have to deal with a large number of newborn infants, and amongst these I have had several cases of melæna neonatorum. As you all know, this is a very serious condition, and is one in which treatment is frequently of no avail. Certainly drugs are practically useless except in the very mild cases. The cases I have seen can be divided into those which are slight and those in which there is really serious bleeding, in which the stools are black with blood, and in which the stool is frequently followed by hæmorrhage which may soak the napkin.

After my first experience with some of these cases treated either by drugs or by gelatine injections, I soon came to the conclusion that something more effective was needed. I also tried injections of horse serum without any conclusive effect. One case in which horse serum injections were given apparently recovered, so that at the end of some

four or six days we regarded the baby as cured, but shortly afterwards it had another hæmorrhage and died. This case was such an object lesson that I decided in future to try injections of fresh human serum or blood.

The work which has been done during the war on the transfusion of blood has advanced our knowledge of treatment of hæmorrhagic disorders very greatly. We now know that the injection of fresh blood is a very valuable remedy. Care has to be exercised however with regard to the use of fresh blood, because of the danger of a reaction if the two bloods are not suitable, that is, if the blood of the recipient agglutinates the cells of the donor, or vice versa. The donors and recipients are divided into four groups, and in these the universal donor group comprises 46·2 per cent. It is not my intention to raise this question of the groups here, because in cases of melæna neonatorum it is questionable whether this problem of agglutination is so important as in the treatment of conditions in which large amounts of blood have to be injected. The newborn infant is very small, and probably only 15 to 20 c.c. of blood need be used. According to Lee[1] the grouping is not established at birth, but takes place during the first year of life. I merely mention this question of the grouping to show that I recognize it is important, and consider that if possible it is advisable to have universal donors available in any clinic where these cases may arise.

Description of Cases.

The first case was that of an infant in the hospital, whom I now show to you. The infant was brought blanched, with a severe attack of melæna neonatorum, to my out-patients clinic. I said to my clinical clerks that we should have to give an injection of fresh blood or of fresh serum. One of them offered at once to supply the blood, and the Resident Surgical Officer, Mr. Brentnall, and House Surgeon Miss Boullen, drew off 20 c.c. of blood from his arm, mixing it with 7 gr. of sodium citrate, and they injected it into the external jugular vein of the infant. The next day the melæna had ceased entirely, and there was no recurrence. The infant was less blanched, and made an uninterrupted recovery. The coagulation time of the infant's blood was tested by a method which I had seen described in the *American Journal of Children's Diseases* for April, 1920, xix, p. 269, and was found to be seventeen

[1] *Brit. Med. Journ.*, November 24, 1917, p. 684.

minutes, approximately. Three weeks later it was found to be normal, about nine minutes. The coagulation time of the father and the mother was found to be normal, but they were both tested later on.

The second case was that of a baby to whom I was called out by by Dr. Robinson of Stretford a few days later. This baby was exceptionally big, but he had had very severe melæna, his blood-coagulation time was much delayed, and he was going downhill rapidly. The case was urgent, and it was late at night, so I had no time to select the donor by scientific methods, but I obtained the blood of a friend of the father, and injected 15 c.c. of citrated blood. In this case also the melæna ceased at once, and except for the passage of two more stools, which contained meconium mixed with blood and which had probably been in the intestines from before the time of the injections, the child made an uninterrupted recovery. The coagulation time in the second case was over seventeen minutes at the time of the injection, and remained at seventeen minutes for some days, but about a week afterwards it had come down to approximately ten minutes.

With regard to the technique, the injection of fresh citrated blood into the jugular vein is by no means an easy operation, because the parts are so small, and it is my intention to use in future the superior longitudinal sinus for such injections.

This method of treatment is not new. Koplik reports that he has made use of citrated blood and found it brilliantly effective, one transfusion being all that is necessary. He also says that horse serum and gelatine are not nearly so satisfactory. Other authors give fresh serum subcutaneously in 10 c.c. doses. J. E. Welch[1] recommends injections of normal serum subcutaneously 10 c.c. three times a day. However it is doubtful in my opinion whether the serum injected subcutaneously is likely to prove as effective as intravenous injection of fresh blood from a suitable donor.

With regard to the question as to whether the blood of the mother can be used, it is quite possible, according to Koplik, that this will prove suitable, and in this case there is not likely to be any need for the classification of the donor and recipient, but of course it is not always a feasible plan.

I must apologize for bringing up this subject without more complete elaboration, but my reason for doing so is to bring it before the notice of other workers in the same field at the earliest possible moment.

[1] *Amer. Journ. Med. Sci.*, 1910, cxxxix, p. 800.

Case of Fragilitas Ossium with Typical Blue Sclerotics in a Girl aged 8 years, with Traces of Seven Fractures, one in the Right Arm the others in the Legs.

By CATHERINE CHISHOLM, M.D.

THERE was no otosclerosis, no peculiarity in the shape of the head, no family history, but a history of dyspepsia in the mother during pregnancy. The girl when coming under observation showed physical and mental signs of the thyroid deficiency. Great improvement had followed administration of thyroid, calcium, and cod-liver oil, together with massage and electricity. The girl was now bright and could stand and walk with slight assistance.

Partial Fracture of Radius and Ulna in a Breast-fed Infant, aged 6 weeks.

By CATHERINE CHISHOLM, M.D.

THERE was sudden swelling, tenderness and loss of motion of the right forearm from which recovery took place in less than a week. An X-ray photograph showed partial fracture of both bones. There was no history of trauma. There were no blue sclerotics nor was there a family history of fragility of the bones. The mother was very hardly worked during pregnancy though well fed and in comparatively easy circumstances.

Case of Double Sprengel's Shoulder Deformity.

By CATHERINE CHISHOLM, M.D.

AN X-ray photograph showed a mass of bone on the right side, extending to the occipital bone. Movement of both arms was very good. The infant was eight months old.

Case of So-called Renal Infantilism.

By E. BOSDIN LEECH, M.D.

THE patient, a girl, aged 16 years, has been small from her early days, being smaller at 5 years than a niece of 2 years old. Her legs

were straight till she was aged 13. Since then she has developed acute genu valgum and varum of the two legs. The urine, when she was first seen four months ago, contained much albumin, now it contains a mere trace. The systolic blood-pressure is just below 100 mm. of mercury. Mental development is somewhat backward.

Congenital Absence of Pectoralis Major and Minor; Elevation of the Scapula.

By GEOFFREY JEFFERSON, M.S., F.R.C.S. (Manchester) (introduced).

W. L., AGED 9 years. This child is an example of the relatively common Sprengel's shoulder with associated deformities. In this case these deformities take the shape of absence of the pectoral portion of the pectoralis major, the clavicular head being intact. The pectoralis minor is also absent. The hand on the affected side, the left, is distinctly smaller than the right, and the fingers webbed up to the proximal interphalangeal joints. The elevation of the scapula is of the ordinary type without bony spurs. A radiogram shows that there is no vertebral or other bony abnormality.

Bilateral Baker's Cysts—Recurrence after Operation.

By GEOFFREY JEFFERSON, M.S., F.R.C.S.

A. B., AGED 6 years. Some twelve months ago swellings were noticed behind the knees of this patient; these gradually increased in size until there were two small lumps present, the size of pigeons' eggs. There did not appear to be very much pain, but the child was taken to a hospital. Here an operation was performed on both legs. The child came under my care some two months later with a fully developed recurrence. There are now present two typical Baker's cysts, reducible on pressure when the knees are flexed. There are irregular scars representing the skin incisions over the cysts. The precise nature of the operation performed is not known. Presumably it was not very radical. The special point of interest in the case is that it shows that thorough removal is necessary. There are no signs of disease in either joint.

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Clinical Section.

President—Sir ANTHONY BOWLBY, K.C.B., K.C.M.G., K.C.V.O.

Tumour of the Scalp in an Infant.[1]

By ARTHUR EDMUNDS, F.R.C.S.

E. E., A FEMALE child, aged 9 months, presents a large swelling over the frontal region in the middle line. This is covered with thick normal skin, but although the posterior part of the swelling has invaded



FIG. 1.

Tumour of scalp. Front view.

the hairy scalp no hairs grow on the tumour. It is firm in consistency, does not pulsate nor fluctuate, and only gives the very slightest sign of expansion when the child cries.

[1] At a meeting of the Section, held November 14, 1919.



FIG. 2.

Tumour of scalp. Lateral view.



FIG. 3.



FIG. 4.

Fig. 3.—Tumour of scalp. Skiagram, lateral view.
Fig. 4.—Tumour of scalp. Skiagram, antero-posterior view.

A skiagram shows a deficiency in the bone corresponding to the attachment of the tumour to the skull, and a shell of bone extending up into the substance of the mass itself.

The possibility of the tumour being a teratoma is suggested.

Postscript.—An operation has been performed and a meningocele removed.

Multiple Bone Abscesses of an Unusual Nature.

By Arthur Edmunds, F.R.C.S.

F. B., aged 23. This patient is a well-nourished girl, who gives the history that ten years ago she had a severe illness, which was diagnosed as rheumatic fever. She lay prone for nine months owing to an abscess over the left sacro-iliac joint, which only slowly healed. This abscess did not extrude a sequestrum. During this illness both hips became firmly anchylosed. During the first five years after this she had a series of thirteen abscesses, over both arms. These were not accompanied by any general constitutional disturbance, and only the very slightest local reaction. Nevertheless each one of these extruded a bony sequestrum. There was then a period of complete quiescence for five years, that is to say, up to a few weeks ago, when she had another abscess over the left femur, from which a small, shell-like piece of bone was exfoliated. The scars of the abscesses are depressed, and although not actually adherent to bone, are in close proximity to it. Both hip-joints show bony anchylosis, in spite of which the patient walks about extraordinarily well.

The suggestion is that the case is one of a smouldering bone infection; the patient's resistance to the organism being sufficiently high to obviate any serious illness.

Extensive Rodent Ulcer involving Bone Excision; Plastic Operation; Recovery.

By Arthur Edmunds, F.R.C.S.

The patient is a married woman, aged 67, who for four years had suffered from a gradually increasing ulceration above the left eye. This commenced as a "wart," which was a little painful and bled occasionally. After tolerating this for two years, she applied to a

medical man, who gave her "some ointment," and then after a lapse of another two years she came to the Great Northern Hospital for treatment. The sight of the eye which had begun to fail three months before, had by this time been completely lost for a month.

The disease extended beyond the middle line on the inner side and well into the temporal region on the outer side; it involved the whole of the upper and most of the lower eyelid. The cornea was perforated, and the eye completely disorganized and suppurating. The centre of the growth over the external angular process of the frontal bone, was adherent to bone, definitely involving it for an area about an inch in diameter (fig. 1). There was considerable secondary septic infection, and the patient was suffering extreme pain; she had lost weight for twelve months, but her general health was reasonably good, and her vital organs were sound.

Operation: August 13, 1919: Intratracheal anæsthesia. An incision was carried around the whole of the affected area, including both eyelids. The area of skin thus marked out was then dissected up until the neighbourhood of the affected bone was reached. When this was done a convenient part of the frontal bone had been exposed from which a $\frac{3}{4}$-in. trephine crown was cut. The upper margin of the orbit was then cut free with forceps, starting from the trephine opening. The zygoma and the body of the malar bone at the lower and outer angle of the orbit were divided with a saw and the whole mass thus liberated was loosened. This involved a fracture of the thin outer wall of the orbit and the bone at the inner canthus, which was accomplished without any difficulty, and by deepening the dissection, the eye was enucleated together with the conjunctival sac. A ragged wound was thus formed in which were exposed the cut edges of the malar bone and zygoma, the tissues in the apex of the orbit, and an area of dura mater about 2 in. wide by 1 in. high. Toward the inner side, the detachment of the mass had led to the opening on the frontal sinus. To cover in this, a flap was turned down from the scalp. For this purpose an incision was carried upwards over the top of the head from the inner angle of the wound, extending somewhat to the opposite side to provide the necessary fullness. This incision was carried down to the epicranial aponeurosis and the scalp thus marked out dissected up, turned down and sutured into position. In this way the whole of the dura and the orbit was satisfactorily covered in, and all that was left to heal was a small area on the face below the zygoma and a long area on the vault where the flap had been dissected up. The face

healed without trouble, but the deficiency on the vault was too large to cicatrize, and accordingly when the wound was sufficiently healthy, that is to say, on October 20, 1919, this was covered with Thiersch grafts, which took well, and the patient left the hospital on November 23, 1919, completely healed, in excellent health and free from pain (fig. 2).



FIG. 1.
The ulcer on admission.



FIG. 2.
The patient on conclusion of treatment.

The line of the hairy scalp is a little lower than normal on the affected side, and there is of course a marked deformity where the orbit and the surrounding structures have been replaced by a smooth area of skin, but an ordinary eye-shade conceals this quite satisfactorily.

Femoral Aneurism of (?) Rheumatic Origin.

By PHILIP HAMILL, M.D.

G. C., AGED 22, had rheumatic fever six years ago. He enlisted in 1915, was classed A1 and remained in good health until March, 1918, when in a runaway accident he was much shaken but received no blow nor injury. Next day he complained of pain under the heart and was admitted to hospital, where he remained six months. He was discharged from the Army in October, 1918. In November, 1918, he was seized with sudden pain in the left groin and was unable to walk. In December, 1918, he was admitted to the London Hospital where the aneurysm was treated by ligature of the superficial femoral artery with removal of the clot. At that time he had enlargement of the heart with signs of aortic and mitral disease. The Wassermann reaction was negative.

At the present time he still suffers from shortness of breath, there is some difficulty in walking and the knee cannot be completely extended. The heart is much dilated with aortic and mitral lesions, the pulse-rate is raised, the liver and spleen are slightly enlarged; there is no albuminuria nor casts. There is good pulsation in the femoral artery below the point of ligature. Blood count: Red blood corpuscles, 4·6 millions; white blood corpuscles, 3,700; Hb. 55 per cent. Blood culture and Wassermann reaction negative.

A Case of Bilateral Cervical Ribs with the Special Type of Muscular Atrophy limited to part only of the Thenar Eminences.

By S. A. KINNIER WILSON, M.D.

THE patient is a woman of 58, who has noticed the wasting of the thumb muscles for some eight or nine years. For some six years previously she has complained of numbness in the fingers. The condition has not progressed in any way whatever since 1911, when the patient first came under observation.

Radiographic examination shows bilateral cervical ribs. The muscular atrophy is not global in the thenar eminences, as in progressive muscular atrophy, but is strictly confined to the muscles innervated by the median—or rather, since the other median-supplied muscles are normal—to muscles of the thenar group innervated, via the median, by filaments derived from the seventh cervical segment.

A Case of Dislocation between the Fifth and Sixth Cervical Vertebræ.

By S. A. Kinnier Wilson, M.D.

The patient is a young woman who sustained an accident about one year ago, being knocked down by a taxi-cab on November 17, 1918. At first there were marked weakness and pains in the distribution of the



Dislocation between the fifth and sixth cervical vertebræ.

seventh and eighth cervical segments mainly, and fixity of the neck. Steady improvement, however, has resulted in the disappearance of nearly all the objective signs; the patient can move her neck freely; there is no muscular wasting to speak of, and little objective sensory change. Occasional radiating pains still occur.

A Case of Trophœdema.

By S. A. Kinnier Wilson, M.D.

The patient is a woman aged 48, who was operated on in the Middlesex Hospital in 1908 (both breasts removed; interval between; precise nature of condition not certain; supposed malignant, then stated to be chronic inflammatory (?)). From about 1911, she has complained of increasing swelling of both arms and of the legs, especially the right, till they have now become enormously swollen. The enlargement has never gone down. There is little if any general disturbance of health, and but trifling pain or discomfort. No evidence of disease in cardiac, pulmonary, renal, or blood systems. No evidence of venous thrombosis or lymphatic obstruction. No definite evidence of myxœdema. The segmental character of the œdema is striking.

Dr. F. Parkes Weber: I agree that the case is one of "trophœdema," although a very unusual one. The familial cases (so-called "Milroy's disease") are in my opinion far rarer than the cases in which there is no family history of the disease, just as familial examples of neuro-fibromatosis (Recklinghausen's disease) are rarer than non-familial examples.

A Case of Extreme Œsophagectasia (Hurst's Achalasia of the Cardia).

By H. Batty Shaw, M.D.

Mrs. G., a widow, aged 66, presented herself for treatment at the Brompton Hospital for Consumption and Diseases of the Chest, because she had become so wasted and had a cough. She had been treated at various times for consumption, for bronchitis and for dyspepsia and debility. Her dyspepsia took the form of pain, occurring shortly after taking solid food. The act of vomiting (?) relieved the pain. For years she had lived on liquid food which caused her no pain nor vomiting.

Her sputum, such as there was, was free from tubercle bacilli. She volunteered the strange statement, that as a girl of 15 she could remember members of her family calling attention to the fact that she was the last to leave the table at meal times, and her explanation is that she never could take her meals in the same period of time required by the rest of the family: she always had to eat slowly, and wait till the food "passed on." So curious was this history that pointed questions were put to her, which led to the fact being established that the taking of solid food was followed by the following symptoms:—

(1) It caused pain at the epigastrium.

(2) This pain was accompanied by what she called vomiting, but what was really regurgitation of food.

(3) The act of taking the solid food soon provoked a cough.

(4) It sometimes happened that she became breathless, and this was particularly so after she attempted to take more solid food than usual.

(5) Liquid food caused none of these symptoms.

She was radiographically examined by Dr. Stanley Melville, with the result that diagnosis of an extremely marked œsophagectasia was established. So condensed was the wall of the dilated œsophagus that the picture was very well shown without the use of a bismuth meal. On the administration of the bismuth meal a sharply defined pencil-like portion of it was shown by Dr. Stanley Melville to reach from the point of contraction at the lower end of the œsophagus into the stomach. This phenomenon has already been shown by Dr. Stanley Melville in some of the cases published in the *Lancet* of December 2, 1916, p. 936, and he further showed that the act of deep inspiration intensified the sharpness of this pencil-like appearance, or led to its development, if absent in quiet breathing, and that this periodic manifestation was dependent upon the compression of the large dilated œsophagus by the expanding lungs. The clinical recognition of the condition of œsophagectasia, largely due to Dr. F. Parkes Weber and Dr. Arthur F. Hurst, is now comparatively easy, and radiography has completed the ease of the diagnosis.

The case shows an extremely marked dilatation, and further, it demonstrates that it is possible when the above five clinical manifestations can be defined, to diagnose almost with certainty a condition which in the absence of such symptoms would be missed by a physician, and would only be discovered by a radiographer, who in the routine

examination of cases in which bismuth meals were being investigated, paid attention, not only to the behaviour of such meals as they passed through the stomach and intestines, but also through the œsophagus.

Three Cases of Malignant Disease of the Face—one of the Lower Lip, two of the Nostril—illustrating the Modern Methods of Radical Operation; Survival after Five Years.

By Henry Curtis, F.R.C.S.

WITH PATHOLOGICAL REPORTS BY T. W. P. LAWRENCE, M.B., F.R.C.S.

The three cases of malignant disease of the face which I desire to record may be summarized as follows :—

Case I.—A man, aged 53, with extensive squamous epithelioma on the left half of the lower lip, encroaching on the angle of the mouth. Operation, June 9, 1914, at the Metropolitan Hospital, Kingsland Road, E. 8. Patient exhibited, November 14, 1919.

Case II.—A man, aged 43, with squamous epithelioma on the external aspect of the left ala nasi, with no evidence, clinical or histological, of its having originated as a rodent ulcer. Operation, December 20, 1913, at the Dreadnought Hospital, Greenwich. Alive and well, November, 1919.

Case III.—A lady, aged 57, with squamous epithelioma on the external aspect of the right ala nasi, also with no evidence, clinical or histological, of its being derived from any previous rodent ulcer. Operation in private, August 7, 1914. Alive and well, November, 1919.

According to the authorities, it may be mentioned that primary squamous epithelioma of the ala nasi is rare; so that to meet with two cases within eight months or so must be an unusual experience.

Since the radical cure of cancer involves removal of the lymphatic areas which are, or may be, infected, it may be as well here to refer to the

Lymphatics of the Face.

Briefly, we may say that, for the most part, the skin and mucous membrane of the nose and cheeks, as well as the eyelids and conjunctivæ are drained by the facial glands, and though the parotid lymphatic glands also drain the eyelids and, to some extent, the root of the nose,

unless these are definitely enlarged, the parotid lymphatics may be left untouched in most cases of growths of the cheek and nose. The facial glands are arranged into the following groups :—

(1) *Infra-orbital*, or *maxillary*, draining the area between the nose and cheek, down to the zygomatic arch. There is also a small *naso-genial gland* in the groove between the ala nasi and the cheek.

(2) *Buccinator*, on the buccinator muscle, opposite the angle of the mouth.

(3) *Supra-mandibular* or *supra-maxillary*, on the outer surface of the mandible, in front of the masseter.

The efferents from the facial glands pass to the submaxillary lymphatic glands. Below the chin, on either side of the median line, are the *submental glands*, draining the central portions of the lower lip, the floor of the mouth, and the apex of the tongue. The efferents from the submental and the *sublingual glands* pass to the submaxillary lymphatics, the sublingual also partly passing to the *deep cervical glands* on the internal jugular vein. The efferents from the submaxillary, like those from the parotid lymphatic glands, pass to the superior deep cervical glands.

As to the *microscopic appearances presented by the lymphatic glands removed*, considerable experience of the normal and morbid histology of lymphatic glands is necessary to determine whether or not, especially in early cases, they are infected with cancer cells. While we all subscribe to the doctrine that enlarged glands associated with a growth are not necessarily malignant, it is somewhat remarkable that this should actually be so in two out of the three patients now reported upon (Cases I and III). Apart from definite tubercle, such enlargement of the glands may be due to irritation and cloudy degeneration, rendering more evident than usual the normal endothelial cells lining the lymph spaces, and covering the fibrils of the reticulum; in other instances (Case I), the hypertrophy of the gland is associated with the presence of "germ centres," which Mr. Lawrence defines as "the pale areas seen in the midst of the normal lymphatic tissue, composed of clearer and larger cells, resembling endothelioid cells." There is, as yet, no reason to regard these as a pre-cancerous stage of disease.

I have therefore been so fortunate as to obtain a detailed report on the sections from all the cases here reported from Mr. T. W. P. Lawrence, M.B., F.R.C.S., Pathologist to University College Hospital.

The operations now recorded were devised in general conformity with the principles laid down by Sir Lenthal Cheatle, in planning incisions

for the removal of a cancer from the front of the face, so as to excise the growth with, wherever possible, at least ¾ in. of skin around, and taking the entire thickness of the cheek, "everything down to the bone," together with all the lymphatics and glands likely to be involved, *en bloc*. A formal block dissection of this kind, performed, necessarily, at one sitting, is perhaps best reserved, in practice, for growths of the lower lip, or in its vicinity, though it was carried out also in the earlier of the two cases of cancer of the nostril (Case II).

Though radical cure is the chief thing to be aimed at, the cosmetic effect needs also to be borne in mind, the risk of septic infection, and as a result, disfiguring scarring in the neck being considerable after such an extensive operation as was done in Case II. Experience seems to show that the alternative risk of leaving cancer cells behind, in the interval between the area left after freely excising the growth and a separate incision in the neck for the removal of the glands there, does not seem great if the less extensive operation, done in the second case of cancer of the nostril (Case III) be selected.

Case I.—Cancer of the Lower Lip, encroaching on the Angle of the Mouth.

Before describing the modern operation in detail, some preliminary remarks are desirable. Until recently, cancer of the lower lip was almost invariably removed by means of a V-shaped incision—but the frequency of recurrence is shown by Mr. Rowntree's statistics, collected from the Middlesex Hospital records. Of 126 cases of cancer of the lower lip, the majority of which had been removed by the simple V-shaped incision, there were seventy cases of recurrence, in forty of which the growth had recurred in the lip—i.e., 31¾ per cent. of local recurrence. The reason will be obvious when one realizes that while the lymphatics leading from the growth, and carrying infection with it, *diverge* as they pass to the corresponding lymphatic glands, the lines of the V-incision *converge*, and thus cut across these channels, which may be infected, thus leaving the cancer cells behind. This explains the great frequency of local recurrence. Sir Lenthal Cheatle was therefore more than justified in condemning altogether the use of V-shaped incisions in this region, since the importance of really removing the cancer and all its evil works is the object desired, a picturesque appearance being of less importance. The final result of the modern operation is not so great a deformity as might at first sight be imagined as will be seen from the accompanying photograph of Case I (fig. 3).

Cancer of the lower lip usually spreads along the margin, but more rarely it spreads downwards to a greater degree than usual before it eventually curls round the angle of the mouth to reach the upper lip. Hence in early cases it is impossible to say in which of the two directions the cancer is mainly spreading, and it is therefore advisable that the incisions be planned so as to include both paths of spread. This was specially indicated in the case now recorded, the angle of the mouth being encroached upon. Where there is less reason to fear such involvement, a less extensive operation, arranged on the same general principle of divergent incisions, is indicated.

Case I.—W. P., a labouring man, aged 53, was admitted to the Metropolitan Hospital, Kingsland Road, E. 8, under the writer's care, on June 6, 1914, suffering from an extensive typical malignant growth involving the outer half of the left side of the lower lip, and spreading subcutaneously towards the adjacent angle of the mouth, with enlargement of the left sub-maxillary gland, removal of which subsequently showed the associated lymphatic glands to be also distinctly enlarged. The encroachment of the lower lip growth on the angle of the mouth necessitated the wide removal shown in the shaded area of the diagram (fig. 1), so that the limiting internal and external incisions, AB, CDE, and FG, should be a good $\frac{3}{4}$ in. external to the edges of the growth, and so as to deal with the entire lymphatic area likely to be involved. This area included the left nasogenial gland, left buccinator, supra- and inframaxillary, right and left submental and the left cervical groups of glands, and, as will be seen from the diagram, it could be effectively dealt with through the incisions indicated.

Operation, June 9, 1914: *The inner incision*, in its lower segment, AB, divided the lower lip and chin, beginning at the free margin of the lip, just to the left of the middle line, so as to be at least $\frac{3}{4}$ in. internal to the edge of the growth, and ending below so as to incline inwards to the point of the chin, to allow for the radiating spread of the lymph channels. In the upper segment of the inner incision, the upper lip was similarly split about midway between the middle line and the angle of the mouth, this incision, CD, being then carried round the left ala nasi, DE, to expose any enlarged lymphatic gland in the nasogenial groove. *External incision*: Passing outwards from the upper end of the nasogenial groove, in a wide semicircle, the apex of its convexity, F, being directed outwards and upwards to reach the anterior border of the masseter muscle, the external incision descended in a sinuous curve, FG, so as to reach the lower border of the inferior maxilla, at G, in a slightly diverging direction. These incisions included everything to the bone. The terminations, B and G, of the inner and outer incisions AB, FG, were then joined by two converging cuts, BH, GH, going through skin and superficial structures only, to meet at a point, H, midway between the lower border of the jaw and the hyoid bone, from which point a curved incision, HK, was carried outwards

and downwards to the middle of the anterior border of the corresponding sterno-mastoid muscle. After carefully dissecting up the entire thickness of the lips, cheek, and chin, included in the area outlined above the lower margin of the jaw, the flap was wrapped in gauze soaked in 1 in 20 carbolic lotion, to prevent possible infection of the remaining tissues with cancer cells, as Cheatle recommends. The triangular area of skin, BHG, outlined below the jaw remained, however, still attached to the underlying fascia, platysma, and lymphatic glands. Both outer edges, BH and GH, of this area were then very



FIG. 1.

Diagram of operation in Case I; for details see text.
(All the diagrams and drawings of parts removed are by W. Thornton Shiells.)

freely undercut, and on the median side both right and left submental glands were isolated, while from the outer edge the left submaxillary gland and its lymphatics were similarly dissected free from the deep structures whilst remaining attached to the triangular area of skin, BHG, below the jaw. The entire area outlined, the growth and the glands, with intervening lymphatics, was then removed, no enlarged cervical gland being met with (fig. 2). The free undercutting of the tissues in the submaxillary and submental regions greatly facilitated closure of the gap in the cheek and lips, the lower lip being chiefly

formed from the tissues below the jaw, composed of the flap GHK. The cut edge of the left cheek was carefully stitched to the side of the nose and upper lip, but as such apposition tended to raise an oblique pucker, a V-shaped piece of skin, indicated, diagrammatically only, by the dotted line FMG, needed to be excised. A drainage-tube was inserted at the lower angle of the wound. The stitches were removed on June 21, and the patient was discharged on July 16, 1914.



FIG. 2.

The parts removed from Case I.

The photograph, taken May, 1917 (fig. 3), shows very satisfactory coaptation and relatively little disfigurement, considering the radical nature of the operation involved; and you have had an opportunity of examining the patient himself to-day.

Pathological Report by Mr. Lawrence on Case I.

"The primary growth is a mass of squamous carcinoma: detailed description superfluous. The left submaxillary lymphatic gland shows marked signs of activity in the direction of hypertrophy, and the

"germ centres," so-called, are particularly large and distinct. There is considerable fibrous overgrowth in places, especially about the hilum. There is no evidence of carcinoma in the section."

Sir Lenthal Cheatle has pointed out that when cancer begins in the upper lip, the columella is more invaded than the ala nasi. The septal origin of the orbicularis oris forms an easy pathway by which the disease reaches the columella.

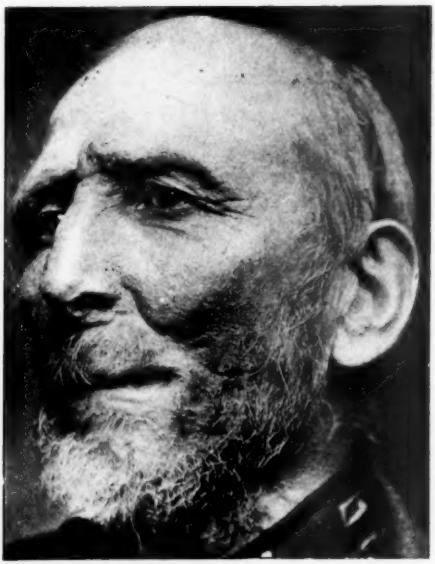


FIG. 3.

Photograph of Case I, May, 1917.

In the first of the two cases about to be described, the growth arising in the ala nasi appears to have spread thence towards the tip of the nose, and had it not been operated upon, the columella would probably soon have been invaded. A more radical operation than has hitherto been generally advocated is therefore necessary for growths of the upper lip and ala nasi.

CASE II.—CANCER OF THE LEFT NOSTRIL, SPREADING SO AS TO INVOLVE THE TIP OF THE NOSE.

This case of a man, aged 43, was fully reported at a meeting of the Clinical Section, on March 13, 1914,[1] some twelve weeks after the primary operation; not to claim it as a cure, needless to say, but to draw attention to the considerations upon which the modern methods of operating on malignant growths of the face are based. It is unnecessary further to describe the operative details of this case, which will be found in the volume of the *Proceedings* above quoted. It was a formal "block" operation, strictly comparable to that just described for the lower lip, and I have already alluded to the objection to which it is open.

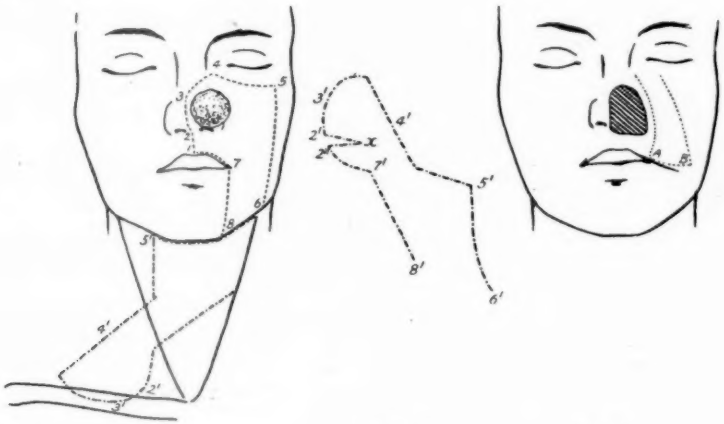


FIG. 4. FIG. 5. FIG. 6.

Figs. 4, 5, 6 and 7, relating to Case 2, are reproduced from the original account.

Fig. 4.—Primary squamous epithelioma of the left nostril. First operation. The gap in the cheek—6, 5, 4, 3-8—was filled up by a flap from the neck—6, 8, 5′, 4′, 3′, 2′—having its apex at the opposite (right) clavicle.

Fig. 5.—Flap rotated into position and slit up at 2′ *x* 2″ to form flaps for nostril and upper lip.

Fig. 6.—Second operation. The new flap, with apex AB, at level of exuberant fold at left angle of mouth (which was thereby obliterated), used to close the gap and form the new left nostril.

[1] *Proc. Roy. Soc. Med.*, 1914, vii (Clin. Sect.), pp. 109-113.

Report by Mr. Lawrence on Case II.

This should be substituted for that appearing in the original account given of this case in the *Proceedings* for 1914. He says:—

"The primary growth is a mass of squamous epithelioma: detailed description superfluous."

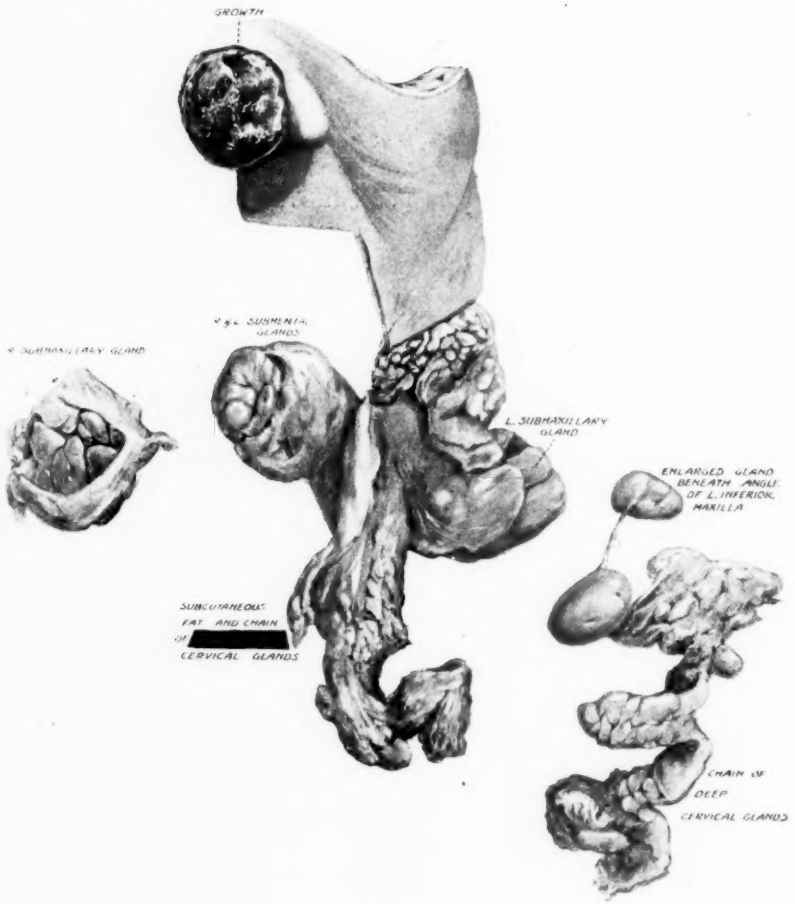


FIG. 7.
The parts removed from Case II.

[It had been examined earlier by an expert pathologist, who reported it to be a typical squamous epithelioma not arising in connexion with any previous rodent ulcer.]

"Left buccinator region: The section through this region shows areas of voluntary muscle, fat, vascularized fibrous tracts and nerves. Some of the muscle fibres have undergone hyaline degeneration. At one spot is a small collection of lymphocytes lying in a very delicate reticulum, probably a developing lymphatic gland. In addition there are several hæmorrhagic areas, and at one spot a group of blood-vessels, seen in transverse section, the sheaths of which are infiltrated with carcinoma cells.

"Left masseteric region: This shows the same structures as the last, except for the malignant changes mentioned.

"Left submaxillary lymphatic gland: The enlargement is mainly due to hypertrophy of the lymphoid tissue. The capsule is somewhat thickened and hyaline, and the connective tissue is increased in places in the substance of the gland. Several of the fibrous tracts in the latter situation are infiltrated with cells of sharply defined outline and well-staining protoplasm: these I take to be carcinoma cells.

"Left lowest deep cervical gland: Beyond a slight tendency to hypertrophy of the lymphatic tissue, and slight proliferation of the endothelial elements, the gland shows no pathological changes."

The definite involvement of the lymphatics, revealed by Mr. Lawrence's report, confirmed the necessity for removal of the entire thickness of the cheek and the involved lymphatic system by a block dissection. In my original account I remarked that "recurrence is highly probable, even now; it must have been inevitable had anything less radical been attempted." Happily, so far, more than five and a half years after the operation, the patient is able to report himself as alive and well, following his old occupation as a waterman at Greenwich.

Case III.—Cancer of the Right Nostril.

Case III.—Mrs. S., aged 57, seen in consultation with A. E. Bullmore, F.R.C.S., for an ulcerated growth on the external aspect of the right ala nasi, an excised portion of which proved on examination by the Department of Clinical Pathology, University Hospital, London, to be a primary squamous epithelioma, with no evidence, clinical or histological, of having been derived from any previous rodent ulcer. The right submaxillary gland was a little enlarged, and a gland could be made out in the region of the bifurcation of the right common carotid artery. The modern operation involved removal *en masse* of the primary growth with an area of skin and all the soft tissues, right down to the bone, for at least $\frac{3}{4}$ in. around its ulcerated edge, and the lymphatic gland in the nasogenial groove, and the buccinator group of glands; the supra- and

inframaxillary and cervical glands being removed through a separate incision below the jaw. The area excised is indicated by the shaded area, ACEF, in the diagram (fig. 8), limited by internal and external incisions, ABC, FGE, which were joined at their upper extremities by a nearly horizontal cut, CDE.

Operation, August 7, 1914.—*Internal incision*: The upper lip was split in the middle line, AB, and the incision BC, carried through the columella, and then slightly to the right of the tip of the nose to just above the level of the junction of the ala with the cheek and side of the nose. The *external incision*,



FIG. 8.

Diagram of operation in Case III. For details, see text.

FE, from the right angle of the mouth directed upwards to the same level as the internal incision, was carried through the lip and cheek down to the bone upwards and outwards in the direction of the external canthus of the eye, so as to include the zygomaticus minor muscle. The upper ends of these two incisions were joined by a third cut, CDE, passing outwards and slightly upwards—through the nose and entire thickness of the cheek to the bone. To prevent displacement of the remaining nostril, a crescentic area, M, around

the left ala was excised and the left half of the lip completely detached from the nose and jaw, and the adjacent left cheek thoroughly undercut, so that this half of the lip could be brought across to fill in the gap by suturing to the remaining portion of the right half of the cheek. To facilitate the closure of the gap in lip and face, three parallel incisions were made in this portion of the cheek down to the bone, outward and slightly downward from the outer cut edge, the middle one, GH, about $1\frac{1}{2}$ in. long, commencing at the level of the junction of the ala with the upper lip, running outwards and downwards to the centre of the buccinator muscle, avoiding the line of Stenson's duct and the danger of salivary fistula by first observing the position of the duct orifice inside the mouth; the lowest incision passed outwards from the angle of the mouth, and the uppermost from the upper end of the gap. Reflecting these flaps allowed of exposure of any enlarged buccinator glands, but none were met with. Of the two flaps thus marked out, the lower one, HGFK, was stitched to the freed left half of the upper lip, and the upper, LEGH, attached above it, leaving only the gap caused by the removal of the right ala, to be covered by a subsequent plastic operation, experience having proved this to be safer than any attempt to do so at the primary operation. The edges of the crescentic gap, M, were then sutured, and the upper edge of the left half of the lip attached to the columella, &c. The enlarged right submaxillary salivary and lymphatic glands, and the large deep cervical gland at the bifurcation of the right common carotid artery, with small chain of glands above it, shown in the sketch of the parts removed (fig. 9), were then excised, through the curved incision PQ, passing down and out from below the symphysis menti to the middle of the anterior border of the right sterno-mastoid muscle, which was defined by the incision OR, extending downwards from the level of the angle of the jaw. The region of the supramaxillary glands, exposed by turning up over the lower jaw the flap PQO, was explored, but no enlarged lymphatics were discovered. Convalescence was rapid, and the patient remains alive and well, with no sign of recurrence.

Examination of the sections of the lymphatic glands was negative as to malignant infection, but the detailed report, illustrating some of the difficulties in diagnosis, previously alluded to, is worth putting on record.

Pathological Report by Mr. Lawrence on the Lymphatic Glands removed in Case III.

"The right submaxillary lymphatic gland is horse-shoe shaped in section, with central area of fat. It shows signs of hypertrophy, especially in the peripheral lymph follicles. There is considerable proliferation and swelling of the endothelium of the lymph sinuses, principally on the concave side of the gland. I see no evidence of carcinoma.

"Right cervical gland: In parts there is considerable connective tissue overgrowth, more especially at the hilum and around the vessels. The capsule of the gland and a narrow zone of the lymphoid tissue subjacent to it have undergone hyaline degeneration in places. The endothelial elements of the gland are abundantly proliferated and in some spots are collected into well-defined nodules; in two of these are several giant cells, one of which has the peripheral arrangement of the nuclei seen in tubercle. Thrombosis has occurred in one or two vessels. I see no evidence of carcinoma; the nodules are, I think, tuberculous."



FIG. 9.

The parts removed from Case III. *A*, squamous epithelioma of right nostril, parts removed with a good ¾-inch of skin around margin of ulcer. *B*, mass of right submaxillary salivary and lymphatic glands. *C*, chain of right cervical lymphatic glands, the lowest being above the bifurcation of the carotid artery.

Clinical Section.

President—Sir ANTHONY BOWLBY, K.C.B., K.C.M.G., K.C.V.O.

Two Cases of Cirsoid Aneurysm.[1]

By R. P. ROWLANDS, F.R.C.S.

Case I.—A young lady, aged 18, was sent to me by her doctor with the following report: "She was run over by a hansom cab at the age of 5, but without any known injury to the foot. She did not complain of pain until about two years ago when a small vein appeared at the base of the little toe. The doctor said it was a varicose vein and ordered a bandage. For the last year she noticed she required a larger size of boot. I saw her for the first time on Saturday, October 5, when on the left foot, just at the base of the little toe and the next, there was a tender, oval, pulsating swelling about the size of a florin. The swelling could be felt on both the dorsal and plantar surfaces of the foot, and over the swelling was a small varicose vein. Pulsation was quite distinct and was stopped by compressing the posterior tibial artery. I saw it again on October 9. The swelling had increased and was very tender and rather red. I could not make out that it was connected with the bone and I could get no egg-shell crackling. My diagnosis was, therefore, a traumatic aneurysm. I asked Colonel —— to see it with me, which he did to-day. He thought otherwise and diagnosed sarcoma and he wishes to amputate the foot at the ankle. The mother, although I assured her we should make quite sure by an exploratory incision at the time of the operation, wishes to have further advice, and if an operation is necessary will let her daughter go into Guy's. To-day, October 10, owing to rest, the swelling is rather less and there is no redness, but the pulsation is still distinct."

[1] At a meeting of the Section, held December 12, 1919.

When I saw her I diagnosed a cirsoid aneurysm. An X-ray examination did not show any change in the bones of the foot.

Operation at Guy's Hospital: A tourniquet was applied, and the tumour exposed through a longitudinal dorsal incision. It was disappointingly firm, and I thought I had made an error in diagnosis, and that the tumour was after all a fibrosarcoma. I removed it widely and also the periosteum of the contiguous metatarsals, but microscopic examination by Dr. Nicholson proved that it was an arterial nævus, or cirsoid aneurysm.

Now, fourteen months after the operation, she is perfectly well and the foot is normal in every respect. It shows the great advantage of early excision of these troublesome progressive vascular tumours and this case is very striking when contrasted with Case II, which had been allowed to progress for many years until complete restoration of function and shape was impossible.

Case II.—Miss W., aged 50. This patient was sent to Guy's Hospital under my care, with a history of gradual spontaneous development of a pulsating swelling over the back of the right hand, near the thumb. Some years ago she injured her hand, and she says that the progress of the growth increased very distinctly after this. Amputation of the distal half of the thumb had been performed for gangrene, and a year later the basal half was removed for the same reason. Curiously there was no trouble with hæmorrhage after either of these operations. When I first saw her she had gangrene of the greater part of the index finger which had been bleeding repeatedly. All the veins of the hand and forearm were greatly distended and pulsating and there was a loud bruit (fig. 1). The brachial artery above the elbow was greatly dilated, probably to about four times the natural size. Digital compression of the radial and ulnar arteries had no effect on either the size or the pulsation in the vessels of the hand, but compression of the brachial artery at the middle of the arm caused the most striking shrinkage, cessation of pulsation and relief of pain. Therefore the brachial artery was ligatured with catgut in the lower third of the arm. Its wall was thin, probably owing to the enormous distension. The limb was wrapped in cotton wool and kept perfectly warm; it shrivelled a great deal. The hand became anæsthetic, but the patient retained power of movement of the fingers. Some of the skin about the wrist became purple and gangrene threatened. The sensation gradually returned in the fingers which, however, became

moderately flexed, possibly from want of sufficient blood supply to the flexor muscles. The gangrenous index finger was removed a month later, without a tourniquet being applied, so as not to interfere with the circulation, and risking extension of clotting in the spongy vessels of the forearm. The end of the second metacarpal was removed with the



FIG. 1.

Note the enormous dilatation of the veins in the hand and forearm, and gangrene of the index finger.



FIG. 2.

Note the great enlargement of the index finger and palm; also the stump of the thumb and the dilated veins.

finger, together with several dilated vessels from the palm. These contained organizing thrombi, but there was a good deal of bleeding during the amputation. The wound healed well, but some weeks later

some of the skin about the wrist ulcerated. Clots could be felt in many of the dilated veins of the hand and forearm. It is clear that the extension of the thrombosis and the pressure of the clots in the enormously dilated vessels tended to obstruct the neighbouring vessels in which the blood was still unclotted, and thus to threaten gangrene.

It is possible that ligature of the ulnar and radial arteries in the upper third of the forearm would have been better treatment in view of this danger from extension of thrombosis in the enormously dilated vessels. Amputation through the middle of the forearm appeared to be the alternative to ligature of the arteries. At an earlier stage excision of the vascular tumour in the hand would almost certainly have prevented extension of this progressive disease, and effected a complete cure without deformity.

Dr. F. PARKES WEBER: In regard to the danger of gangrene following ligature of the main artery of a limb affected by "cirsoid aneurysm" or "racemose aneurysm," I should like to point out that the artery in these cases has to supply the limb with more blood than is required for an ordinary normal limb of the same size. Sudden occlusion of the artery is therefore more likely to be followed in such cases by ischæmic troubles than is ligature of the same artery in a normal limb.

Case of Dislocation of the Patella.

By R. P. ROWLANDS, F.R.C.S.

A GIRL, aged 16, had suffered from dislocation of the patella and knock knee since she was aged 3. When I saw her at Guy's Hospital the patella was lying on the outer side of the lower end of the femur, and could not be reduced. The knock knee was considerable, the internal malleolus being 4 in. farther out than normal. I thought it probable that the dislocation of the patella was secondary to the genu valgum. There was no sign of paralysis of the muscles of the thigh.

Operation: I performed Macewen's osteotomy a month ago, and at the same time placed the patella in its natural position, after incising the outer side of the fibrous capsule of the knee-joint without opening the joint. I then retained it in position by transplanting the sartorius and gracilis muscles to the inner and upper part of the patella and securing them there with catgut. At the present time the patella maintains its normal position, and there is some movement in the knee, but it is too early to say what the ultimate result will be.

I used the same method some ten years ago in the case of a boy, whose dislocation was caused by infantile paralysis. The result was most satisfactory. This method seems preferable to displacing a part or the whole of the ligamentum patellæ, or its bony insertion, into the inner side of the tibia.

Case of Auricular Flutter.

By A. Hope Gosse, M.D.

The patient is a man, aged 72, who first noticed shortness of breath of sudden onset towards the end of 1918. Cough commenced at the same time, and he has had one small hæmoptysis. He has had periodic

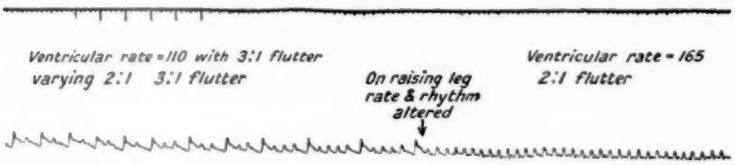


Fig. 1.

Radial tracing showing a varying rhythm which spaces. At first the rate is 110 and after raising a leg it increases to 165, and becomes regular.

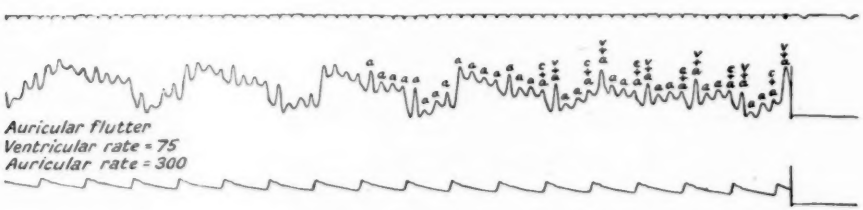


Fig. 2.

After rest in bed. Jugular and radial tracings. Regular rhythm with a ventricular rate of 75, and 4 : 1 auricular flutter.

attacks of shortness of breath since the onset; his ankles have not been swollen. When first seen a few weeks ago the heart's rhythm was regular, with a rate of 110 lying down. When he moved his leg the rate immediately jumped to 165, to return again to 110 after a few minutes. The polygraphic tracing shows a series of auricular waves equally spaced, running at the rate of 330 to the minute. The heart appears to be of about normal size, and there are no murmurs. The

Wassermann reaction is negative. Four days after first being seen he had a pulse-rate of 75 beats per minute, and equally spaced beats in the jugular tracing at a rate of 300 each minute.

The case is diagnosed as auricular flutter for the following reasons:—

(1) Sudden increase to 160 beats per minute on moving one limb.

(2) At times an irregular pulse, which spaces as in the tracings shown.

(3) A jugular pulse showing four waves equally spaced at a rate of 300 per minute to each radial pulse, when the latter is regular.

(4) Symptoms of heart failure.

His block has varied from 2 : 1, 3 : 1, and 4 : 1 in the tracings shown.

His treatment consists of rest, and administration of digitalis, of which he is having 15 minims six hourly, and when the rhythm has been changed, as it is anticipated, to auricular fibrillation, the digitalis will be stopped so that a normal regular rhythm may assert itself, as is not uncommon in these cases.

Two Cases of Unilateral Sweating of the Face.

By G. de B. Turtle, M.D.

Case I.—W. H. K., aged 32. This patient had a shrapnel wound of the right mastoid in September, 1916. This was followed by some suppuration involving the parotid gland, and a salivary fistula resulted which has now healed. The sweating is apparently limited to the distribution of the auriculo-temporal nerve on the right side. It only occurs when the patient is eating, and he volunteers the information that the "action of the jaw seems to increase it." It has been present about eighteen months.

Case II.—G. B., aged 43. This patient has unilateral sweating of the left side of the face, apparently limited to the distribution of the upper division of the fifth nerve. It has been present for the past four to five months, and occurs at any time, but it is noticeable after exertion or taking a hot meal, more especially after drinking hot tea. He was a leading seaman in the Naval Division, and was captured at Antwerp five years ago and interned in Holland for four years, returning home in November, 1918.

Dr. F. PARKES WEBER: I think that Dr. Turtle's first case belongs to a definite clinical group of rare cases with which I was specially interested in 1905. They are cases in which localized flushing and sweating (so-called "parotid sweating") of the cheek occurs on eating, in certain persons who have had suppurating wounds (e.g., from parotid abscess, &c.) on the affected side of the face. I then wrote (1905): "I believe the most probable explanation of the phenomenon to be that sympathetic nerve fibres are involved in the scar tissue, and are stretched or pulled on owing to the physiological swelling of the salivary glands during eating."[1]

Case of Pituitary Tumour showing very great Improvement after Operation.

By H. LAWSON WHALE, F.R.C.S.

PATIENT, a woman, aged 32. The most insistent symptoms were local; vertical headache, and bitemporal hemianopia eventuating in complete blindness on the left side. The general symptoms were lethargy, coarseness of hair, uninterrupted amenorrhœa, and an increased tolerance of sugar. The skiagram (*see* fig. 1, p. 30) shows enormous dilatation of the pituitary fossa, with deformity of anterior and posterior clinoid processes.

Operation (March 8, 1919) by the paranasal route: No special difficulties were encountered. The microscopic section of the growth shows it to be an endothelioma (exhibited).

After-history: Complete cessation of all headache and recovery from lethargy. The patient performs her ordinary household duties and lives a happy and normal life. As regards vision: There is no recovery of the temporal fields, but the nasal fields, as shown by the perimetric charts (*see* fig. 2, p. 30) are greatly enlarged. The visual field on the left side could not be charted at all shortly before operation because this eye had not even perception of light. It now has $\frac{6}{24}$ and reads J. 10.

[1] F. Parkes Weber, "Remarks on Localized Flushing and Sweating of the Cheek on Eating," *Medical Press*, Lond., 1905, cxxx, p. 261.



FIG. 1.

The pituitary fossa is enlarged both vertically and in the sagittal plane. The anterior clinoids are deformed and thickened; the posterior clinoids are atrophied, thick and rounded.

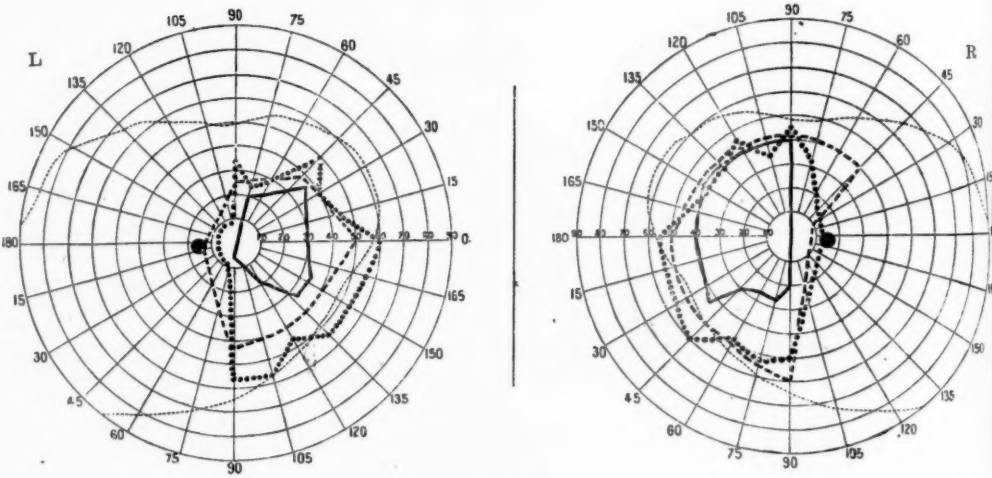


FIG. 2.

Perimetric charts. Neither before nor subsequent to operation was there any record of vision in the temporal fields, excepting from March 22 to March 29, between the lines 90 and 45 in the upper part of the right side. But the improvement in the nasal fields is striking. Continuous black line ——— September 18, 1917. Interrupted black line - - - - March 22, 1919. Dotted line ..... March 29, 1919. Immediately previous to the operation the fields were not charted because on the left side she had only perception of light.

Case of Rodent Ulcer, treated by Radium.

By R. H. BROWNE-CARTHEW, M.D.

THE patient is a woman aged 74, sent to me by Mr. Lennard Stokes, of Blackheath, for treatment by radium. On August 31, Mr. Stokes wrote: "The ulcer is 1½ in. across, and is inoperable without taking half her face away." At that time the ulcer was spreading rapidly. The ala of the nose was involved. The base of the ulcer was ¼ in. below the level of the margins, which were slightly raised and surrounded by dusky-red infiltration. The condition was very painful. Radium of an activity of 24,000 units was applied once a week for about an hour at a time, the radium being moved to adjacent positions over the ulcer every ten or fifteen minutes. After the first two treatments the ulcer was no longer painful and the spreading seemed to be arrested. The improvement continued, and on October 12, Mr. Stokes writes: "I have seen the face-woman. The improvement is marvellous."

Besides the rapidity of the change from what was the condition on September 1, what was noticeable was the manner of healing. The ulcer filled up from the base, and when the normal level was reached, the cells required for the new skin seemed to be in place, ready to form it. There was no creeping in from the margins or spreading from isolated places, of the epithelial cells, characteristic of the usual manner of healing. This may possibly be the reason why the contraction and scarring are less where healing has been brought about under the action of radium. It is certain that repaired skin and connective tissue are more normal in character when brought about in this manner.

The PRESIDENT said that he agreed with the diagnosis of rodent ulcer; that judging by its appearance at present there seemed to be a very good chance of a permanent cure, and that it would be interesting to see the case again later on.

Case of Friedreich's Disease.

By S. A. KINNIER WILSON, M.D.

PATIENT, a girl, aged 16.

Family history: One sister, healthy; one aunt died of "paralysis of the legs."

History: Could run about normally till three years ago, when she began to walk "floppily," then began to stagger. The hands also were shaky, but she can still sew. Has stooped for six months. Feet have always been arched.

Present state: Somewhat childish in appearance and mind. Spine: No scoliosis, but stoops when walking. Speech: Slight dysarthria. Feet: high-arched. Nystagmus is present on lateral deviation. Sensation: No loss detected. Motor system: Slight ataxia in upper and lower limbs. Reflexes: Arm-jerks absent; abdominals present; knee-jerks present, average intensity; ankle-jerks present, diminished; plantars, double extensor. Gait slightly ataxic.

The case is shown, *inter alia*, for the presence of knee-jerks and ankle-jerks.

Case of Multiple Congenital Anomalies of Development.

By S. A. Kinnier Wilson, M.D.

Patient, a boy, aged 5. These anomalies of development consist of internal hydrocephalus, spina bifida, lumbar myelopathic hypertrichosis, and double dislocation of the hip.

Case of Intussusception caused by Papilloma at the Ileo-cæcal Valve.

By C. Firmin Cuthbert, F.R.C.S.Ed.

Patient, a lady, aged 40. June 27, 1912: Complained of pain in abdomen for two days. Slight diarrhœa. Tenderness over the left rectus abdominis, low down, and feeling of sickness. These symptoms were attributed by her to riding a bicycle, the pedals of which were too long for her, for twenty-two miles. Whole abdomen felt somewhat dull, tense, and indefinite. She was therefore examined under an anæsthetic, and then a small rounded movable tumour could be distinctly felt. I took this to be an ovarian cyst with a long pedicle. No urgent symptoms.

June 28: Abdomen opened. A large ileo-cæcal intussusception found. This was gently squeezed out in about twenty minutes.

Cæcum was in very bad condition, but bled when scratched with a needle, so it was left in the abdomen. Knowing that a large proportion of intussusceptions in people of this age are caused by a growth in the cæcum, I re-opened the abdomen on July 12. An apparently movable tumour was found in the cæcum. The ileum was clamped and divided about 4 in. from ileo-cæcal valve, and the ascending colon was divided high up.

Microscopical report: "Papilloma, no evidence of malignancy."

Patient recently reported to be quite well, and to have been doing war work.

The points upon which I should like specially to comment are: (1) The extraordinary absence of urgent symptoms, considering how long the intussusception had been going on: (2) the situation of the papilloma; (3) the freedom from malignant invasion.

Case of Bony Anchylosis of the Knee; fixed at Right Angle; ? Gonococcus, or some other form of Arthritis.

By C. Firmin Cuthbert, F.R.C.S.Ed.

Patient, a girl, aged 25. Admitted August 11, 1915. Had been getting about on crutches for the last two years. Six months previously had acute pain in the knee, and high temperature, which was called rheumatism. Knee gradually drawn up into above stated position. No other joints affected. The symptoms she described as having at this time might have been due to the gonococcus, but that organism was not found.

August 13: X-ray picture showed tibia fixed to femur, and patella fixed on outer condyle.

August 15: Excision by Kocher's method.

December 5, 1919: She has an excellent leg. Frequently walks sixteen miles a day. Has been married three years without any signs of pregnancy; this may be due to her having been rendered sterile by the gonococcus.

Case of Appendix containing Thirty-six Shot; followed by Signs of Malignant Endocarditis.

By C. Firmin Cuthbert, F.R.C.S.Ed.

January 13, 1906: Mr. F., aged 41. History: Fifteen months before had sudden pain in right iliac region. Never well since. Always cold. Condition on above date: Abdomen distended, and colon well mapped out. Foul tongue and fœtid breath. Temperature 101° F. Pulse 88. No pain nor tenderness, but looked very ill. No actual diagnosis was made, but duodenal ulcer or chronic appendicitis was suspected. Laparotomy January 15. Appendix 7 in. long: on palpation it felt as if it contained shot; actually there were found to be thirty-six present. No adhesions anywhere, and no other lesion found in the abdomen. Having read in some article, which I regret I cannot now trace, that lead in the appendix was followed by malignant endocarditis, I gave a very guarded prognosis.

January 18: Blood examinations were made and *Streptococcus longus* grew in culture tube. Vaccine treatment given, and saline *per rectum*. Patient was still running a temperature of 101° F., and looked very ill, and had aversion to all food.

March 8: Was very ill and had had many rigors. Urine contained albumin.

March 18: Had less albumin in urine, but was very confused in his ideas, and did not know what he was talking about.

March 19: Left hemiplegia with convulsions, and death at 9 a.m.

Unfortunately no post-mortem was allowed.

Clinical Section.

President—Sir ANTHONY BOWLBY, K.C.B., K.C.M.G., K.C.V.O.

PRESIDENT'S ADDRESS:

On the Application of War Methods to Civil Practice.[1]

By Sir ANTHONY BOWLBY, K.C.B., K.C.M.G., K.C.V.O.

MY absence from England when our session opened prevented me from acknowledging earlier my appreciation of your kindness in electing me to be your President, and I gladly do so now. I have always kept in mind during the war the question of the ultimate utilization in civil surgery of that vast experience of wounds which can only be supplied by a great war; and I think that, while this is yet fresh in our minds, we should test some of those methods which thousands of British surgeons have learnt in various parts of the world.

"SHOCK" AND ITS PREVENTION.

It will be universally acknowledged that the first and most striking danger to the man wounded in war is "shock," and this condition is met with in a far higher percentage of men wounded in war than in those correspondingly injured in the occupations of peace. The reasons for this are sufficiently obvious, for the man injured in the workshop or the factory or on the railroad is not suffering from want of sleep or want of food and of water. He is not unduly exhausted by physical strain or by mental excitement, and when he is injured help is at hand quickly. The man injured in war is frequently suffering from some or all of these causes of exhaustion, and he is often thoroughly

[1] At a meeting of the Section, held January 9, 1920.

chilled after his injury through lying out in cold, wet or even freezing weather. The shock of the soldier is, as a result, often quite out of proportion to the severity of his wounds, and is consequently not to be estimated accurately as true "wound shock" until these other factors have been eliminated by appropriate treatment. Unless this is kept in mind it becomes exceedingly difficult to estimate the true value of remedial measures, for it will be found in war that complete rest, plenty of hot drinks and plenty of warmth will in thousands of cases restore in a comparatively few hours men who were cold and pulseless on admission to hospital. I have always been convinced that the most important of all the remedies for shock is warmth, and that nothing takes its place. When this was appreciated, and when every casualty clearing station had arrangements for warming men by the introduction of hot air under the blankets, the majority of the badly shocked men went to sleep after a good drink, and recovered with surprising rapidity.

It must be constantly kept in mind that the injury inflicted by rough transport may be as potent a cause of true wound shock as was the original gunshot wound itself, and it is therefore obvious that if this statement is as true as I believe it to be much of the wound shock can be obviated by sufficient care: first, in the early application of suitable splints, which should be employed in all severe injuries and not reserved for fracture cases alone; and secondly, by the supply of well-hung and well-warmed ambulance cars.

It was partly by such simple and accessible methods as these that the condition of the wounded man was so greatly improved during the war, and it will be by the application in civil life of the knowledge we have gained that a like improvement will be obtained in peace.

Splints and Ambulances.—I have been myself so convinced of the importance of care in the transport of injured men that, with the aid of the Army Medical Service and of the British Red Cross Society, I have arranged for the equipment with suitable splints of the motor ambulances which have been supplied to various parts of Great Britain, and arrangements are now being made to see that the ambulance drivers and orderlies are as thoroughly trained in the use of the "Thomas outfit" for fractured extremities as were our orderlies at the front in France. It is quite easy to train men who have no surgical knowledge at all so that they shall provisionally put up a fractured leg or thigh much better than the best surgeon could have done it before the war, and I believe that one result of what our experience has taught us is

that in every country, arrangements should be made to teach how badly wounded men may be taken, even for great distances, without further injury being inflicted upon them by the transit.

The Treatment of Shock.

So much for prevention of shock; but, as far as treatment is concerned, the practical deduction which is to be drawn from a consideration of our present knowledge of shock is that, inasmuch as the principal phenomena are loss of blood volume and fall of blood pressure, the obvious aim should be to restore these.

The treatment by rest, warmth and taking of fluids is indicated in all cases, whatever more may be required, and morphia in moderate doses will be useful in allaying pain, anxiety and excitement, and so encouraging sleep. In many cases nothing more is needed. The simplest method of increasing the blood volume is to administer fluids by the mouth, and, as all soldiers in battle areas have only limited supplies of water to drink, it should be accepted as an axiom that all wounded men always require drink as soon as they arrive at an aid-post or field ambulance.

Unfortunately many patients with really severe shock are quite unable to keep down any fluid, and in that case rectal injections are often most useful. I believe it is true that both blood pressure and blood volume are more likely to remain permanently improved by gastric or rectal absorption of fluid than by the intravenous administration of the same quantity. It seems clear that the direct addition of fluids to the venous blood is not so good as the making of blood serum by the patient himself from fluid absorbed through a mucous membrane, but in many cases of severe shock, and in view of the fact that delay may mean disaster, it is often necessary to perform intravenous infusion as soon as the patient has been warmed and rested. We have learnt that normal saline solution is useless for this purpose, but it is the opinion of most observers (and it is mine) that Bayliss's solution of 6 per cent. gum arabic in normal saline is often of the greatest benefit in raising the blood pressure for long enough to carry the patient over the danger period.

In proportion as the shock conditions are due to excessive bleeding, immediate infusion of blood is better than gum solution, and is undoubtedly indicated; not less than a pint should be introduced. There is no doubt that many lives were saved by this practice, and

there is also no doubt that it was not until the surgeons of the United States Army joined the British casualty clearing stations in 1917, before Passchendaele, that blood transfusion became common. Its employment was subsequently utilized yet farther forward, and great benefit ensued when prepared citrated blood was sent up to the field ambulances when a battle was impending during the advance of the Allies in 1918, as it could safely be kept for from twelve to eighteen hours or more before use. I would suggest that in future sterilized gum solution should be kept ready for use by operating surgeons and by all civil hospitals, and also that test sera for donors of blood should be regularly stocked, so that no delay may arise in the choice of a donor, for if such treatment is necessary at all, it is certain that there will be no time to waste, as the experience of war has shown that if blood is transfused too late it is quite useless.

Anæsthetics.

But if we have learnt much that is valuable about shock and its prevention and treatment, we must realize that after the patient had been pulled through with difficulty it was often necessary to operate as soon as possible and before sepsis had appeared upon the scene: so that no sooner had the patient been saved from the shock of his original injury than he was obliged to run the risk of operation shock. Still, where limbs were hopelessly shattered, or main vessels were torn, or the abdomen was shot through, &c., there was no choice left to the surgeon, who also knew that in many cases the rapid removal of a smashed limb resulted in great relief of symptoms. But operation generally meant anæsthetics, and here we were destined to learn a great deal that was new to us.

The Choice of an Anæsthetic.

With regard to the choice of an anæsthetic agent, it may be conceded at once that for all slight operations on patients in good condition "open ether" was very often quite satisfactory. But when in wintry or wet weather very many of the wounded were already suffering from a bronchial catarrh, ether was very liable to cause grave pulmonary complications, and it was not until we introduced the "Shipway apparatus" for warming the ether vapour that it could be safely administered in such cases. I would very strongly advocate that in the future warm ether vapour should be systematically employed as the better routine method where ether is used in civil practice, for it is not only

less irritating to the lungs, it is also much less liable to cause severe vomiting. But in cases of shock neither chloroform nor ether is safe, for a careful record of blood pressure in operations during shock by Major Geoffrey Marshall showed conclusively that after the use of these anæsthetics, although the blood pressure might be high when the operation was finished, this was soon followed by a reactionary fall and death.

And not only is ether not a safe anæsthetic in shock, it is also very injurious if, as is so often the case, it causes nausea and sickness in men who need every ounce of fluid nourishment that can be given; for this sickness, if continued for some hours, will destroy the last chance of recovery for the shocked man.

Much had been expected from the intrathecal use of stovaine, but the sudden fall of blood pressure it caused proved it to be very dangerous to shocked men, and not so good as novocaine. Local anæsthesia was so tedious that it was not possible to use it as a routine method where patients were numerous. It was very useful in selected cases. There remained nitrous oxide and oxygen. The value of this agent in shock had been conclusively proved by Marshall in 1916, and his observation showed that under its influence extensive operations could be performed without any immediate or subsequent fall of blood pressure. Its use saved many lives, and in abdominal operations the very best results of all were obtained by combining it with infiltrative anæsthesia of the abdominal wall. By this combined method the gas and oxygen required was reduced to a minimum, and the operator was not troubled by abdominal rigidity. It was by the use of this combination that Majors D. Taylor and G. Marshall at "Remy Siding" succeeded in saving seventy-four patients in a consecutive series of 101 operations for perforating wounds of the abdomen, and I believe that these results were amongst the best obtained in the British Army during the war.

Very much more might be said of war anæsthesia did time permit, but I would urge that gas and oxygen should be supplied, together with suitable apparatus for all the Army Medical Services of the future, and I would also press upon your attention the value of warm ether vapour in the prevention of pulmonary complications.

May I add further that I believe that the combination of gas and oxygen with local anæsthesia is the very best anæsthetic agent for a number of the abdominal operations of civil life, and for many others also, for it causes no fall of blood pressure, it eliminates anæsthetic shock, and it abolishes post-anæsthetic vomiting and all its attendant

troubles. It must, however, be remembered that it is not always a satisfactory anæsthetic for vigorous and muscular people on whom a prolonged operation is to be done, and that its lengthy administration requires both experience and special apparatus.

The Treatment of Patients with Fractures.

Let me ask you now to direct your attention for a few minutes to the treatment of fractures, for I think it is the opinion of all surgeons that here there have been great developments. For example, compound fractures of the femur are now treated infinitely better than before the war.

I will briefly sum up the position by saying that the fractures in France in the last half of the war were treated by "extension and suspension," so that the circulation, nourishment and mobility of the injured limb were maintained throughout as far as possible. Constriction by bandages has been reduced to a minimum, and limbs are no longer encased in wooden boxes or in plaster of Paris, and, as a result of the precautions to maintain in this way the vitality of the limb, wasting of muscles and stiffness of joints have been largely prevented.

It is greatly to be hoped that our war experiences will be utilized at home, and that all hospitals and medical schools will arrange for a much more systematic teaching of how to deal with fractured limbs, so that all young surgeons will in future realize that the so-called "setting" of the fracture should be really only the beginning and not the end of surgical treatment. And if this aim is to be achieved it will be necessary that some at least of the fracture cases in general hospitals shall be collected in "special fracture wards" equipped with the necessary apparatus for "extension and suspension," and supplied also with a mobile X-ray outfit, for there are many cases where a good result can only be obtained by frequent radiography at the bedside.

Let it be fully realized that so long as the principles I have sketched are kept in view the details of how best to carry them into practice must be left to the skill and ingenuity of individual surgeons.

Wound Infections and Antiseptics.

Throughout the war experience of wound infection was on a larger scale than had been anticipated, and I will only touch on one or two of the more obvious features of this great subject. It is already widely

known that tetanus was prevented by prophylactic injections so successfully that its incidence immediately fell 90 per cent. as soon as the antitoxin could be supplied in sufficient quantities, but it must be appreciated that if success is to be obtained in civil practice the injections should be given within a few hours of injury. Unfortunately we had no similar success in the employment of prophylactic injections for "gas gangrene," and even at the end of the war the most recently manufactured sera were of doubtful value. At first we found no real remedy. Time and experience alone led us to the proper treatment by excision of all devitalized tissue and the careful and thorough mechanical cleansing of the wound; and it was during this period of learning and experimenting that "antiseptics" were given so thorough a trial and were proved to be so valueless when placed in grossly contaminated tissues.

The uselessness of attempting to sterilize a grossly infected wound by the single application of a powerful chemical agent is really nothing new, and my friend Sir George Makins pointed out in his Hunterian oration that Lister himself was quite aware of this, for he wrote, "If for example a pair of forceps is handed to the operator with the intervals between the teeth occupied by dry septic pus, and a portion of this dirt becomes detached and left in the wound, the evil cannot be corrected by any antiseptic wash that is now at our disposal or that the world is ever likely to see." Very striking words! In spite of this dictum, however, some of Lister's admirers in England urged us in France to try to sterilize wounds by the early introduction of powerful antiseptic agents, and with the natural result that such measures were tried and absolutely failed.

Suture of Wounds.—I have already said that the only real method of sterilizing the wounds of war, as we saw them in France, was by a suitable operation and the mechanical removal of all that might do harm, so that no tissue which is too badly damaged is left. The next question we had to decide was how to treat such a wound subsequently, and, as you are well aware, the answer was ultimately found in the closure of the wound by either "primary" or "delayed primary suture."

And we also learnt both that the results of suture were not materially influenced by the use of any antiseptic agent, and also that wounds could heal well and firmly even though they contained considerable numbers of micro-organisms, so long as the latter did not include virulent streptococci.

It was evident that the tissue cells could destroy the less virulent bacteria, provided that the latter were surrounded by healthy living

tissues, and that by closure of the wound the advent of fresh infection was effectually prevented. The presence of bacteria of very various kinds in wounds which nevertheless did well was recorded by observers of every nationality, and especially in the United States units by Crile, Kenneth Taylor, and H. Cabot; and in the British units by Forbes Fraser, Adrian Stokes, McNee, and the workers in Almroth Wright's laboratory. In almost all regions of the body we closed wounds successfully, either by primary or secondary suture: in the head and neck, the limbs, the chest, the abdomen, and the large joints, and we soon learnt by experience that if patients could not be retained at the front after suffering from extensive wounds it was better to send them to the general hospitals with a packing of gauze, and let the wound be sutured without further interference after arrival. We found no advantage in using antiseptic substances with this packing, although many surgeons did employ flavine or solutions of the bismuth, iodoform, and paraffin paste suggested by Rutherford Morison.

Much more might well be said as to details of treatment in different regions of the body, but the results obtained in the later stages of the war may be briefly summed up by saying that not only were many lives and limbs saved by wound closure, but that compound fractures healed more rapidly and with much less necrosis, joints were saved from acute arthritis, suppuration of wounds became rare, septic infection with its fever and emaciation diminished, and convalescence was infinitely more rapid. From the army point of view fewer beds were occupied, nurses and surgeons were spared much unnecessary work, and many soldiers were returned to their units fit and well within a few weeks instead of suffering for months from suppuration.

War Methods in Civil Practice.

Surely it is evident that much of what we have learnt is applicable to civil practice. Is it not certain that we shall close open wounds of the knee, that drainage-tubes will be much less employed, and that "delayed primary suture" will become a common method for the treatment of bad compound fractures and the lacerated wounds caused by machine accidents? The same method of wound closure by delayed suture will be found just as useful in amputations through tissues near to suppurating areas. We found it most helpful in the treatment of Syme's amputation, and Colonel Elder, when consulting surgeon at Rouen, showed me a very striking series of successful amputations by Syme's method performed upon natives of India for gangrene following

frost-bite, and treated by suture after being packed with gauze for three or four days. I believe that there is a wide field for the application of war methods to the treatment of the injuries of civil life, but I would emphasize that in suturing wounds after the interval of a day or two it is essential for success that no further wound toilet be permissible. The packing should merely be removed as gently as possible and the sutures inserted and tied.

Secondary Infections of Wounds.—Our experiences of the value of closure of wounds very naturally directed our attention to the question—How did the wounds become infected by pyogenic organisms? We found that not more than 15 per cent. of recent wounds were infected by hæmolytic streptococci when examined in the operating theatres of the casualty clearing stations at the front; but we were told by our bacteriologists at the different bases that within a week of arrival over 90 per cent. were infected; and shortly before the war ended we appointed an Anglo-American Committee to investigate the whole question of streptococcal infection. Of course, it was evident that when wounded were numbered by the thousand it was not possible to ensure complete sterilization of hands and instruments after each case had been examined or dressed, but it did appear to be very clear that, as instruments are more easily sterilized than hands, it was very desirable not to touch the wound or the dressings except with instruments, for we had no doubt that wounds usually became contaminated when they were dressed. It was then decided that an investigation should be made in order to ascertain whether many of the medical officers, nurses, or orderlies at the bases were acting as "carriers," and 8,000 examinations were made (in the American Army) of their throats, with the result that it was reported at the last meeting of the Committee that "carriers" of hæmolytic streptococci had been found in different bases to number from 15 to 25 per cent. of all the hospital personnel. Here, unfortunately, the matter had to be left, for the war had ended, and the streams of wounded men had, happily, ceased to flow.

It is, of course, not to be supposed that carriers of streptococci are to be found in civil life in any such numbers as those I have mentioned; but experience of the general hospitals in France does show the danger of wound infection spreading when large numbers of streptococcal-infected wounds are accumulated in war conditions: and, what is most important, it explains why it was that the closure of wounds by suture was of such great benefit to the wounded soldier. I would also suggest that the investigations we began are worth pursuing in hospital wards

given over to the treatment of cellulitis and erysipelas, and that, in the interests of both staffs and patients, this should be done.

Suppurating Wounds.—There remains the question of the treatment of suppurating wounds, and here we have learnt two things: first, that it is more often possible to suture them than we had previously thought; and, second, that Carrel's method is often a very great improvement on the drainage-tube and the daily syringing.

With regard to the first, we found in France that very many wounds could be successfully sutured even if they were suppurating. Secondly, with regard to Carrel's method of treating suppurating wounds, there is no doubt at all that it has marked a very great advance. It supplies a thoroughly scientific and carefully thought out technique, and has been proved to be of the utmost value in the hands of many of the best French and Belgian surgeons, as well as in British hospitals. I have seen admirable results from its careful employment, and I believe that where surgeons have had an unsatisfactory experience this has been largely due to an imperfect technique, for in many hands it has given admirable results in stopping sepsis and suppuration, and in enabling granulating wounds to be closed by secondary suture.

It is difficult to decide the rather academic question as to whether the method owes its success to the hyperchlorous solution of Carrel and Dakin or to the method of irrigation, but I think myself that the method is more important than the particular solution. Here, again, we have learnt much that is useful in the treatment of the suppurations of civil surgery, for Carrel's method is not only valuable in the wounds of war, it is quite as efficacious in the treatment of an empyema or pelvic suppuration. I have seen most excellent results from its use in large appendix abscesses on the one hand, or in suppurating joints on the other, and I am convinced that it has supplied to the operating surgeon a valuable addition to his armament.

Conclusion.

In conclusion, I cannot doubt that in the future the civilian surgeon will also work even more with the bacteriologist than he did before the war, for it has been by collaboration and experiment that many of our advances have been maintained without risk to our patients. And, whatever we may have learnt, there is inevitably much more to learn in the near future. Let us all keep this in mind, and let us also remember that, as far as details are concerned, a too rigid standardization may be harmful. The only safe attitude is that we should be always inclined

to believe that however good a method may be it is always possible to evolve another which may be better still.

I have endeavoured to point out to you in what I have said that the surgery of the immediate future may well be expected to benefit from our recent experiences; that we should improve our transport service and our first-aid for the wounded in civil life; that shock can be better treated and can often be prevented; that anæsthesia can be rendered safer; that no antiseptic can at once sterilize infected wounds; that in delayed suture we have an important advance in surgical technique, and that suppurating wounds can be treated by improved methods. You are all well aware that were I able to consider in detail the surgery of the various regions, such as the head, the chest, the abdomen, &c., I should be able to record very many examples of advances in surgery, but this is not the occasion for such a discussion, and I am content to deal only with more general principles.

DISCUSSION.

Mr. VICK: I am sure that we must all appreciate the importance of the President's insistence upon the value of the application of methods learnt during the war—be they new or be they old and improved—to civilian practice. I do not think that much is left to say under the various headings but I should like to deal briefly with the use of true primary suture in civil accidents. My experience of this treatment dates from eighteen months before the end of the war. During those eighteen months, I did the surgery at a large general hospital in Salonika on the main Seres Road. To this hospital, numbers of transport accidents were admitted and it was our practice to carry out primary suture on all cases of compound fracture (which included femora, tibiæ, fibulæ and skulls). The results were most encouraging and I attribute the success to the fact that the essentials to success were ours, namely, immediate operation, a good theatre, and no reason for evacuating the patient until complete healing of the wound and union of the fracture had taken place. Recently I have had the opportunity while working at St. Bartholomew's Hospital under Mr. Gask, of treating all similar cases admitted during our tour of duty by this method. The cases are uniformly satisfactory and demonstrate the value of the method. The head cases are particularly so, and one realizes that in its application to these the method is not new. The operations were performed immediately the primary shock had passed; all bruised and damaged tissue was removed; ether was used before closure of the wound and absolute immobility in as perfect position as possible was obtained. One case will show the method and the result. A woman was admitted who had fallen down an area. She was six months pregnant and had sustained a compound comminuted fracture of her vault and her left tibia and fibula, also another very extensive scalp wound. She was operated upon by the team method

and all the wounds closed by primary suture. The wounds healed by first intention except part of the leg wound which was later closed easily by secondary suture. The fractured leg is now united in good position. This case is a good illustration in view of the mulitiplicity of the wounds, of the pregnancy undisturbed by the accident or the operation, and of the ultimate good result.

Sir CUTHBERT WALLACE: I hope that the method of first line application of a Thomas's splint which was learned in the war will come into general use as a first-aid measure in this country. In the case of men the application is easy, but in the case of women there are obvious disadvantages if this splint is to be applied in public. As to the primary suture of wounds after fractures it is really no innovation, for I can read you directions as to the treatment of compound fractures written in the year 1899, which sounds as if they had come out of the "Memorandum on Wounds" which was published towards the end of the war. When one considers a compound fracture, say of the femur, produced by indirect violence such as is met with in civil practice, it is very obvious that the excision of the wound as practised in France is a matter of very considerable difficulty. In the case of a gunshot wound the injury is more or less localized, but in the case of indirect violence fractures and consequent distortion of the limb by the accident, the laceration of the muscles is very widespread, so that really to excise the wound the limb would have to be slit from top to bottom.

Mr. W. GIRLING BALL: In the first place I should like to express my appreciation of the remarks of the President on the application of war methods to civilian surgical practice. It is impossible on an occasion such as this for one to touch on more than a few points. I should like to draw attention to two of them, viz., the methods of suture and the relationship of the bacteriologist to the surgeon. With regard to the former, it has been extremely interesting to watch the progress which has taken place in the treatment of wounds produced by war implements during the past four and a half years. Passing from the time when drainage was first used, going on to the application of all sorts of antiseptics and various methods, the time arrived when it was possible to suture closed wounds by primary suture. This was due to the discovery that it was necessary to excise all devitalized tissues so as to prevent the wounds becoming infected by contaminating material, and the success of this method became more marked towards the end of the war period. One of the methods with which I was more intimately impressed was that of delayed primary suture. In this case it was usual for some doubt to have occurred as to whether or not the excision had been so complete as fully to eradicate the chances of infection. Having carried out the excision as far as possible, the wound was then packed with some antiseptic, e.g., gauze soaked in flavine, ether and so forth. This was allowed to remain in contact with the tissues for a period of not more than forty-eight hours, then removed, and if the clinical appearances of the wound indicated it, the wound was closed by suture.

In carrying out observations on some 100 cases, together with minute observations, both bacteriological and clinical, I found that eighty of these cases healed by primary union, and of the remaining twenty some ten or eleven of them were almost completely healed. The application of the method was not only limited to injury to soft tissues, but also to a considerable number of compound fractures which became united by primary union without suppuration in a large number of cases. Since returning from France I have applied these methods to compound fractures in civilian practice. There is considerable difference in the type of wound one sees: the great laceration of the tissues usually observed in war wounds is in most cases wanting amongst civilian patients, but I have on three occasions applied these methods to compound fractures of the humerus with success. Passing then to the relationship of the bacteriologist to the surgeon, it must be evident to everybody who has done any work in war surgery the great advantage of this association; it is not only an interesting fact to learn what is the nature of the infection in a wound, but it is of considerable value from the point of view of prognosis. It is not every surgeon who has had the opportunity of working in a pathological department, but to those of us who have, the appreciation of this fact has been the common knowledge for many years. It is hoped that this method of observation will be further extended now that the usefulness of bacteriology from the point of view of prognosis has been learned. A great deal of the progress which has been made in the treatment of wounds has been due to those many efforts of the workers in this branch of medicine, and there can be little doubt that in the future, results of their investigations, especially from the serological point of view, will be a beneficial factor in helping the surgeon to deal with those dire cases of general infection, for which at the present time there is no adequate remedy. I should like to add one other word with regard to the treatment of fractures. The use of the Thomas's splint has become a valuable asset of the surgeon. To so many types of fracture can this be applied that the many complicated and various splints which were used before the war will in all probability disappear. In civilian practice it has become my custom to utilize all those methods which were used in France, and I am quite convinced that the best results can be obtained by the application of these methods, far better than the majority of those which were in use prior to the war.

Major-General CORKER: I desire to add something in support of the last speaker's reference to the prevailing ignorance of the principles of first-aid to the injured. The President is, no doubt, familiar with the increasing efforts of the Army Medical Service to instil some knowledge of first-aid and hygiene into the officers and men of our armies. Schools seem to regard the subject as quite outside their programme. I have been several years on the Committee of a school in which every "ology" is taught except the science of keeping alive. Yet, except the individual can remain alive, all knowledge must be valueless to him. I have at last succeeded in having instruction therein added to this particular curriculum. I advocate greater attention in all universities and schools to such an all-important subject.

Mr. A. W. SHEEN: The position of surgical consultant in India did not give one the opportunity of seeing cases, at the earliest, until nearly a fortnight after their incidence. My experience of Mesopotamian wounds was that they did not suppurate so frequently or so freely as the French wounds, being more like the South African wounds; this probably being due to the relatively uncultivated, unoccupied soil of Mesopotamia. Where infection was established we endeavoured to conduct treatment on the lines described. I might mention a method of avoiding shock which was apparently successful in a case of thigh amputation for severe infection, viz., the direct temporary anastomosis of the femoral artery with the femoral vein at the amputation site over a U-shaped glass tube, the blood thereby being brought directly back to the right heart. The President has referred more particularly to the lessons of the war as regards treatment. I should like to add and amplify that, accompanying these, must be the lessons as regards organization. It should be less difficult in peace than in war to organize "first-aid," so that we may find in peace the counterparts of our regimental aid, our field ambulances and our casualty clearing stations. Everyone with a large hospital experience knows in what a very "bad shape" many injuries reach the hospital. There is need for organization in factories, mines, railways and all big industrial works; also in and for the home. I commend this work to the Joint Committee of Red Cross and St. John which I am glad to know is going to carry on its war-time activities into peace. There is nothing new in the cleaning and immediate closing of a "contaminated" wound. It has been done many times successfully. I recall a case which I saw when a student at Guy's under the late Mr. Davies-Colley, in which a large lacerated wound opening the knee-joint widely was scrubbed with soap and water and closed throughout with an immediate perfect result, I admit somewhat to the surprise of those concerned. Various factors influence the healing of wounds. One knows how well face and head wounds heal. I noted from many years' experience, how, of the wounds from a combined iron-works and colliery district, the colliers' wounds healed at once, for coal heals in and is almost an antiseptic—the tattooing from healed-in coal is a common sight—while the greasy wounds in the ironworkers almost always suppurated. I hope that the President's influence will help to bring about that improvement in organization without which improvement in treatment will be, relatively, useless.

Case of Comminuted Fracture of the Shaft of the Left Humerus, with Unusual Nerve Complications.

By T. P. LEGG, M.S.

The patient is a young man, aged 17. He was knocked down by a motor lorry and run over on October 1, 1919. On admission, he was found to have a comminuted fracture of the left humerus, about the

middle of the shaft, contusions of the left arm and thorax, and to be suffering from severe shock. He was blanched; the pulse was rapid—130 per minute; there was left-sided abdominal pain and slight increase in the area of splenic dullness. These signs gradually abated, though the anæmia persisted for some time longer. The fracture was treated by extension in an abducted position on a Thomas's ring splint.

The signs of injury to the nerves were first observed about the twelfth to the fifteenth day, inability to move the fingers being first noticed.

On October 24 the splint was removed, as union was taking place, and the arm placed between sandbags in a less abducted position. He was unable to flex the elbow, and movements of the wrist-joint were absent. Massage was begun on October 27, and electrical treatment on November 3, 1919.

On October 17 the electrical reactions were as follows: (1) Median muscles—no reaction; (2) ulnar muscles—diminished reaction; (3) musculo-spiral forearm—no reaction; (4) musculo-cutaneous—no reaction; (5) deltoid reacts; triceps not tested.

On November 11, 1919, he was examined by Dr. S. A. K. Wilson, who reported "definite motor and sensory paresis (not an absolute paralysis of either) in the median, musculo-spiral, and musculo-cutaneous groups. The ulnar is relatively intact." On this date he was encouraged to begin to use his arm.

On December 9 the median nerve showed R.D.; the ulnar and musculo-spiral react to reduced faradic current.

On December 29 the reactions were unchanged; he could flex the elbow to 90°.

Treatment is still being continued, and improvement in muscular power is manifest. The hand and forearm are kept on a splint.

Case of Sarcoma of the Scapula.

By Philip Turner, M.S.

The patient, T. L., aged 46, occupation an asphalter, has a large tumour of the right scapula involving the whole of the bone below the spine. When first seen as an out-patient, about three weeks ago, he complained of weakness and stiffness of the right arm, which had first been noticed in September, 1919; he was unaware of the presence of the tumour which was found in the course of examination. He had

also been under treatment for two or three weeks for "tonsillitis," but had noticed only discomfort on swallowing rather than any severe pain; there had been no previous attacks of throat trouble.

On examination, there is a large hard tumour of the right scapula, extending from the spine to the inferior angle, not adherent to the skin or to the chest-wall. Enlarged and hard glands can be felt in the axilla, and there is also a gland in the posterior triangle. The patient has lost flesh and strength generally during the past few months, but more especially in the right arm, which is distinctly smaller and more flabby than the left. All movements are present in the affected arm, but there is a general loss of power. There is marked wasting of the extensor muscles of the forearm, and of the biceps, triceps, deltoid, trapezius, and of the axillary folds. Tactile sensation is normal, and the weakness is due to pressure on the brachial plexus by the enlarged axillary glands. The right tonsil, which is large and fleshy, with some irregular ulceration near its centre, appears to have slightly decreased in size during the past three weeks; enlarged glands can be felt behind the angle of the jaw. Radiographic examination shows considerable destruction of bone, and the appearance confirms the diagnosis of sarcoma. The Wassermann reaction is negative.

The patient has a cough which is probably due to the enlarged tonsil, but there is some dullness at the left base behind, though there are no other physical signs in the chest.

Excision of the scapula and removal of the glands is practicable, but further investigation as to the nature of the enlarged tonsil and as to the presence of secondary growths in the lungs must be carried out before this is decided upon.

Note.—A few days later the projecting part of the enlarged right tonsil was removed by the guillotine and examined histologically by Dr. G. W. Nicholson. It proved to be a squamous-celled epithelioma. There were also signs of fluid at the left base, and the patient on several occasions expectorated blood-stained mucus. No operation was performed, and the patient preferred to go home.

Clinical Section.

President—Sir Anthony Bowlby, K.C.B., K.C.M.G., K.C.V.O.

Case of Thrombo-angeitis Obliterans with Rarefactive Bone Changes, but without Massive Gangrene.[1]

By W. Salisbury Sharpe, M.D.

H. N., Jew, aged 23.

Family history: One relative is known to have suffered from some disease characterized by necrosis.

History: Disease commenced at the age of 4 and has since been steadily progressive. Pain is, and has been, constant.

Present condition: Both hands and both feet are affected by the disease. Right hand: Thumb, fore, middle and ring fingers, part of ungual phalanx lost. Little finger: All ungual and two-thirds of middle phalanx lost. Left hand: Similar changes in thumb, fore and middle fingers. Ring finger has lost whole of ungual and most of middle phalanx. Little finger contracted. Feet: Loss of almost all ungual phalanges.

The skiagrams (figs. 1, 2, p. 52), beside showing the definite loss of portions of the bones of the digits, show a very definite expansion and rarefaction of the distal articular ends of some of the bones, specially noticeable in the metatarsals. The metatarsophalangeal joints of the little toes show arthritic changes. It is noteworthy that in both hands and feet, if anything remains at all of an ungual phalanx, a nail is present. Pulsation can be felt in both anterior tibial arteries, but not in either posterior. The skin of the affected parts is atrophic and shrunken. Blood and urine normal.

[1] At a meeting of the Section, held February 13, 1920.

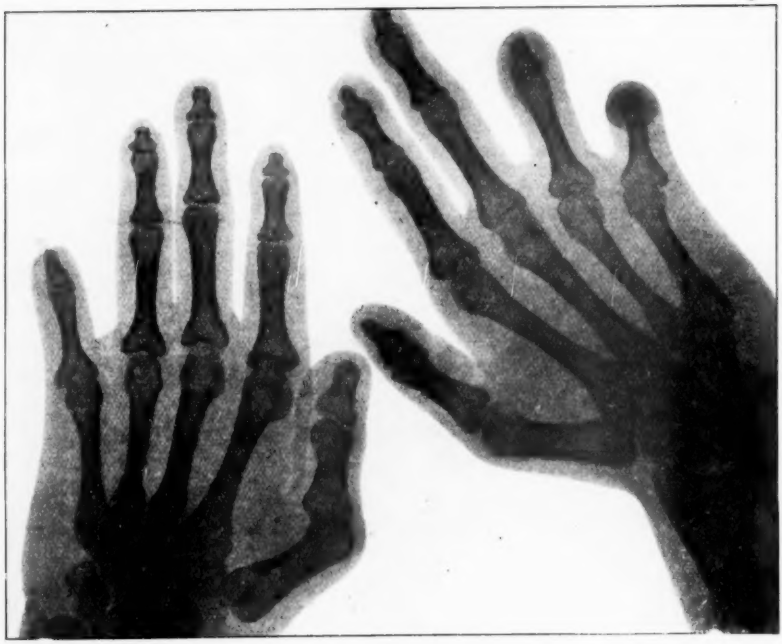


FIG. 1.



FIG. 2.

DISCUSSION.

Dr. F. PARKES WEBER: In this case the pulsation in the dorsalis pedis artery in both feet is very good, and that fact in itself, in my opinion, is sufficient to negative the diagnosis of "thrombo-angeitis obliterans." I showed the same patient before the Clinical Section of the Royal Society of Medicine[1] on November 14, 1913, as an example of "Angiokeratoma, with Bony Changes." He was then aged 16, and since the age of 4 he had suffered severely from chilblains of the hands and feet and pinnæ of the ears, which had left a good deal of scarring. The Wassermann reaction was negative. I remarked at that time that the chilblainy condition ordinarily associated with angiokeratoma of the hands and feet seemed to me insufficient to account for the bony changes revealed by Röntgen-ray examination.

Sir JAMES GALLOWAY: It would be very unwise for anyone to criticize adversely a diagnosis of thrombo-angeitis obliterans made by Dr. Parkes Weber. Dr. Weber has had unique experience of this disease in London, the great majority of the cases have been seen by him, and many of them carefully described. His association with the Dalston Hospital has given him an opportunity of seeing these cases amongst the Jewish population in the East End of London. Dr. Weber told me that this case had been previously shown by him, both at the last International Congress of Medicine and at this Section of the Royal Society of Medicine. I have now some recollection of having seen the case previously with Dr. Weber. The difficulty of the diagnosis, however, is now much greater; the disease has progressed with definite loss of tissue in all four extremities, including bone, and I would draw attention to the fact that though certain of the arteries might be felt pulsating in the extremities a perfect circulation does appear to exist. Angiokeratoma is usually a disease in which destruction of tissue is slight and the lesions on the extremities are comparatively small and localized. While admitting Dr. Weber's criticism, I think it is difficult to accept a diagnosis of angiokeratoma as complete in this case, and I suggest further investigation to ascertain the reason of the remarkable destruction of tissue, possibly associated with defective circulation due to obstructed blood-vessels.

Case of Unilateral Sweating.

By G. DE BEC TURTLE, M.D.

THIS case was shown before this Section at the December meeting. Since then he has developed a paresis of the external rectus on the right side, producing diplopia. A Wassermann reaction was negative, but there was a direct specific history some twenty years ago. He has therefore been treated on these lines with considerable improvement.

[1] F. Parkes Weber, *Proc. Roy. Soc. Med.*, 1913-14, vii (Clin. Sect.), p. 25 (with illustrations).

At the same time the sweating has improved, and I have brought the case again this afternoon, as I thought it might be of interest as to whether there is any connexion between the two conditions.

Unilateral Dwarfism of Limbs connected with Congenital Chondromata.

By F. Parkes Weber, M.D.

It is not generally recognized that the presence of congenital chondromata in long bones may be in some way causally connected with dwarfism of the affected limbs.



Fig. 1.

Photograph of the patient (1920) to show the dwarfism and deformity of the right limbs.

The patient, V. K., aged 13, is a healthy boy, whose only abnormality on ordinary examination is that his right limbs are too short and somewhat deformed (fig. 1). Following are measurements: arms, from acromion to tip of the styloid process of the radius, on the right side, 35 cm.; on the left side, 45 cm. Legs, from the great trochanter to tip of the external malleolus, on the right side, 50 cm.; on the left side, 67 cm.



FIG. 2.

Röntgen skiagram (1920) of the right forearm, to show the chondromata at the distal ends of the radius and ulna; there is also a smaller one near the proximal end of the radius.

The accompanying illustrations (figs. 2 and 3) are from Röntgen skiagrams taken in January, 1920, and show the presence of a large chondroma at the lower end of the right tibia and of chondromata in the right ulna and right radius. There is a curious bending of the right fibula, doubtless due to pressure from the tibial chondroma.



FIG. 3.

Röntgen skiagram (1920) of the distal portion of the right leg, to show the large chondroma at the lower end of the tibia.

There is no history of any similar condition in the patient's family. According to the mother nothing abnormal was noticed on his birth,

excepting a peculiar mottling of the skin of the right upper and lower extremities. The shortening of the right limbs was not observed until the boy was 1½ years old. One or two orthopædic operations have, I believe, been attempted. In January, 1920, the boy fell and fractured his right forearm. I am indebted for the case to my surgical colleague, Mr. A. Compton.

An almost exactly similar case in a boy, aged 6, was recently shown by J. E. Adams, at the Section for the Study of Disease in Children, as a "Case of Shortening of the Limbs on one side of the Body."[1]

Another case, in some respects resembling the present one, was shown by Cameron and Trethowan at the same Section of this Society on April 26, 1918.[2] Recently also Clemens described the case of a woman, aged 36, with dwarfism, in whom he thought that the deficiency in height was connected, not with rickets or achondroplasia, but with the presence of multiple chondromata.[3]

Myxœdema, with High Blood-pressure, preceded by Enlargement of the Thyroid Gland.

By F. Parkes Weber, M.D.

The patient, D. O., a woman, aged 55½, has still the typical appearance of myxœdema (see figure), but, under thyroid treatment lately, she has been losing in body-weight and her face has become thinner; her speech is also quicker than it was and she has become less sluggish in her mentality. Her symptoms commenced about seven years ago, rather more than a year after her menopause. She at first complained of a feeling of ".stiffness" in the feet. The thyroid gland was observed to be enlarged six years ago, and it is still decidedly enlarged, but is apparently smaller than it was then. There have been no symptoms of Graves's disease, such as exophthalmos, tremor, or cardiac palpitation. She complains much of a kind of "acroparæsthesia" in her fingers and toes. The Wassermann reaction is negative.

More observations are required as to the blood-pressure in cases of myxœdema. In the present patient the brachial systolic blood-pressure is 210 to 240 mm. Hg.

[1] *Proc. Roy. Soc. Med.*, 1918-19, xii (Sect. Study Dis. in. Child.), pp. 5-8.

[2] Cameron and Trethowan, *ibid.*, 1917-18, xi, pp. 45-49.

[3] Clemens, "Multiple Ekchondrosen bei Zwergwuchs," *Med. Gesellschaft zu Chemnitz*, May 14, 1919; abstract in *Münch. med. Wochenschr.*, 1919, lxvi, p. 1093.

In regard to sluggish emotional effects in cases of myxœdema the following is, I think, an illustration. The present patient's son was very rude to the hospital porter on a visiting day. The patient was sorry, but not much affected. A few days later, however, I found her almost in tears and wishing to leave the hospital at once, because, she said, she feared that, at the next occasion when her son was coming to visit her, there would be a fight between him and the porter. This idea had been growing for six days or so in her mind, and I had some



The patient during thyroid treatment.

difficulty in dispelling it. On the other hand I am not sure that the subjects of exophthalmic goitre are always so emotive as they have been supposed to be. On one occasion, when I was holding a small glass vessel, containing ether, in my hand and was standing by the bedside of a woman with Graves's disease, the ether unfortunately caught light from a neighbouring spirit lamp. As a result there was a column of fire, for a moment, reaching up to the ceiling, but the patient did not seem to be specially alarmed.

Pityriasis Rubra Pilaris (Generalized Form).

By F. Parkes Weber, M.D.

The patient, M. L., aged 54, a strongly-built man, is a typical example of generalized pityriasis rubra pilaris (Devergie), the "lichen ruber acuminatus" of Kaposi. The disease commenced on October 27, 1919, with itching all over his body. On the previous night he had partaken of sardines and brandy. During the two preceding years he had suffered a good deal from dyspeptic pains. The eruption is of the characteristic grater or goose-skin type on the trunk and upper extremities, and there is diffuse keratosis on the palms of the hands and on the feet and legs. On the scalp the eruption has a somewhat seborrhœic appearance. The patient is apparently slightly improving. He has lately been treated by thyroid extract, liq. arsenicalis and an alkaline sodium salicylate mixture; with bran baths for the pruritus.

I wish to point out that this rather rare disease might be diagnosed in the present case by a doctor with his eyes blindfolded—merely by feeling the follicular papules on the backs of the proximal segments of the fingers. I have just taken an impression, with ordinary printer's ink on white blotting-paper, from the back of one of the patient's hands. The papules about the hair-follicles are very strikingly shown in this way.

Duodenal Ulcer, Gastrojejunostomy, Gastrojejuno-colic Fistula, Excision of Ulcer and Entero-anastomosis; Recovery.

By R. P. Rowlands, M.S.

Man, aged 24. I saw the patient four and a half years ago in the medical wards at Guy's Hospital with Dr. Hurst. He was then in a bad condition, was moderately thin and somewhat pale. His history was, roughly, that he had had a gastrojejunostomy performed over two years earlier for duodenal ulcer. He was never quite relieved but was in fair health until about two years later, when he had an attack of acute suppurative appendicitis, and had the appendix removed. Not very long after this he began to have diarrhœa, the bowels being opened about six or seven times a day and often at night. At times the diarrhœa abated, or even ceased for days and sometimes for weeks at a time, but he was never well or able to do much. He often brought

up very offensive gas, and occasionally vomited foul material. Dr. Hurst examined him with the X-rays, and found that the opening in the stomach was much too high and to the left. Also there was much delay in the exit of food from the dependent pouch of stomach below, unless the patient was lying on his left side with the pelvis raised. Dr. Ryffel examined the contents of the stomach after a barium enema had been administered, and found barium sulphide in the contents. The X-ray examination after a bismuth meal and barium enema had not revealed any fistula between the colon and the stomach, but judging from the history of diarrhœa and the chemical examination I concluded that he had a fistula from the gastrojejunostomy to the transverse colon to the left of the middle line, and I advised an operation.

Operation on November 9, 1915: A fistula between the gastrojejunostomy and the transverse colon was found. It admitted a finger easily. It was well to the left of the middle line, the opening being anterior to and to the left of the gastrojejunal opening. I divided this fistula, pared the edges and sewed the resulting wound in the colon in the transverse direction. I treated the opening into the gastrojejunostomy in a similar way, after excising the ulcer the size of a shilling, and first passing my finger through the rent and feeling it enter the stomach; also feeling a sharp edge to the right of my finger, being the spur to the left of the dependent pouch of the stomach, seen with the X-rays. The proximal jejunum was 8 in. long and enormously distended. There was very marked kinking with definite intestinal obstruction at the anastomosis. I joined the two limbs of the loop 3 in. below the stomach, making a 3 in. anastomosis. Catgut was used for the deep suture and linen thread for the superficial.

He has kept fairly well ever since and has led an active life, playing football and taking active exercise. He was not accepted for military service, being clearly unfit for the strain.

Remarks.

Gastrojejunal fistula follows about 1·5 per cent. of gastrojejunostomies. Two-thirds of the ulcers are jejunal and within 3 or 4 in. of the anastomosis. The remaining one-third are gastrojejunal occurring at the anastomotic ring.

Gastrojejuno-colic fistula is a much rarer thing, and I have only operated on three cases. It is a very grave condition, and this patient is fortunate to have completely recovered from it and to be well four and a half years later. My other two cases may be briefly mentioned.

Both were very thin and ill, having suffered from diarrhœa and vomiting for months. In one of them I performed an anterior gastrojejunostomy under gas and oxygen anæsthesia, meaning to excise the original anastomosis and fistula at a later date if the patient's condition improved sufficiently, but he got weaker and died about a month later. The other patient was of unusual interest. He had remained well for seven years after gastrojejunostomy but developed a gastro-jejuno-colic fistula when on active service at the Dardanelles. When I saw him at Guy's Hospital he was suffering from intestinal obstruction in addition to his other troubles. At the operation this proved to be due to kinking at a gastrojejuno-colic-ileac fistula. I overcame the obstruction by entero-anastomosis of the ileum, hoping at a later date to do a radical operation on the complicated fistulæ, but he got thinner and weaker and clearly could not stand such a severe operation, and preferred not to take the risk of it and went home to die.

The cause of gastrojejunal ulceration appears to be hyperacidity of the gastric secretion acting on the mucous membrane of the jejunum, especially where the latter is not bathed with alkaline secretions. It is, therefore, particularly liable to follow the Roux and the EN-Y operations. In nearly all cases there is some obstruction at the anastomosis, either due to kinking, torsion or compression of the jejunum. It appears to be more likely to follow when the opening is low in the jejunum.

The use of unabsorbable deep sutures is also an important cause. The after-treatment of gastrojejunostomy is also often at fault. We are too apt to think that the patient is quite well, as he often appears to be a month after the operation. It is a mistake to let him return to hard work and casual diet too soon. The war showed that patients are rarely quite normal after gastrojejunostomy and that it is unwise to submit them to the strain of abnormal diet and other conditions of active service. It is wise to tell patients to abstain from work for about three months after the operation and to be careful in their diet, especially avoiding acids, alcohol and hard or indigestible food. Also to give magnesium hydrate and olive oil regularly before meals for this period.

Treatment.

Medical treatment by careful diet, rest and neutralization of the gastric juice should always be thoroughly tried.

Surgical treatment of peptic ulcer of the jejunum is difficult, but when medical treatment has failed then some form of operation has to be tried.

(1) *Enlargement of the Anastomotic Opening.*—In some cases this is sufficient, and in exhausted patients it may be all that can be safely attempted.

(2) *Excision of the ulcer* is the most radical procedure, but may be a very severe and grave operation owing to extensive adhesions and the poor general condition of the patient. The ulcer having been excised, a new and more perfect gastrojejunostomy has to be made, unless the pylorus and duodenum are patent, when the openings in the stomach and jejunum may be closed with advantage in many cases.

(3) *The addition of another gastrojejunostomy*, generally anterior, without excision of the ulcer is not likely to be of any permanent value.

The surgery of *gastrojejuno-colic fistula* is extremely difficult, for nothing short of excision of the fistulæ and ulceration involving three viscera, is likely to be successful. In my case shown to-day, excision of the fistula and ulcer has proved most satisfactory, but the ulcer was not a very large one, and only occupied the left side of the anastomotic ring. Here it was possible to close the resulting wounds in the colon and anastomosis without narrowing the lumen of either, the suture line in the colon being transverse to its axis and 2 in. long, that at the anastomosis being circumferential. It is clear that sewing in the opposite direction would have narrowed the channels. It was still necessary, however, to overcome the obstruction in the jejunum at the anastomosis, which was due to kinking, by performing entero-anastomosis 3 in. below the gastrojejunostomy.

Bilateral Destruction of Gasserian Ganglia by Alcohol Injection for Chronic Trigeminal Neuralgia.

By Wilfred Harris, M.D.

Mrs. M. H., aged 49, began to suffer fifteen years ago from spasmodic pains on the right side of her face, commencing in the upper jaw, and spreading into the lower jaw and temple. At first she used to get remissions of the pain, perhaps for a few months, but the spasms became much more frequent and severe in the latter part of 1912, attacks of pain occurring every five minutes, keeping her awake. With the neuralgic spasms she used also to have well-marked motor right facial spasm, with the exception of the aural muscles and the

platysma, and her habit was to rub the skin of the right side of her face during the paroxysms of pain, so that she had rendered the skin of the lower portion of the cheek and the chin on the right side almost horny, and had rubbed away the outer half of the right eyebrow. Early in 1913 she was admitted into the National Hospital under the late Dr. Batten. She then had no teeth in either right jaw, and the sensation of the face was perfect, with no true hyperæsthesia, though she was afraid of light touches starting the painful spasms.

On February 9, 1913, at Dr. Batten's request, I injected the right foramen ovale, under chloroform, by the usual route beneath the zygoma, using $1\frac{3}{4}$ c.c. of 80 per cent. alcohol. As a result, she had complete right fifth nerve paralysis, motor and sensory, with loss of taste on the right side of the tongue, anterior two-thirds. Slight keratitis developed after a week, so that Mr. Paton sewed up her eyelids for six months. After the eyelids were opened again, the cornea has remained perfectly healthy. There has been no return of any neuralgia on the right side, and the anæsthesia of the right fifth has remained permanent and complete to all forms, including pressure, and also for taste. The motor root, however, recovered, so that the right masseter, pterygoids, &c., recovered well.

Five and a half years later, in August, 1918, she came again to see me, with a recurrence of neuralgia on the *left* side, in the cheek and upper jaw. She stated that she had had a queer feeling in the cheek for some months, but no pain until July, when a severe twinge seized her in the upper gum, spreading into the temple and side of the nose; and walking or eating or even a touch on the lower lip, would start it. Examination showed the right fifth area totally anæsthetic, with right-sided loss of taste complete as before, though the right masticatory muscles were now fully recovered. I then injected her left foramen rotundum with 90 per cent. alcohol, using the route in front of the coronary process, behind the antrum, and through the pterygo-maxillary fissure. Fairly deep anæsthesia of the second division of the left fifth was produced, and she was completely free of the neuralgia until November, 1919, when it recurred, after sixteen months. She came to see me again on December 10, 1919, and I then decided to destroy her left Gasserian ganglion by alcohol injection, so as to give her permanent relief. This was a feasible operation, owing to the recovery of the right masticatory muscles, though bilateral gasserectomy is an impossible operation for the surgeon, owing to the permanent jaw-drop which would be produced from the destruction of the motor branches of the two fifth nerves.

On December 10 I therefore, using novocain anæsthesia only, injected the left Gasserian ganglion with 90 per cent. alcohol, with resultant total left fifth nerve palsy, motor and sensory. As my present custom is, I closed her left eyelids with strapping at once, and with washing out the conjunctival sac twice daily for ten days, her cornea has remained clear, and is now quite healthy. There is, therefore, now present total anæsthesia of the whole of the front half of the head and face to all forms of sensation, including pressure, owing to the bilateral ganglion destruction, and she can taste nothing at all until she begins to swallow, when she appreciates taste quite keenly. On testing the sense of taste carefully, there is no perception of taste whatever on the front portion of the tongue, but she at once recognizes salt, sweet or bitter, with solutions placed on the back of the tongue or fauces.

This case, therefore, is a good illustration of the undoubted fact that in the majority of subjects *taste* sensations are conveyed to the pons from the anterior two-thirds of the tongue by the fifth nerve, and not by the pars intermedia Wrisbergi, the so-called central prolongation of the chorda tympani.

Case of (?) Polyneuritis Cranialis.

By S. A. Kinnier Wilson, M.D.

Patient, W. J., a male, aged 50.

History: September 17, 1918—*Pain* in the right ear and tenderness over mastoid. Examination of tympanic membrane negative. Hearing normal. Two weeks later—*Broncho-pneumonia*, with unconsciousness some days. On recovering consciousness he found that the pain had disappeared, and he had the following symptoms: *Right ear, deaf; right face, paralysed* and *dead; unable to swallow* (nasally fed for eight weeks). *Headache* in parietal region; occasional vomiting after food. *Diplopia*—good vision otherwise. *Aching* and *wasting of right shoulder*. *Aphonia*—could only whisper. Since then swallowing has improved, but he still gets occasional regurgitation of fluids through the nose, and cannot swallow hard solids. Phonation has improved, but is not normal. Headache occasional, bitemporal; vision has deteriorated, specially in the right eye. Diplopia has improved. Sphincters: Occasional frequency and weakness of control. No ictus at any time.

Past history: Dysentery and nephritis in Gallipoli.

Present Neurological State.—Cerebration normal. Special senses: *Smell* defective, right > left. *Taste:* No recognition right nor left. *Hearing:* Middle-ear deafness, right ear. (Some otorrhœa.) *Vision:* Fields normal, discs physiological; acuity, left $\frac{6}{6}$, right $\frac{6}{12}$.

Cranial nerves: (5) Hypo-æsthesia to touch and pain, middle division right, 5. (6) Weakness: right external rectus, with diplopia on looking to right. (7) Complete right paralysis, with wasting. (9) Paralysis of right constrictors of pharynx: "curtain movement" of pharynx. (10) Right hemipalsy of larynx; defective phonation; paralysis right half of palate. (11) Paralysis and wasting; right sternomastoid and trapezius. (12) Paralysis, wasting, and fibrillation; right half of tongue; paresis, slight wasting of left half. Dysarthria: rest of motor and sensory systems normal. Reflexes normal.

Cases of Symptomatic Paralysis Agitans following Encephalitis Lethargica.

By S. A. Kinnier Wilson, M.D.

Case I.

Patient, S. N., a female.

Family history: One brother had influenza three weeks before patient.

History: February, 1919, influenza. One week later nocturnal delirium, followed in the morning by diplopia and headache, stiffness and weakness in arms and legs. Two days after this *ptosis*, lasting three weeks, and *lethargy*, lasting two months, i.e., "she fell asleep after everything." Memory good for events that happened during this state. Rigidity began while she was in bed, and increased for a time after she got up. She could neither eat nor walk quickly. Involuntary movements of the jaw and left arm occurred during the early part of the illness. Those in the jaw persisted.

Present state: On admission, December 17, 1919, cerebration and memory perfectly normal; phonation weak and monotonous; articulation slow; special senses normal. Cranial nerves: Central, circular pupils: spontaneous nystagmoid movements; slow punctate lateral deviation of the eyes; heavy-lided; no diplopia. (5) Slight trismus: involuntary "chewing-the-cud" movement of the jaw. No sensory loss. (7) Symmetrical immobility of the face; spastic smile. (12) Spasticity of the

tongue. Sensation: No sensory loss. Motor system: General rigidity; upper limbs maintained in the attitude of a Parkinson's disease; no involuntary movements, but slight tremor of the right hand; gait and general attitude (propulsion, &c.) that of marked Parkinson's disease. Reflexes: Superficial, normal; deep knee-jerks obtained with great difficulty; ankle-jerks absent.

CASE II.

PATIENT, A. R., a male, developed an attack of encephalitis lethargica in December, 1918. He was in Guy's Hospital for four months with it. The acute symptoms of the encephalitis gradually disappeared, and the patient's condition became as it is seen at present—a highly typical symptomatic paralysis agitans.

Case of Nerve Disease for Diagnosis.

By J. WALTER CARR, M.D., and Miss MCKEOWN, M.B.

PATIENT is a boy, aged 8. Present illness began insidiously in June, 1919, with unsteadiness of the legs in walking and some loss of strength in the left arm. He has been getting gradually worse since. No headache nor vomiting. He is now unable to stand or walk on account of persistent tonic spasm of the right leg, leading to flexion of the hip and thigh, and extreme extension and inversion of the foot, which is in the position of pes cavus. The left leg shows a much slighter degree of spasm. The spasm persists during sleep, relaxes under anæsthesia, but returns before consciousness is regained. The knee-jerks are brisk; the plantar reflexes are difficult to obtain, but are probably flexor. In the left arm there is some muscular spasm (increased by attention) and slight weakness; the boy does not use it much. The right arm appears to be normal. There is some weakness of the back muscles, with a tendency to lateral curvature of the spine.

The tongue cannot be protruded for more than about ½ in., and speech is very blurred and indistinct. Sensation probably unaffected. The boy has lately had occasional loss of control over the sphincters, and his intelligence is perhaps not quite up to the average for his age. No ophthalmoscopic changes. Wassermann reaction negative

both in blood and cerebrospinal fluid; the latter fluid is normal in all respects.

The case is probably one of cerebral diplegia starting later in life than usual.

Case of Periosteal Sarcoma of the Clavicle; Clavicle completely removed Two Years ago; no Recurrence; Unimpaired Function.

By Alan H. Todd, M.S.

P. J. H., tin box manufacturer, now aged 32, was a private in the Bucks Hussars at the beginning of the War. At rifle-drill, he noticed a pain in his left clavicle, which he attributed to constantly bringing the rifle up against the bone. After a while, he transferred to the R.F.C., where he became a drill-sergeant; in training recruits, he had to be constantly doing rifle-drill, and his left clavicle became so tender that for a while he took to bringing the rifle up to the right shoulder instead of the left. Next, he was sent to France; the pain continued although he had now discontinued rifle-drill, and eventually he had to "go sick" with it. He was sent to a base hospital, where, in spite of a negative history and Wassermann reaction, he was diagnosed as a case of syphilitic periostitis, and treated for three months with Hg pills and for four or five months with KI, without improvement of any kind.

Early in 1918, he came under my care at Lewisham Military Hospital, with a fusiform swelling of the shaft of the left clavicle (fig. 1). A further Wassermann test proved negative, and the X-ray appearances (fig. 2) and clinical characters were those of periosteal sarcoma of the bone. A piece was removed for histological examination, and this showed the typical structure of a mixed-cell sarcoma, with spindle-cells predominating.

In February, 1918, I removed the whole bone, the soft parts being excised rather freely towards its inner end, as here the growth was just beginning to break loose into the tissues. The sternomastoid was sewn as well as possible to the remaining soft parts around, and the deltoid and trapezius, having been detached from the clavicle, were sutured to one another. The wound was drained for three days, and united by first intention. There was a dragging pain at the nape of the neck for

about a month afterwards, due, no doubt, to the fact that the deltoid had practically been given a new origin from the cervical and upper dorsal spine, and that the weight of the limb was being felt there; this pain,



FIG. 1.
Before operation.

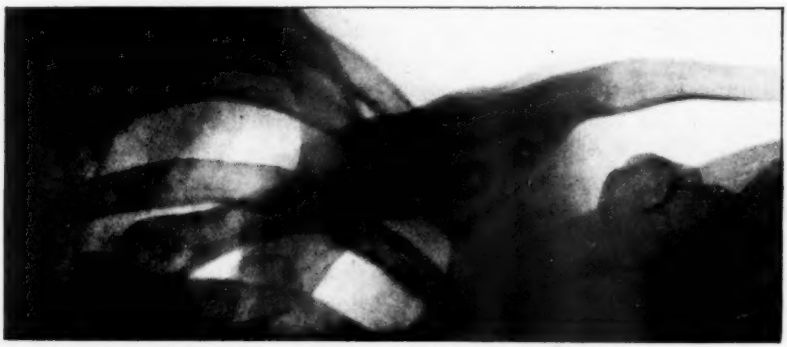


FIG. 2.
X-ray appearances before operation.

however, gradually disappeared, and no untoward symptoms have appeared since.

There seem to be no signs of recurrence. A few shotty glands can be felt in the left side of the neck, but these were present at the time of

operation, and there are similar ones to be felt in the axillæ and in both groins. They do not seem to be sarcomatous, and are probably due to slight chronic dental sepsis. The patient's left shoulder droops a little, but he says that he has no disability from the loss of his clavicle; he can swing his arm round, and lift it above his head quite well; he can pull, push, lift, and carry, very well indeed (fig. 3).



FIG. 3.
After operation.

DISCUSSION.

Mr. TODD: The main interest of the case lies in the surprisingly small disturbance of function that the patient suffers: one might almost say that he suffers none at all. Cases of complete extirpation of the clavicle are most uncommon, but after seeing this one, no one would hesitate, I think, to remove the whole bone if there appeared to be good grounds for doing so. In this particular instance, I never had a moment's doubt that it was the right thing to do, in view of the mortal character of the man's disease, but I naturally speculated somewhat, beforehand, as to the degree of impairment of function that would ensue. Lieutenant-Colonel Toogood, who was then O.C. at Lewisham Military Hospital, pointed out to me that a dog got along very well without a collar-bone, and that encouraged me very much! I attach great importance to the suturing of the supra- and infra-clavicular muscles to one another. During the exhibition of the cases Lieutenant-General Sir T. Goodwin remarked to me that of all the ordinary wild animals in India, the

only one that had a clavicle of any size and utility was the monkey, and that this fact was commonly supposed to be due to the fact that the monkey had a lot of climbing to do. So we took my patient, and tried him, to see if he could hang on to an overhead bar quite well, without his clavicle, and found that he could.

The PRESIDENT: The functional result is certainly excellent.

Mr. TODD: The disability is infinitely less than that of, say, an ankylosed acromio-clavicular joint.

Case of Malignant Endocarditis affecting the Pulmonary Valve in a Youth, aged 16½.

By H. BATTY SHAW, M.D.

(Shown by Dr. F. P. N. PARSONS.)

W. Y. WAS admitted to University College Hospital with the history that he had been troubled with "heart complaint" for two and a half years, his symptoms being palpitation and shortness of breath on effort. Previous to that he had no illness of any kind. He was admitted to hospital on December 3, 1919, with the history that he had developed pain over the heart during the previous four days. His mother is said to have had consumption, but is well again, and his father and brother and two sisters are quite well.

Whilst in the hospital he has had fever of a remittent and intermittent character almost continuously, the maximum being 104° F.; the pulse varies from normal to 120 and the respiration from 24 to 30. He has always been of pale complexion, and during his stay in the hospital has steadily lost weight and is now 2½ st. under weight. The blood examination shows secondary anæmia and 5,500 leucocytes per cubic millimetre, the polymorphonuclear cells numbering 75 per cent.: cultures made from the blood on two occasions have shown the presence of a *Streptococcus longus*. The spleen is enlarged and tender; blood has been found in the urine on one occasion and there are pain and some râles and rhonchi in the right axilla. A loud systolic murmur is heard over the pulmonary area, and there is a distinct systolic thrill in the same area, conducted towards the left clavicle. From the facts that cardiac symptoms have only been noted during the last two and a half years, and that there is no cyanosis nor clubbing

of the fingers, the conclusion is drawn that the narrowing of the entrance of the pulmonary artery is due to malignant endocarditis of the valves, and that the case is not one of malignant endocarditis developing on valves damaged by congenital heart disease.

DISCUSSION.

Dr. F. PARKES WEBER: I would nevertheless suggest, in spite of Dr. Batty Shaw's remarks, that the malignant endocarditis may be engrafted on some congenital defect of the pulmonary orifice. Perhaps there are four semilunar cusps to the valve or only two cusps, as in Sir Dyce Duckworth's case.[1] Sometimes a kind of frænum or "cross-bar," as described by John Hunter, separates the bigger of the two semilunar cusps into two portions, suggesting that the bigger cusp is formed by the union of two cusps as a result of intra-uterine endocarditis.[2]

Sir JAMES GALLOWAY: In view of the unusual feature presented by Dr. Batty Shaw's case, I presume that one of the difficulties he feels is how to explain the origin of the infection producing endocarditis affecting the pulmonary valve and the right side of the heart. The cases are rare but have been noted and reported as occurring from the time of early clinical investigators of heart disease, such as the late Dr. Peacock, and others onwards. I recollect vividly an example in my own experience, when I was assistant physician and pathologist at the Great Northern Hospital. I had the opportunity of seeing a boy in the wards who suffered from a serious febrile condition difficult of diagnosis, which was ultimately considered to be a case of infective endocarditis. This occurred before the days when trustworthy bacteriological blood examinations could be made. I had the opportunity of making the post-mortem examination in this case later, and there was no doubt about the endocarditis; it was of "malignant" or infective nature, involving the right side of the heart and affecting the base of the tricuspid valves, and it produced a mural abscess and perforation between the right and left auricles. The origin of the infection was not evident, but on a little investigation I found a patch of periostitis on the right tibia with pus present, and also a small infective lesion on one of the toes. No doubt could remain that the right-sided cardiac disease in this case originated in pyogenic infection from these lesions in the right lower extremity. I have had an opportunity recently of seeing a very similar case of infective endocarditis of the right side of the heart in the case of a young child in Charing Cross Hospital.

[1] *Trans. Clin. Soc. Lond.*, 1888, xxi, p. 18.

[2] I had unfortunately not examined the case at the meeting, but by the kindness of Dr. Batty Shaw I was able to do so on the following morning. Owing to the rough murmur and thrill and (probably) great hypertrophy of the right side of the heart (and dilatation of the right auricle), I believe the case was originally one of congenital pulmonary stenosis.– F. P. W.

Clinical Section.

President—Sir ANTHONY BOWLBY, K.C.B., K.C.M.G., K.C.V.O.

Traumatic Aneurysmal Varix between the Cavernous Sinus and the End of the Internal Carotid Artery on the Left Side.[1]

By W. G. SPENCER, F.R.C.S.

W. P., AGED 26, whilst on duty in France with the Army Service Corps, was kicked by a horse upon the left side of the head on February 6, 1918. He was unconscious for a time but did not bleed from his ear or nose. After recovering consciousness he continued mentally dull, occasionally vomited and complained of headache. On February 20, clear fluid under excess of pressure was drawn off by lumbar puncture. An increasing œdema of the eyelids was noted on March 4, but the noise in the head was apparently overlooked. He was transferred to King George Hospital on March 4, with the diagnosis of fractured base of the skull. No confirmation of this diagnosis is forthcoming unless it be the paralysis of the left sixth nerve; no sign of fracture could be found by X-ray examination.

The most obvious feature was the venous engorgement of both upper eyelids, especially the left, also the proptosis most marked on the left. There was chemosis of the ocular conjunctiva of the left eye and paralysis of the left external rectus. The man complained of a continuous rushing noise in the head on the left side. A loud systolic blowing noise or souffle was heard over the forehead; it was loudest over the upper eyelids and eyeballs. It could be heard more faintly over the left temporal region, the face, and along the line of the carotids in the neck. When the left carotid was compressed the souffle was not heard (fig. 1).

The left common carotid artery was ligatured at the level of the

[1] At a meeting of the Section, held March 12, 1920.

cricoid cartilage on March 28—i.e., seven weeks after the accident. On tightening the ligature the souffle disappeared, but was soon heard again at the root of the nose and more faintly over the upper eyelids. The venous engorgement of the eyelids rapidly diminished as shown in the photograph taken ten days after the ligature (fig. 2).

Mr. Lang reported that there was no optic neuritis; there was complete and permanent paralysis of the external rectus.

The man has been under observation and has only been able to do light work. Headaches have been very variable. The rushing noise in his head gradually returned; when he pressed his left eyeball outwards the noise seemed to stop for the moment.

On admission to the Westminster Hospital at the beginning of this year the souffle was heard at the root of the nose, and less loudly over the forehead. It could also be heard, but more faintly in the right infra-orbital region, not, however, over the left temporal and facial region as before the first operation. The right carotid artery was ligatured on January 13, twenty-two and a half months after the left. Before tightening the ligatures his head was lowered to the level of the trunk and he was let out somewhat from the anæsthetic. The tightening of the ligatures caused deep anæsthesia with stertorous breathing and contracted pupils. He was kept in bed with the head horizontal; he fainted several times when being moved or dressed, and did not get up for six weeks. The ligatures used were of kangaroo tendon, and were applied so as to compress the arteries according to the method of Ballance and Edmunds.

Since the second operation he has not heard a noise unless his head is sideways on the pillow. He does not hear it even in the quiet of a night, if he is lying on his back. He complains of headache at irregular times and the usual remedies do not relieve him.

I do not know of any material additions to the subject since Rivington's paper in 1875.[1] I presume that in this case there is a pinhole communication between the left cavernous sinus and the dilated end of the internal carotid. The anterior communicating and the intracranial part of the ophthalmic arteries may also be dilated. There is no positive sign of any aneurysmal sac formation, apart from the arterial dilatation, nor any sign of thrombosis of the cavernous sinus. I hope that the bilateral ligation will control a tendency to further arterial dilatation; the most marked effect has been the relief to compression of the left cavernous sinus.

[1] Rivington, *Trans. Med. Chir. Soc.*, 1875, lviii, p. 256.

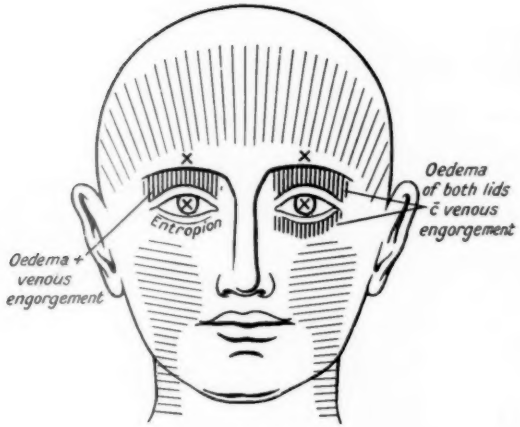


FIG. 1.

Drawing made before the ligature of the left carotid. Shaded area indicates where "souffle" was heard; also heard over vessels of neck on either side. X indicates the points where the "souffle" was heard loudest over upper eyelids and eyeballs.



FIG. 2.

Ten days after the ligature of the left carotid.

Microcephaly in Two Sisters, Children of First Cousins, exhibited shortly after Craniectomy.

By W. G. Spencer, F.R.C.S.

The parents are both perfectly healthy, aged between 20 and 25, and except that they are first cousins there is nothing in their personal or family history which bears on the condition of their children.

The children have been well cared for by the mother whilst the father was away in the Army, and when brought to the hospital were in the same general state of nutrition as at present. They were admitted to the Westminster Hospital under Dr. de Souza, and after due examination and observation the parents were informed that presumably the children would continue idiots. They then asked for something to be tried and were referred to me. I informed them of Horsley's first case, including the note in Mr. Stephen Paget's "Life of Sir Victor Horsley," in which the father reported of his son, whom before the operation he had considered an idiot. His son had grown up, had become an officer, had served in the trenches at the front and had received a decoration. After considering the matter, the parents asked that the children might be operated upon.

The children, one aged 3, and the other 1½, are in good general health, they have bright eyes and a clear complexion and skin. Except for the calvarium, deficient hair, and general dwarfing, they present no deformity. They have all their teeth and these are practically perfect. The development of the calvarium has been arrested, so as to give them the repellent aspect of anthropoidal apes (figs. 1 and 2).

I am much indebted to Miss S. Glover, the sister of the ward, for instituting a comparison between the patients and normal children of their age.

(1) Jessie K., aged 3; weight, 22½ lb. A normal child of 3, can walk, talk, and feed itself; is clean in its habits; can play and amuse itself; is good tempered; has plenty of hair on its head; is obedient. Jessie is unable to walk more than a few steps: she cannot feed herself; is dirty in her habits; can say nothing except "bad"; can only sit up, toss her arms and fumble with toys and does not otherwise amuse herself; has violent fits of temper, in which she bites herself and throws herself backwards so as to strike the back of her head. Her hair is very scanty.



FIG. 1.



FIG. 2.

(2) Joan K., aged 1½; weight 13½ lb. A normal child of 18 months can generally walk; can say most common words; if well trained is clean in its habits; plays and amuses itself; is obedient; has some hair on its head. Joan is unable to walk or stand; can only say "bad," "mum"; is dirty in her habits; does not play or amuse herself, merely tosses her arms; is uncertain in temper; is unable to feed herself; has no idea of obedience; has no hair on her head.

The operation was performed on the elder child at one sitting and caused no perceptible shock. On the younger child, the operation was done in two stages at a fortnight's interval. There was a little shock after the first, and more after the second stage. The operation consisted in making an incision over the sagittal suture with a short horizontal cut at each end. Then holes were bored in the frontal bone just in front of the coronal suture, one on each side, and the same in the occipital bone behind the lambdoid suture. By means of a circular saw and rongeur forceps a strip of bone was removed from each parietal bone, about 3 cm. in breadth, but tapering at the ends, so as to join the hole in the frontal bone with that in the occipital, thus cutting across both the coronal and lambdoid sutures on each side. After this a strip of bone about 1 cm. in width was removed between the two holes in the frontal bone, a little in front of the coronal suture.

Both skulls were thin, like those of a baby under a year. The lines of the sutures were occupied by ridges of soft bone. There was nothing abnormal about the exposed dura mater. There was no hæmorrhage from the bone to need wax. The intracranial pressure was not above normal.

The children are now in the same state as before the operation; the gaps made in the skull show no sign of intracranial tension. In the region of the left parietal eminence of the younger child, the brain is felt protruding and pulsating under the scalp. Elsewhere the saw was held so that it did not penetrate further than the vitreous table; at this spot it must have scratched the dura, for a week after the second stage of the craniectomy, there was a discharge of cerebro-spinal fluid for a few days. At any rate, the protrusion confirms the view that there is no abnormal intracranial tension. I propose to include the name and address of the parents. Perhaps years hence some Fellow may be moved to inquire into the fate of the two children.

Parents: Mr. and Mrs. Kilgom, 1, Albion Street, Stockton-on-Tees.

Hysterical Anæsthesia of the Eyeballs; Cutaneous Artefacts; Voluntary Regurgitation of Stomach Contents.

By Hildred Carlill, M.D.

Miss W. F. was sent to me by Dr. D. S. Jackson, of Catford, on February 24, 1920. Her parents are well. Of four brothers one was killed and another is said to have phthisis; the others are well. Her only sister died of phthisis and the patient's remarkable condition probably dates from the emotional occurrences which were associated with this event. She has never been out of this country and appears to have led an uneventful life. Six years ago her engagement was broken off.

She was well up to early in 1914, when she had abdominal pain and diarrhœa associated with mucus in the stools. During 1914, 1915, 1917 and 1918 she was an in-patient at Guy's, King's College Hospital, and the London Hospital. She was at one time treated for typhoid fever and she has also had her stomach examined under the X-rays. Vomiting appears to have attracted considerable attention in 1915, for she had the stomach washed out and charcoal administered. Silver nitrate injections were employed and olive oil enemata.

In 1919 she had her appendix removed at the Royal Free Hospital, on account as she says of the persistent vomiting and abdominal discomfort. For a month she was better but the vomiting recurred and since December, 1919, she has "vomited after every meal." The bowels are regular, the menses frequent; there is no trouble in swallowing. She states that she is happy at home but admits that she obtains a great deal of sympathy. Her appetite is good.

Directly I heard her story I told her that if it was correct there would be no difficulty in curing her. She was well nourished and her appearance did not suggest organic disease. Moreover her mother now told me that the vomiting occurred only after the end of the meal and was limited in amount. Directly I gave her food and witnessed the vomiting it was obvious that it was controlled. The abdomen was stained by a large belladonna plaster. There was no evidence of organic disease, no retching and no flatulence. The girl was somewhat emotional but otherwise placid. The lower limbs were covered with circular scars, superficial and deeply pigmented. There were twelve on the right side and ten on the left. The calves were spared. These

scars were artefacts: the patient said that they started as ulcers three months ago (*see* figure).

The patient also told us that for three months past she could rub her fingers across her eyeballs without feeling the touch, and Dr. Jackson had verified this. She says that she discovered this for herself when looking in the mirror. The ocular conjunctiva was



To illustrate (1) Hysterical anæsthesia of globes; (2) Voluntary regurgitation of gastric contents; (3) Cutaneous artefacts.

insensitive to any stimulus and at times the cornea could be touched and rubbed without pain or feeling. As regards the conjunctiva she showed a gross exaggeration of the normal comparative lack of acute sensibility. The corneal reflex was present at all times, the conjunctival sometimes. The palpebral conjunctiva was acutely sensitive and the

eyes and eyelids were moist and free from foreign bodies. A pencil or a brush could be placed across either globe and the lids brought together over it without the patient being aware that anything unusual was taking place. The discs, visual fields and acuity were normal. The superficial reflexes otherwise were healthy, also the tendon-jerks, skin reflexes and visceral reflexes. There was no sensory abnormality apart from that of the conjunctiva.

The patient was admitted to Westminster Hospital under my care. As regards the vomiting it was found that this consisted of a voluntary regurgitation of gastric contents, sometimes fast and sometimes slow, but always at the bidding of herself or of the physician. The act could be produced some time after a meal. The ejected food was acid and there was no explosive character about the act. If it was inconvenient to expel the food after it reached the mouth she would swallow it again. The whole affair was a quiet and orderly acquired merycism and there was no distress. She told us that no physician had ever witnessed her vomit until she came into Westminster Hospital. [A demonstration of immediate and delayed vomiting was given at the meeting.]

The diagnosis was hysterical anæsthesia of the eyeballs, voluntary regurgitation of the gastric contents supervening probably upon hysterical vomiting, and self-inflicted injury to the legs. The disability of stomach and skin was in each case the result of voluntary action, produced by an hysterical patient and not with the ulterior motives which are associated with the behaviour of a malingerer.

The patient was persuaded to confess that the skin lesions were caused by a penknife. The vomiting ceased directly she was told why she vomited and that it would cease. Persuasion and suggestion, aided by the use of a faradic brush to the eyes and eyelids, failed to cure the anæsthetic state of the eyeballs directly, but it was found that they were normal a few days later. She told us that they had recovered and we confirmed her statement. She denied that she had exercised any voluntary control over her sensation, but she admitted that the vomiting was controlled.

In a written communication the girl stated that she planned to make herself ill in the first instance because she understood that grave illness was associated with an ability to communicate with the dead, and she wished to do so with her sister whom she had seen when she lay dead. For this reason she made no attempt to check the early vomiting. Apart from what has been described the girl may be said to be mentally normal and she has improved in every way since her secrets have been wrested from her.

Dr. F. PARKES WEBER: The case seems to me to be an example of acquired merycism or human rumination. The patient has acquired the power of bringing food from the stomach back into the mouth, and she can then either swallow it again or expel it. If she expels it, the act constitutes a kind of "voluntary vomiting," which she can make use of to excite sympathy or interest in her case. (Dr. Carlill has said that she has likewise produced skin trouble on her legs.) With regard to the eyes, I found that when I touched the cornea she closed the eyelids (corneal reflex) sharply.

Postscript.—July 20, 1920. The girl remains cured.

Clinical Section.

President—Sir ANTHONY BOWLBY, K.C.B., K.C.M.G., K.C.V.O.

Acute Osteo-myelitis of a Lumbar Vertebra.[1]

By ARTHUR EVANS, M.S.

THE patient, a boy aged 16½, left off work on October 21, 1919, feeling unwell. Previous to this he had had three boils over the sacral region. Two days later he complained of headache, and pain in the legs and lower part of his back; he began to have great difficulty in micturition, and complained of pain in his "stomach." There was no history of vomiting or of a sore throat.

When seen on November 13, 1919, the boy was complaining of great pain in his lumbar and sacral regions and in both legs. For ten days previously he had been running a temperature which had risen to 100°, 101° or 102° F. daily. The cranial nerves were normal. Abdominal reflexes present. Knee-jerks absent. Tendo Achillis jerk present. Plantar reflexes, flexor. Right leg: Could flex and extend knee and ankle (these movements were all feeble). Left leg: Could flex but could not extend knee, could flex and extend ankle. Both limbs were wasted, especially the left quadriceps extensor cruris. There was no sensory loss. There was marked tenderness over the upper part of the sacrum, more on the right than on the left side; there was also great tenderness on pressure over both erector spinæ. The cerebro-spinal fluid obtained by lumbar puncture showed an excess of polymorphonuclear leucocytes; no organisms were seen. A tentative diagnosis of acute anterior poliomyelitis involving the third and fourth lumbar segments was made.

On November 19, the condition showing no improvement, an operation was performed. A median incision was made over the lumbar

[1] At a meeting of the Section, held April 16, 1920.

spines, and the muscles retracted. The third lumbar spine was found necrosed and loose, and from this site an abscess extended into the right and left erector spinæ. On exposing the vertebral canal it was found to contain thick yellow odourless pus. The vertebral canal was opened down to the sacrum. Pressure over the sacrum caused pus to well up out of the superior aperture of the sacral canal, so the sacral canal was opened along its whole length. The spinal canal throughout the whole length exposed was filled with necrotic tissue, covering over the dura and the cauda equina. Bismuth-iodoform paste was gently rubbed over the whole exposed surface and the wound closed, save where it gave exit to a gauze drain.

For a few days subsequent to the operation the patient had incontinence of fæces. Recovery was rapid. In two months the patient could walk perfectly well.

Actinomycosis of the Thoracic Wall.

By Arthur Evans, M.S.

The patient, a railway clerk, was admitted to Westminster Hospital on April 12, 1919. He first noticed pain and swelling in his left side and back, about ten weeks before admission. He gives a history of having been hit by a pole in the ribs, and he thinks that the present swelling is due to that. Previous health good. There was a swelling on the left side of the lower thoracic margin, involving apparently the seventh, eighth, ninth and tenth ribs, and extending from the middle line to the mid-axillary line. The left lower thoracic margin when grasped between one's fingers felt three times as thick as that of the right. There was tenderness on pressure over the tumour. An X-ray plate showed a shadow in the region of the tumour.

On April 14 the tumour was excised. Before this was done an incision was made through almost the entire thickness of the tumour, but no inflammatory focus was found. Looking upon the case as one of sarcoma, I excised the tumour widely; this necessitated the removal of portions of the eighth, ninth and tenth ribs, and the muscles of the abdominal wall adjoining. When this mass was removed, I deepened the exploratory incision which had been made into the tumour substance, to find immediately subjacent a narrow streak of pus containing yellow granules—suggestive of actinomycosis. The base of the large wound

was composed of peritoneum and pleura, both of which were unopened. As a substitute for the abdominal muscles two large silver filigrees were sutured into position between the peritoneum and the skin; they covered an area extending from the outer border of the left rectus to



Wire filigree repairing abdominal and thoracic walls, after wide excision of actinomycosis.

the mid-axillary line and from the rib margin to the level of the iliac crest.

About three months after the operation a nodule appeared posterior to the site of the original tumour. This broke down and a sinus formed

discharging the actinomyces. The patient was put on large doses of potassium iodide, and the swelling gradually disappeared.

Microscopically the fungus was demonstrated to be present.

DISCUSSION.

Mr. A. W. SHEEN: I should like to know whether the diagnosis of sarcoma was arrived at by macroscopic or microscopic examination of the tumour. The infection in the interior of the rib shown is interesting. Did it affect the other portions of ribs removed? Has Mr. Evans any views regarding the route of infection? The man is a railway clerk but he tells me his relatives are farmers and that he used to spend his holidays at home. The safety of the well acting filigree would seem to be imperilled by the sinuses which have formed since the operation.

Mr. ARTHUR EVANS (in reply): The diagnosis of sarcoma was made from the clinical features of the case. The surfaces exposed on making an incision into the tumour preliminary to its excision, confirmed this opinion, as this section just failed to open up the area which would have revealed the true nature of the tumour. The normal appearance of the pleura and of the peritoneum did not suggest that the ribs had been invaded from either the pleural or the peritoneal cavity.

Cystic Adenoma of the Bile-ducts.

By ARTHUR EVANS, M.S.

THE patient was a woman, who was admitted into Westminster Hospital on November 20, 1919. She was emaciated and looked ill, and was slightly jaundiced, with constant pain in the upper part of the abdomen. In the epigastrium, and continuous with the edge of the liver, there was a tense cystic tumour reaching down almost to the umbilicus.

On December 1, 1919, the abdomen was opened. Free fluid of a myxomatous nature was found in the peritoneal cavity. The tumour proved to be a large cyst attached to the under surface of the liver. Upon the cyst being opened, yellowish viscid fluid escaped. The examining finger felt masses of what were thought to be daughter-cysts adherent to the inner wall. These were detached with difficulty. The greater portion of the cyst wall was removed and a large drainage tube inserted.

Microscopical examination proved the tumour to be a multilocular cystic adenoma of the bile-ducts.

Fracture of the Left Humerus; Non-union (Pseudo-arthrosis) after Thirty-four Years.

By PHILIP TURNER, M.S.

D. C., AGED 53, presented himself for treatment as an out-patient about three weeks ago for a partial rupture of the right biceps. In the course of examination it was found that he had an ununited fracture of the

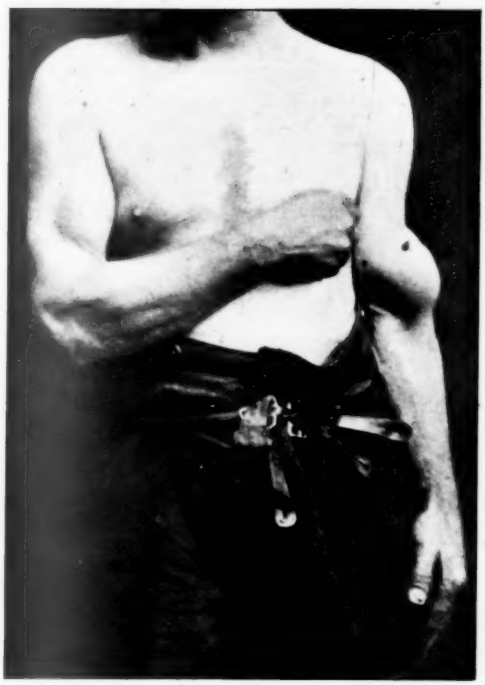


FIG. 1.

left humerus, the result of an injury received thirty-four years ago. The report of the old injury states that he was admitted to Guy's Hospital, under Mr. Bryant, in September, 1886, for injuries received as the result of a "stack of iron" falling on him. There was a compound fracture of the right tibia and fibula, a fracture of the shaft of the left humerus about

the junction of the middle and lower thirds, and a wound of the right hand with dorsal dislocation of all the first phalanges of the fingers. The fracture of the humerus was treated at first by an anterior angular splint with three small splints, later by a plaster of Paris splint and then, as this was not satisfactory, by an internal angular splint and small splints. Before discharge it was noted in the old hospital report, "Position better, much firmer union."

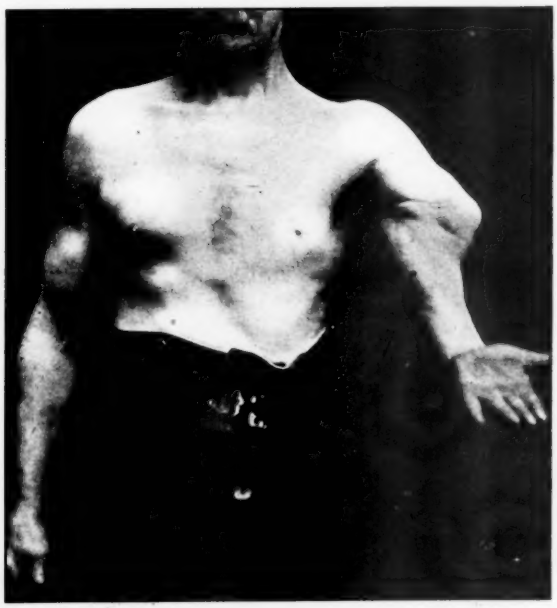


FIG. 2.

The results of the fracture of the tibia and fibula and of the injury to the hand are excellent, but the fracture of the humerus never united. Though at the present time there is a very marked deformity, the appearance superficially resembling that of a Charcot's disease of the elbow-joint, the functional result is remarkably good. The elbow-joint is not absolutely ankylosed, but flexion and extension of the forearm on the arm take place in the false joint in which, in addition to the normal movements, there is lateral mobility and also a certain amount of hyper-extension. The muscles of the forearm and arm are well developed

and there is a powerful grip. All the normal movements of the arm and forearm can be carried out with the exception that, owing to the lack of rigidity in the false joint, the arm cannot be raised above the level of the head (*see* figs. 1, 2, 3).

The patient says that the arm is strong, "but is not quite so strong as the other." He has continued since the accident in his original employment as a labourer at an ironworks. The work is very heavy



FIG. 3.

and includes lifting heavy girders. There is no pain in the joint and the patient complains of no disability.

The case is shown on account of the very long interval since the original injury, and to show that a strong, useful, and painless arm may result with an ununited fracture of the humerus.

Case of Tuberculous Spine; Tibial Bone-graft.

By A. W. Sheen, M.S.

The patient is a girl, from Breconshire, aged 22, who was operated on by me on July 16, 1914. Before operation the back had been "weak" for one year, and a curve noticed for six months. There was much pain and tenderness in the back, and a pronounced curve of the seventh, eighth and ninth dorsal spines. She could "hardly hold up," had got much thinner and could not do her work as a school teacher.

I grafted a 6-in. length of bone from the tibia into her back, removing the graft with a chisel and hammer. She remained in hospital until October 24, then rested at home for six months; then, after a holiday, resumed her school duties which she has carried on since. At present you see she is a fat, healthy girl; the back curve remains but is free from pain or from tenderness on most forcible treatment. Mr. Owen Ll. Rhys kindly radiographed her on April 9 last, and I show the plates. His report says, "the graft is visible as a long curved rod rounded at the ends." There can be no doubt that the graft has maintained its vitality.

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

JOHN BALE, SONS & DANIELSSON, Ltd., 83-91, Great Titchfield Street, London, W. 1.

Section of Dermatology.

President — Dr. Arthur Whitfield.

Case of Epithelioma Adenoides Cysticum of Brooke, with Rodent Ulceration.[1]

By E. G. Graham Little, M.D.

The patient is a Scotswoman, aged 54, whom I have seen to-day for the first time. She was sent to me by Dr. Walker-Love, of Airdrie, with a very interesting history, contained in the following extract from his letter: "She says she has had the eruption on her face for thirty-five years. She remembers it beginning as a very small form of eruption but she cannot give me any better description. I saw her about twelve or thirteen years since when I diagnosed the case as one of molluscum contagiosum. I sent her to McCall Anderson (my old teacher of skin diseases), when he confirmed my diagnosis. A section was never taken, and at this time I was anxious to get a picture of the condition before any part was removed for section. The patient tells me that several of the affected parts healed up years ago and on examination of the right side of her face I could see distinctly the old scars." No other members of the family have had any similar eruption.

At the present time there are two types of lesion, a small waxy translucent nodule about ½ in. in diameter, firm and hard to the touch with numerous vessels coursing over the surface; and much larger fungating masses of obviously malignant growths, which, however, are said to have originated in similar nodules. The face is the only part affected and nearly the whole surface of this is covered with these lesions, which produce an unsightly appearance, and render the patient

[1] At a meeting of the Section, held October 16, 1919.

sensitive to observation so that she will only travel by night trains. There is no enlargement of glands, although ulceration has been present in the oldest patches for at least sixteen years.

One is naturally diffident of disputing McCall Anderson's diagnosis made in an earlier stage of the disease, and, in fact, when the excellent picture of the case was sent to me, in the first instance, by Messrs. Wright, of Bristol, as an illustration of molluscum contagiosum, I was not unprepared to accept the diagnosis, owing to certain resemblance it bore to a famous case shown by Colcott Fox of large molluscum tumours of the scalp. But after seeing the case to-day I am convinced that epithelioma adenoides cysticum explains all the symptoms satisfactorily. The case is, in fact, extremely like that of a man seen by me, and also by Dr. Sequeira, the record of which I published in 1914. This man had also been under Dr. Sequeira, who had regarded the case as rodent ulcer, and I am now convinced that the two affections are phases of the same disease, an identification first suggested by Dr. Adamson.

DISCUSSION.

Dr. SEQUEIRA: I agree with Dr. Graham Little's opinion on this case. A man was under my care a long time who had a somewhat similar condition, in which the growths were more or less fungating. I called it multiple rodent ulcer. The lesions yielded remarkably rapidly to X-rays, but recurred.

Dr. G. PERNET: In Dr. Colcott Fox's case of fungating molluscum contagiosum in a woman, the lesions were all over the scalp. On the face there were discrete molluscum contagiosum lesions which were absolutely typical.

Sir JAMES GALLOWAY: I very well remember Colcott Fox's case; in spite of the gigantic size of the mass, it was easy to identify the characteristic units of molluscum contagiosum on the scalp and in the coalesced overgrowth of epithelial tissue. There does not appear to be any suggestion of that in the present case, and we shall probably all agree as to the correctness of Dr. Little's view.

Dr. H. C. ADAMSON: I think this is a case of multiple rodent ulcer, or multiple benign cystic epithelioma, which I regard as ætiologically the same condition. Ten years ago I published a note in the *Lancet*,[1] pointing out that Brooke's adenoides cysticum was a benign form of rodent ulcer. I collected

[1] "On Multiple Rodent Ulcer; its relationship with Multiple Benign Cystic Epithelioma," *Lancet*, 1908, ii, p. 1133; see also "On the Nature of Rodent Ulcer; its relationship to Epithelioma Adenoides Cysticum of Brooke, &c.," H. G. Adamson, *Lancet*, 1914, i, p. 810; "Epithelioma Adenoides Cysticum of Brooke in mother and daughter and two sons; its relationship to Multiple Rodent Ulcer," H. G. Adamson, *Proceedings*, 1914, vii (Sect. Derm.), p. 95.

cases and published photographs. In Brooke's disease the lesions do not ulcerate, and often the condition occurs in families. In those cases that ulcerate (multiple rodent ulcer) there is not so strong a familial history. All except one of the ulcerating cases which I collected were in males: it is unusual to see the ulcerating type in women. I think that some lesions in this patient, notably that on the forehead, have become secondarily epitheliomatous. X-rays will probably increase the growth of the secondary epitheliomatous element, and I would not recommend that form of treatment in this case.

The PRESIDENT: Much can be done for these lesions by curetting and by using arsenical paste or some destructive caustic. I think Dr. Little may be able to get the patient well, and in any case she should not be left in that condition. Even the forehead lesion does not seem to be adherent to bone.

Dr. GRAHAM LITTLE (in reply): The patient suffers very much from the pain of these lesions. She wants to go back to Scotland to-day, but I shall try to persuade her to stay for treatment. I intended to apply arsenical paste, which I have found relieves pain.

Case of Acanthosis Nigricans.

By H. W. BARBER, M.D.

E. L., FEMALE, aged 27, married. Diagnosis: Acanthosis nigricans; carcinoma ventriculi.

Family history: The patient is married, but has no children. Her father and mother are alive and in good health.

History of present disease: She felt perfectly well until January of this year. She then noticed that her neck was becoming pigmented, her hands roughened, and her nails fissured. Later she observed pigmentation of her axillæ and of her abdomen around the umbilicus. She also noticed that the skin of her feet was becoming roughened like that of her hands. Her hair also began to fall. It was not until the end of August that she began to complain of a dull aching pain in the epigastrium and in the back. This pain was soon followed by early morning vomiting. She was sent to me by Dr. Hulke, of Deal, on September 18. The diagnosis of acanthosis nigricans was then obvious, and with the history of abdominal pain a tentative diagnosis of abdominal carcinoma, probably carcinoma ventriculi, was made. She was at once admitted to the wards under Dr. French.

Present condition: There is deep pigmentation associated with proliferation of the papillæ and accentuation of the natural lines of

cleavage of the skin in the following situations: (1) the neck; (2) the axillæ; (3) the mid-thoracic region and around the umbilicus; (4) the flexures of the arms and legs. The skin of the hands and feet are roughened from hypertrophy of the papillæ. There is also pigmentation around the eyes. There are no obvious mucous membrane lesions.

Test meal: Volume 22 c.c., contains no blood, but much undigested solid. No free HCl—contents scarcely acid to Congo red. Active HCl, 0·055 per cent.; acidity, 0·04 per cent. HCl; hypochlorhydria.

X-ray report after barium meal: There is an irregularity of the pyloric antrum, which suggests a neoplasm of the greater curvature projecting into the lumen of the stomach. There is no pyloric stenosis. On examination of the abdomen a swelling can be palpated in the epigastrium, which is very tender to the touch.

The blood-pressure while in hospital has varied between 120 and 130. While in hospital the patient has had a great deal of abdominal pain, particularly at night time.

The left knee-joint became swollen about two months ago, but X-ray examination shows no bony change. There are a large number of pigmented moles on the face and elsewhere. These on the face have been present ever since she can remember, but she thinks that fresh ones may have appeared lately elsewhere.

DISCUSSION.

Sir JAMES GALLOWAY: Though I have only seen the neck and hands, I am prepared to agree with the diagnosis which has been made by Dr. Barber. I would mention a case which has been under my observation at Charing Cross Hospital during the last three months, the patient being a woman a little older than Dr. Barber's patient. Pigmentation appeared in the regions which are characteristic of acanthosis nigricans. There was a little roughening and a suggestion of papilliform overgrowth, as in Dr. Barber's case. Knowing very well the suggestions made some years ago, among others by French colleagues, that these cases were associated with tumours of the abdomen, especially involving the region of the crura of the diaphragm and the lymphatic structures in that area, probably with extension to the suprarenal glands, we made a very careful examination, and there was undoubtedly something to be felt in the nature of a tumour in that region. The patient lost weight and strength, and did not look well. She was under observation for some time, and I felt inclined to make a complete diagnosis of acanthosis nigricans with the development of an abdominal tumour in the region I have named. It never occurred to me, however, that it would be wise to do an operation with any hope of a cure in such a case. If the growth has commenced in the stomach, the diagnosis carries the suggestion

that the growth is already extending and involving the lymphatic glands in the neighbourhood of the suprarenal body, and even the suprarenal body itself. Under those circumstances one can hardly imagine any operation of a curative nature being possible. It appears to me that in this case all that should be done is an operation of exploratory nature. If there is a gastric growth it does not involve the pylorus, as there is no constriction nor difficulty in the food passing into the bowel. The removal of any growth found in the stomach would, I fear, not cure the disease.

The PRESIDENT: Sir James Galloway's suggestion or contention is placing too much weight on what, after all, is but a hypothesis. I gather from Dr. Barber that the patient is to have an exploratory operation to-morrow, with which, apparently, Sir James agrees, and what will be done subsequently will, of course, depend on what is found at the operation. If the surgeon finds a growth of the stomach which is apparently removable, and does not find evidence of diffuse infection or invasion, he will do the operation. If it turns out to be carcinoma of the stomach, the opening of the abdomen will (as we know from cases of gastro-intestinal carcinoma) give great relief from the removal of the main mass of growth, for the pain in some of these cases is very severe.

Postscript.—A laparotomy was performed by Mr. Fagge, who found the peritoneum invaded by multiple nodules of growth, the primary disease being apparently in the stomach. The abdomen was closed without any further operative interference.

Cases of Premycosic Erythrodermia.

By H. W. BARBER, M.D.

CASE I.

M. R., FEMALE, aged 39, married. Diagnosis: Premycosic erythrodermia.

History: The patient is married and has two healthy children. About eight years ago she began to suffer from profuse sweatings, chiefly in the daytime, so that often her clothing was drenched with perspiration. Three years ago she developed abscesses in various parts of the body. The present eruption began as an irritable red rash on her left arm, one year and eight months ago, it then spread to her shoulders and hips, and later involved the back and abdomen. The rash has always been associated with intense itching, but it has been scaly only during the last twelve months. The two patches on the face appeared

during the past twelve months: the one on the left is the most recent of all.

She was admitted under my care on September 2, 1919.

Present condition: Skin: The eruption involves chiefly her trunk, back and front, and her buttocks and thighs. It consists of reddened infiltrated plaques, many of which enclose islets of normal skin. In parts the contour of the plaques is convex, in parts concave. Definite circumscribed nodules can be felt in places. There are two scaly patches beneath the eyes. Glands: The cervical glands are not enlarged, but palpable glands are present in the axillæ and groins. The nails are worn and shiny from constant scratching.

At first while in hospital the itching was intense and caused insomnia. Alkaline baths, a tar lotion, and inunction with liq. picis carbonis 1 dr., past. Lassar ad 1 oz., were only partially successful in subduing the irritation. It was then decided to give intramuscular injections of cuprase, and six injections have up to now been given at intervals of four days. After the first injection the patient noticed an immediate improvement in her itching and this improvement has continued, so that she can now sleep without hypnotics. At the same time X-rays have been applied to her left thigh and the eruption here is disappearing under their influence. One patch behind her left knee has spontaneously involuted without the rays. The patient's weight has remained almost stationary during her time in hospital. Wassermann reaction, negative. Blood count: Unfortunately exact figures have been mislaid; there was a relative lymphocytosis.

Case II.

W. S., male, aged 38; occupation, engine driver. Diagnosis: Premycosic erythrodermia.

Family history: Married, one child.

History of disease: No particular previous disease. The eruption appeared on the feet one year and three months ago. It was next on the small of the back, then spread up the back, and later involved the buttocks and thighs, and trunk generally. Before the eruption came on the body the patient had a "burning feeling" under the skin, so that he would scratch off the superficial layers of his skin. About four months ago he began to have profuse sweatings, which occurred night and day, but have ceased since he received hospital treatment. The itching sensation, which has been very severe, began about four

months ago, and was so intense that he would be sleepless for weeks. His general health has been good.

The patient came under my care on August 18 of this year, and shortly afterwards he was admitted to Guy's Hospital, under myself.

Description of rash: The eruption involves the trunk, the thighs and buttocks, arms and legs; also the neck, the forehead and the eyelids. It consists of infiltrated, irregular, reddish or brownish, plaques, with irregular edges: as in my other case the typical islets of healthy skin enclosed by the plaques are well seen. In places the eruption is fading and the infiltration has disappeared.

The glands in the groin are enlarged. Wassermann reaction negative. Blood count showed slight excess of lymphocytes, but no other change.

While in hospital the patient has been treated with alkaline baths and a tar paste. Since his discharge on September 11 he has had three injections of cuprase at weekly intervals. His itching has practically disappeared and he sleeps much better.

Case of Granuloma Annulare.

By GEORGE PERNET, M.D.

THIS girl is aged 13, and I first saw her on August 29, 1919. This is only the second case of the kind I have had in my clinic at the West London Hospital in seven years at least. When she came to the hospital she had a similar patch on the inner side of the right arm, the remains of which can still be felt. A week later, another appeared symmetrically on the inner side of the left arm. Both have involuted spontaneously. She has only had a little treatment applied to the left hand, the other has not been dealt with at all. With regard to the name, that of granuloma annulare was given to the condition by Radcliffe-Crocker, on clinical grounds. But my microscopical examination did not show plasma cells, so I prefer the name celluloma annulare.

DISCUSSION.

The PRESIDENT: How does Dr. Pernet exclude granuloma in the presence of lymphoid infiltration? Granuloma is an old name, and it was introduced by Virchow, before plasma cells were known, for a round-celled tumour of infective origin.

Dr. PERNET (in reply): There are no plasma cells, and I regard granuloma as a growth in which there are plasma cells, such as the infective granulomata: syphilis, tuberculosis cutis, leprosy, mycosis fungoides; and limit the name to such conditions.

The PRESIDENT: I do not agree with that.

Case of Onychotrophia.

By W. KNOWSLEY SIBLEY, M.D.

THIS girl is aged 17, an orphan. She never remembers having had any finger nails, and there is a complete absence of them, except on the index finger of the right hand, and the thumb of the left, where some atrophied nail is present. On her feet she has no great toe nails: on the other toes there are some nails present, but they are atrophied. We do not see many of these cases, and it is unfortunate that one cannot get a clearer history with regard to this girl. When she first came to the hospital her Wassermann reaction was positive: I do not know whether that fact has any connexion with the onychia. One cannot detect evidence of specific lesions: her hair is fairly good, and the teeth are somewhat pegged, but they, too, are fairly good. I do not detect any enlarged glands. Her mental condition is somewhat below par, otherwise there is nothing very definite to report about her.

DISCUSSION.

Dr. F. PARKES WEBER: I think "partial absence of the finger nails and of the great toe nails" should be the heading of the case. The term "atrophy of finger nails" suggests that this girl had had ordinary finger nails, and that they had atrophied, whereas the case is probably one of imperfect development (or hypoplasia) of the finger nails and nails of the great toes.

Dr. J. H. STOWERS: I appreciate the fact that it has been impossible to learn the early history of this patient, an orphan, but there is no evidence that I can discover, at this time, to confirm the view that she is the subject of congenital syphilis Even when a Wassermann reaction is reported positive in cases of such doubtful character the test should be repeated more than once in view of the conflicting results obtained by bacteriologists whose methods of investigation are not always uniform. The lesions in this patient are sufficiently explained, I think, by congenital defects wholly apart from syphilitic taint.

Case of White-spot Disease.

By Haldin Davis, F.R.C.S.

This woman presents an example of the condition known as sclerodermia guttata, or herpetic morphœa, one of the divisions of what has been called, rather loosely, white-spot disease. The lesions are on the back, chest, wrist, and one has just started on the face. The condition only dates from eighteen months ago. What makes the case more interesting is, that the patient has myxœdema, and has had it for fourteen years. For this she has been taking thyroid tablets regularly, a drug which is frequently exhibited in cases of sclerodermia. She is anxious about the lesion on the face, and I shall be glad of suggestions as to treatment.

Dr. Sequeira: Some time ago I showed a patient who had taken thyroid for fifteen years and who developed sclerodermia on both legs.[1] I do not allege a connexion between the conditions, I simply recorded their coincidence as a fact.

Case of unilaterally distributed Molluscum Fibrosum Tumours, with a History suggesting Recurrence.

By E. G. Graham Little, M.D.

The patient is a boy, aged 12. Two years ago he had a warty growth, similar to the present tumour, removed by excision, the scar of which remains very conspicuous. About 1½ in. below this scar and very soon after the operation, a number of molluscum fibrosum tumours grew over an area of about 3 in., one large one the size of a mandarin orange, and several smaller ones immediately adjoining it. All have the same character of soft tumours in the skin. There is no ulceration or other change, but Dr. Lewis, of Braintree, who was kind enough to send me the case, writes that the increase in size has been rapid, and with the odd history he felt some uneasiness as to the nature of the condition.

[1] *Brit. Journ. Derm.*, 1916, xxviii, p. 46, and *Proceedings*, 1916, ix (Sect. Derm.), p. 41.

DISCUSSION.

The PRESIDENT: I should have thought this was molluscum fibrosum, and we are familiar with the fact that molluscum fibrosum is not always obviously present from birth: these tumours go on developing for a long time, up to and beyond middle age. Probably the germ of this tumour was lying in the skin, and is now developing at this age. He may develop such tumours in other places, or this may be a localized patch of tissue. I think there is no doubt about the diagnosis. It used to be called dermatolysis, and it belongs to the group of neurofibromata.

Dr. F. PARKES WEBER: The case seems to be developing into a "dermatolysis" tumour or "fibroma pendulum" rather than an ordinary case of multiple molluscous fibromata. The question is whether it would be well to try to remove the tumour.

Dr. GRAHAM LITTLE (in reply): The feature in the case with which I am unfamiliar is the very isolated character of the patch, with a number of discrete tumours growing upon it. And the proximity to the previous lesion is rather striking.

Case of Folliculitis Ulerythematosa Reticulata of MacKee and Parounagian.

By E. G. GRAHAM LITTLE, M.D.

THE identification of this case I owe to Dr. Barber, who kindly gave me the reference to two cases published by MacKee and Parounagian under the above title.[1] My case is obviously of the same nature as theirs. The patient now shown is a public school boy, age 15, of Irish race, somewhat small for his age, but in excellent general health, sent to me by Dr. Robert Cuff, of Scarborough. On both cheeks over the malar eminence and nowhere else there is an area of skin about the size of a half-crown, in which the sole obvious change is the presence of tiny pitted depressions with an appearance like the surface of pumice stone. Here and there in this area there are a few comedones, filled with hard horny plugs, which are dislodged with some difficulty. They cannot be extruded with an ordinary comedo extractor, and are much harder and more superficial than acne comedones. There is not, and has never been, any pustulation or inflammation, and there is no sign of erythema at present. The boy states that he has had the

[1] *Journ. Cut. Dis.*, xxxvi, 1918, p 339.

lesions at least five years, and that the majority of the pitted depressions were not preceded by comedo formation. They cause absolutely no subjective symptoms, and the appearance supplies the only reason for troubling about the condition.

I have used MacKee's title, but I should prefer his alternative naming of this condition—"folliculitis atrophicans reticulata." Erythema has never been a notable feature in my case as far as can be gathered from the narrative of the boy, who exhibits the typical aplomb and independence generated at an English public school. His parents live in Ireland, and his mother who came with him to me knew very little about her son; his own history was far more convincing than hers, which however in no way conflicted with his statements. I have not yet been able to obtain a biopsy.

DISCUSSION.

Sir JAMES GALLOWAY: Perhaps none of us may be able to help Dr. Little in regard to the actual naming of this condition, but I would refer members to the records of one or two cases of the type which were shown many years ago before the London Dermatological Society by Sir Cooper Perry. Sir Cooper thought it was due to some peculiar form of infection, of mild type, producing fine atrophy of the skin.

Dr. G. PERNET: I have published a case of this type, and I called it atrophodermia reticulata symmetrica faciei.[1]

Case of Recurrent Erythema Multiforme.

By ALFRED EDDOWES, M.D.

PATIENT, a male, aged 27. General health good; has been subject to this eruption for seventeen years. It frequently recurred until January last, when I first treated him. Since then till now he has been free from it. The eruption affects chiefly the neck, hands and wrists—exposed parts. I have lately discovered that he has had an offensive discharge from his middle ears since he was aged 4—i.e., for twenty-three years. It seems to me highly probable that the chronic disease of the middle ears may be the source of toxins sufficient to render the system vulnerable to the influences of wet and cold weather and to constitute the chief cause of the eruption. It will be interesting to watch the result upon the skin of special treatment for the ear disease.

[1] Pernet, *Med. Press and Circ.*, 1916, n.s., ci, p. 487 (with figure).

Lepra: Sequel to Case shown on June 21, 1917.

By F. Parkes Weber, M.D.

The case was that of a woman (Mrs. S. W.), then aged 66, Russian Hebrew, born in Odessa, but resident for the last ten years in London. She complained of a chronic eruption of nodules in the skin of the upper and lower extremities, and a few also on the trunk and face. This eruption had, she thought, commenced eight years previously, that is to say, two years after she came to England. I regarded the case as an unusual one of xanthoma tuberosum multiplex. Local treatment by X-rays was tried on and off under the supervision of Dr. James Metcalfe. Arsenic was given internally. But the patient did not decidedly improve, and the X-ray treatment was permanently discontinued in June, 1918. After that the nodules increased in number, especially in the face, and the patient now presents the typical "leonine" facies of tuberculous leprosy. Two nodules have been recently excised for "biopsy" purposes, and by microscopical examination the granulomatous tissue is found to contain lepra bacilli in large numbers, in many places collected together to form "clumps." There already are small leprous nodules in the sclerotic and cornea of the eyes, so that the disease is likely to produce blindness in the present case.

Section of Dermatology.

President — Dr. Arthur Whitfield.

Multiple Pigmented Hæmorrhagic Sarcoma (?); a Case for Diagnosis.[1]

By E. G. Graham Little, M.D.

The patient is a market gardener in Cambridgeshire, who has for many years used an arsenical spray for killing plant parasites. About seven years ago the condition now noted on the hands began to be noticeable. This condition is perhaps best described as confluent telangiectases so that the whole of the dorsum of the hands and all the palm is affected, except that a free portion in the centre, about the size of a five-shilling piece, is left white, the rest of this surface being a deeply suffused brick-dust tint, permanent in colour and not blanched by pressure. The surface of the affected skin is perfectly smooth and, indeed, unaltered. There is no exfoliation, nor atrophy, nor tumour formation. During the last eight months numerous discrete hæmorrhagic and pigmented patches have developed on the face and neck, and in this position there has been considerable exfoliation, especially on the pinna of the ears, where some loss of tissue would seem to have resulted. There is a distinct tinge of brown pigmentation here which is lacking in the hands, though the process is obviously essentially the same. The whole face has thus a dusky and patchy appearance, which extends on to the neck and ears. The nails are thickened and there is some tenderness of the tip of the fingers. There is no itching, but some burning sensation in the hands. Recently the man has shown symptoms of severe illness, for which he consulted my colleague, Dr. W. H. Willcox, who sent him to me for an opinion as to the skin condition. Dr. Willcox reports that there is evidence of acute nephritis, and that the urine is

[1] At a meeting of the Section, held November 20, 1919.

loaded with albumin. An examination of the stools shows a very increased count of streptococci.

I have suggested a tentative diagnosis of Kaposi's pigmented hæmorrhagic sarcoma, although the man is not a Jew and has lived all his life in this country.

DISCUSSION.

THE PRESIDENT: I think this interesting case will prove to be one of arsenical poisoning. The patient is a fruit grower, and uses a spray containing arseniate of lead. Owing to the large area to be sprayed, the use of the machine is continued for a considerable time. The user of these fine sprays is liable to have a good deal of spray blown back into the face unless great precautions are taken. The moment I saw this patient, and before I knew his occupation, I was reminded of a case with very similar appearance where arsenic had been administered medicinally in large doses for a long period. If I am right, I presume this patient took his arsenic largely by inhalation. It might be worth while to test his urine for arsenic. It is true that he has no arsenical keratosis, but this peculiarly localized erythema without atrophy or surface disturbance is very characteristic.

Dr. H. C. ADAMSON: I think the possibility of lupus erythematosus ought to be considered here. Although there is some pigmentation on the face the condition there is mainly vascular, and entirely so on the hands, and disappears on pressure. There is also loss of tissue at the ear margins, and he has the "dead fingers," of which patients suffering from lupus erythematosus often complain.

Dr. F. PARKES WEBER: I thought of lupus erythematosus when I first saw the case. The condition of the ears seems to be "chilblainy," and is always worse during cold weather. But I suppose that a chilblainy condition may occur in a patient with arsenical changes as well as in one suffering from lupus erythematosus. Of the two I think it is more likely to be associated with lupus erythematosus.

Dr. J. J. PRINGLE: The greater part of the condition is consistent with the diagnosis of lupus erythematosus, which Dr. Adamson has suggested. Not only the ears of the patient, but also the backs of the terminal phalanges of his fingers are, I think, quite characteristic. I do not recognize the nature of the involvement of the palms to which the President directs special attention.

Dr. G. PERNET: The symmetrical distribution about the hands is in favour of lupus erythematosus, also the condition of the backs of the finger tips. I should like to know if he has albumin in his urine.

Dr. W. J. O'DONOVAN: With regard to the diagnosis of arsenical poisoning, so much depends on the form of the arsenic taken in. During the war I saw

hundreds of women who literally delved in arsenic with their hands and yet had no dermatitis. An important point I elicited is, that the patient knows several other Wisbech growers who are similarly affected.

The PRESIDENT: I agree that the fact that there are other growers in the district similarly affected is important. The arseniate of lead is in a fine emulsion, and it is distributed in an impalpable state of subdivision, hence the user can get it by inhalation.

Postscript by Dr. Graham Little.—In view of the suggested diagnosis of arsenical intoxication Dr. Willcox has kindly undertaken an examination of the urine and the hair for arsenic and will report later on his findings.

Case for Diagnosis.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a major in the British Army, aged about 40, who came to me in February last with a small patch of what looked like a leucoplakia (but then showing no ulceration) on the right side of the tongue in about its middle section. A Wassermann reaction was obtained before treatment and was reported negative. I thought that it was possibly an early lichen planus of mucosa, and lost sight of the patient for some time. On seeing him again a few months later he showed definite ulceration, superficial but of considerable extent, the eroded area being about an inch long and half an inch wide. At the same time there was a very hard nodule in the submaxillary region, obviously an enlarged gland. He had been under treatment in the interval by army doctors, who had apparently diagnosed syphilis, and he had had some treatment based on this diagnosis. The patient denies syphilitic infection at any time and has shown no other symptoms of it. He is otherwise in excellent health.

A further report will be submitted later.

Case for Diagnosis.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a schoolboy, aged 15. He has a waxy semitranslucent tumour on the inner surface of his right ala nasi, about $\frac{1}{4}$ in. above the level of the surrounding surface and about $\frac{1}{2}$ in. in diameter. The case

was sent to me by Dr. Stamford Felce with a history that the tumour had begun, certainly by the age of 4, and two attempts had been made to remove it surgically, but the growth had recurred. The possibility of rodent nodule has occurred to me, and, as the early age of inception would make this a record case, I show it for inspection before removing the small mass for histological investigation, which I propose to do later.

DISCUSSION.

The PRESIDENT: This is a very interesting case, quite apart from the obvious possibility of it being rodent. There are two diagnoses which suggest themselves to me: one, that it is a common wart, which I do not think it is. Still, one occasionally sees warts pull up the skin around them, so as to give a pearl-button-edge appearance. But the diagnosis to which I incline is localized sebaceous adenoma. I have only seen one other case, and that was in a child. I knew it was a more or less congenital lesion: it was hard and warty, and had the curious rolled edge. It did not disappear under radium treatment, though, as the subject was a little girl, we tried to avoid operating. Finally, I excised it, and it was an exquisite, localized, sebaceous adenoma. When this is removed I think it will be found to be the same. There is, I think, a fair amount of hyperkeratosis with sebaceous adenoma.

Dr. MACLEOD: I think the lesion is nævoid and probably partly composed of sebaceous tissue. I saw a case to-day in a child, aged 6, which was somewhat similar and which I took also to be sebaceous.

Dr. H. G. ADAMSON: The nodule seems to be the condition called milium. It is more usual to see single milia scattered over the face, but they do sometimes occur grouped together to form nodular masses.

Case of Lichen Planus and Pseudopelade.

By S. E. DORE, M.D.

THIS patient, a woman, aged 28, shows the unusual association of two diseases, namely, lichen planus and pseudopelade. In the year 1914 she first noticed an eruption on her legs, and at about the same time a white streak on the centre of her tongue. Two years later some papules appeared behind her ears. She then consulted a dermatologist, who made the diagnosis of lichen planus. A group of lichen planus papules is still visible behind the right ear, and a linear patch, presumably of the same character, on the dorsal surface of the tongue to

the left of the middle line. In 1917 a bald patch developed on the right side of her scalp, followed by several smaller patches on the parting of the hair and scattered on other parts of the head, which now present the usual characters of pseudopelade.

I should like the opinion of the members as to the value of treatment in this condition, and whether there is any hope of stopping the gradual extension of the cicatricial patches.

The PRESIDENT: I feel certain there is more than one disease included under the term "pseudopelade" or "cicatricial alopecia." In one type the skin is shiny and obviously scarred where the hair has fallen out. In the other, to which this case belongs, the skin is not obviously atrophic. This, of course, is not alopecia areata in the ordinary sense of the term, though the skin shows the ordinary markings, and there is no obvious scarring. The first I hold to be a curious localization of sclerodermia. In his long article on the subject, in which he said it was not a sclerodermia, Brocq did mention at least one case in which the patient had sclerodermia elsewhere, a fact which, I think, weakens his position. This type, I think, is more probably an infective condition. With regard to treatment, I would hesitate to promise anything, but four years ago I had the case of a nephew of a colleague with, apparently, the same disease as this. He lost his hair in patches, and on pulling out hairs at the edge of a patch he saw the characteristic gelatinous sheath, as in this case, and I gave a hopeless prognosis. But I said we could try antiseptics, and I rubbed in a strong combination of benzoic and salicylic acids in an ointment. After the application it stopped dead, and never relapsed. I do not know whether it stopped because of the antiseptic, or whether it was merely a coincidence. I instructed him to pull the hair gently, so that the loose ones came out, and the others did not; then that the ointment should be rubbed in so as to fill the empty follicles. I have seen him several times since. He still has the original bald patches. I used ½ dr. benzoic acid, 20 gr. salicylic acid, to which is added an ounce of cocoanut oil and soft paraffin, this composition being an excellent vehicle for introduction of the drug by rubbing in.

Case of Multiple Infective Lymphangio-endotheliomata.

By GEORGE PERNET, M.D.

THE patient is a woman, aged 41. Eight years ago I saw her for the first time, and once only, for a fungating growth about the right big toe, with a history that two years previously a dark mahogany-coloured mole, about the size of a split pea, had begun to get larger and warty, and had been steadily getting larger and larger. When I saw

her then, there was a large raised fungating mass, irregular and ulcerating, occupying the whole of the right big toe. There was induration of the borders. The corresponding femoral glands close up to the groin were enlarged and hard. My diagnosis was melano-epithelioma (nævo-carcinoma), and I recommended immediate removal of the toe, growth and glands. Subsequently I heard from the patient's doctor, Dr. Cruden, of Cricklewood, that Mr. Fairbank had operated on her. "A small portion of the growth was previously removed and found to be epithelial in nature. The toe was completely removed and all the glands in the femoral and groin region. On microscopic examination, the growth was found to consist of epithelial downgrowth, and the cells in parts contained pigment. The glands were apparently slightly infected by growth."

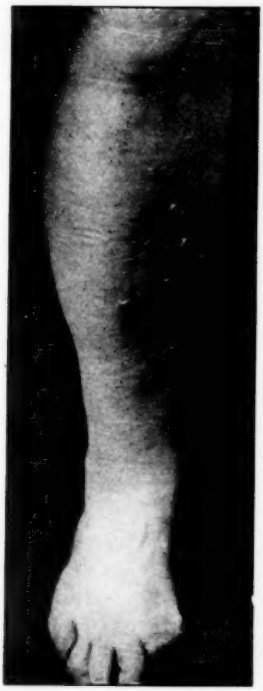
I saw no more of the patient until October 23 of this year, when she presented a dozen or more growths of the skin on the front of the right leg, as shown in the photograph. These growths varied from a pin-head and grain of wheat to a small cherry in size. The former were brownish in tint and somewhat gelatinous and translucent looking, just raised above the skin; whilst the larger ones were paler, softish to touch, and bled slightly on manipulation, and spontaneously. I removed a small growth, which was reported on by Dr. H. G. Butterfield, as follows:—

"The specimen consists of a minute tumour in the skin, about the size of a grain of wheat. On section it is seen to consist of masses of cells of an endothelial character, containing septa of fibrous tissue, with which prolongations of the stratified squamous epithelium of the skin run a varying depth. These prolongations are very thin and drawn out in places, and approach the cells of the tumour mass very closely, but the latter have no characters which suggest that they take their origin from the skin epithelium proper or any of the glands connected with it. The cells of the tumour mass vary in definition of outline, and to a slight extent in size and shape. The nuclei are well marked and deeply stained. Double nucleoli are common. The vessels vary in character from quite well-formed capillaries to mere spaces in which red blood corpuscles can be seen. On the whole the appearances suggest an endothelioma, which has had its origin in the perivascular lymphatics of some minute vessel of the skin. Nothing was seen in the course of the examination suggestive of an epithelioma of the ordinary type."

I may add that the epidermis was flattened, and very thin in parts,

over the small growth, which below was well delimited by corium tissue. The cells were not of the plasma-cell type, and in old days would have been described as round-cell sarcoma. There was no trace of pigmented cells in the sections.

That the growths are infective is obvious from the fact that minute growths are appearing in the neighbourhood. By infective I mean that the present growths are leading to fresh foci of infection in the neighbouring



Dr. Pernet's case of multiple infective endotheliomata of the skin.

skin—i.e., cellular infection. That they are malignant is undoubted. As to treatment, taking the case as a whole, removal by operation is indicated. I do not care to advise amputation in such a case. Perhaps one might try radium or X-rays, but that would be losing time. The seat of the original operation appears to be perfectly right, as also the scar where the glands were removed.

The case is a very unusual one, and in my experience I may say I

have never seen anything of the kind before. Occasionally there has been some swelling of the leg since the big toe was removed, but this can be explained by the absence of the femoral glands.

DISCUSSION.

Dr. MACLEOD: This case is extremely interesting, because Dr. Pernet said in the first instance the patient had melanotic epithelioma, for which her toe was removed, and now there is a growth which is mesoblastic. I suggest that there are a certain number of those melanotic nævi which, apparently, do not originate in the epidermis, but in the corium. Such a case occurred in one of my own colleagues, and I removed the growth, and felt certain it was a connective tissue nævus. I do not think the cells in the section exhibited are plasma cells.

Dr. MACCORMAC: I have only had opportunity for making a very short examination of the sections under the microscope. It seems to me, nevertheless, that the structure of the neoplasm conforms much more closely to a sarcoma than to a pigmentary carcinoma.

The PRESIDENT: I draw attention to two facts. This section is one of metastatic growth. In metastatic growths it is impossible, in some cases, to determine whether it was originally carcinomatous or sarcomatous. For a moment I thought there were here ill-formed plasma cells, but I do not think so now; I think it is secondary nævo-carcinoma. We had a case at King's College Hospital which was diagnosed as multiple lymphomatosis of the skin. It was under the care of a physician. I was certain it was not that, and we had a nodule cut, and the section was universally diagnosed as sarcoma. The patient eventually died, and then it was found that every gland in the body and all the lumps in the skin were of the same variety, and all secondary to prostatic carcinoma, with epitheliomatous involvement of the glands. Nævo-carcinomatous masses when segregated are impossible to diagnose. For years it was called melanotic sarcoma, and the original nævus was discussed by Recklinghausen, who called it lymphangioma-endothelioma. Possibly Dr. MacLeod's contention is right, that either we are altogether wrong, or we have thrown too many tumours into one class. I think that if a primary growth is removed and a secondary growth appears at some distance off, and at some time later, it is impossible to diagnose it. I have sections which Dr. Bulloch, of the London Hospital, gave me, taken from a child who came to hospital because of a new growth of the toe, which was clearly carcinoma, not a congenital condition. It arose from sebaceous glands, and is the only tumour of the kind I have seen which formed regular sebaceous glands in the carcinomatous process. That was removed, and a few weeks afterwards, little nodules somewhat like these appeared up the leg, and the child died of general carcinomatosis. I have only sections of the secondary nodules now, and I defy anybody to say whether they are sarcoma or carcinoma. I think that in

this case they are late metastases, little deposits of malignant disease which have taken on activity. For that reason I doubt whether X-rays will do any good, as they probably would if the condition were sarcoma. I think the only possible treatment is to remove that piece of skin, because it is too late at this stage to talk of amputation if she has already had pigmentary cells in the glands of the thigh. She must be given the chance that she has only a few "rests" left in the skin.

Dr. H. G. ADAMSON: I should have no hesitation in saying that the section shows the appearances of a typical carcinoma, and that this represents a secondary cancerous deposit along the lymphatics.

Addendum by Dr. Pernet.—The case was referred for a surgical opinion on the facts to Mr. John Murray, who has advised excision of the nodules.

Case for Diagnosis.

By J. M. H. MACLEOD, M.D.

THE patient is a well-nourished girl, aged 21, who is suffering from a large number of lesions, which I took at first to be of the scrofulous gumma type. They are situated on the legs, feet, arms, back, and shoulders, there being between thirty and forty of them present, and they consist chiefly of depressed scars that have resulted from the breaking down of nodules varying in size from a cherry to a walnut. The affection began two and a half years ago, and was first noticed as a hard raised white swelling on the outer side of the right leg, which some weeks later softened, broke and discharged a thick green pus. For five months new lesions kept on appearing and took from a few weeks to a couple of months before they softened and discharged. One lesion on the leg resembled erythema induratum, but the others were more rapid and acute in their evolution than the lesions in that disease usually are. The resultant scar is depressed, thin, wrinkled, and slightly pigmented, but there is no surrounding halo of pigmentation such as is so often seen in erythema induratum. For about a year only an odd lesion or two appeared, but recently their appearance has been more frequent. The patient was admitted into Charing Cross Hospital on October 21, 1919, and since then four new lesions have developed.

The patient gives a history of having had "peritonitis" a year before the first lesion appeared, but otherwise her general health has been good. There is a history of tuberculosis on the maternal side, but

she herself has no stigmata of tuberculosis, and her temperature is normal or subnormal. There is no suggestion of congenital syphilis, and her Wassermann reaction is negative. She was given 0·2 grm. of novarsenobillon, but it had no effect in the way of healing active lesions.

The case is still under observation, but so far no organism has been found in the pus. A commencing lesion has been excised for histological examination and inoculation into a guinea-pig, and the results of this will be reported later.

DISCUSSION.

Dr. H. G. ADAMSON: Two conditions occur to me: sporotrichosis and dermatitis artefacta. The peculiar artificial distribution of the lesions and their oval shape strongly suggests an artefact eruption. The earlier nodules appear to me to be deeply seated contusions or "bruises," probably produced by pinching up the tissues.

Dr. MACCORMAC: In cases such the one exhibited, where culture of sporothrix from the lesions may be extremely difficult, use might be made of the method of complement fixation, employing as antigen a relatively old growth of sporothrix on sugar agar. Such a test, where positive, is of course specific evidence of infection, and may thus prove of the greatest value where the diagnosis is in doubt.

The PRESIDENT: Has Dr. MacLeod borne in mind the possibility of it being a very slow and chronic case of glanders? These lesions have come up quickly for sporotrichosis, or for tuberculous lesions as well, while the multilocular character of the lesion on the arm is like the appearance of chronic glanders. The *Bacillus mallei* is a difficult thing to grow, and is very liable to be missed in cultures: moreover it does not retain the ordinary stains very well. I think the case will prove to be tuberculous.

Dr. MACLEOD (in reply): The lesions cannot possibly be artefacts as they appear first as nodules, which take from a few weeks to two months to evolve and break down.

Case of "Erythrodermie Congénitale Ichthyosiforme."

By H. W. BARBER, M.D.

THE patient, a boy, aged 7, is the last child but one of a family of ten children. No other children in the family are affected, and no history of a similar condition of the skin among members of either parent's

family could be obtained. He was a full-time child. According to the mother's statement he was born with "thick whitish skin" on the palmar and plantar surfaces; elsewhere the skin was quite soft and smooth. When 4 months old he developed lesions on the scalp and body, which were diagnosed as "eczema," but from the mother's description it is more likely that they were of the nature of a bullous impetigo. When 3 years old the skin of the trunk and limbs became roughened and covered with horny growths, and the present appearances have existed for several years. The face is spared, and there is no ectropion such as is present in some cases. The scalp is scaly. The palms and soles are distinctly hyperkeratotic. The nails are poorly developed, but not fissured. The skin of the neck, axillæ, antecubital fossæ, groins, popliteal spaces, elbows, knees, and of practically the whole trunk is roughened, reddened in places, and covered with dark, horny vegetations. In some parts, e.g., forearms and legs between ankle and knee, the skin is practically normal. A musty odour, such as that described by Darier in his case, is perceptible in the patient.

There is no doubt that this case should be included in the group "ichthyosiform erythrodermia," and it is very similar to a case described by Darier as "erythrokeratodermie verrugueuse en nappes, symmétrique et progressive." A complete review of the literature of all published cases will be found in the *Journal of Cutaneous Diseases*, May, June, August, 1917.

Multiple Xanthoma in a Boy.

By H. G. Adamson, M.D.

The patient is aged 6. The eruption was first noticed by the boy's mother six months ago, and it has gradually increased. The whole trunk is covered with numerous small raised reddish-yellow plaques varying in size from that of a hemp-seed to that of a split-pea. There are a few lesions also in the neck, scalp, upper arms and thighs. The smallest lesions are slightly raised flat disks with a slight central depression. They have an angular margin, are soft to the touch and apparently quite superficial. Their colour is reddish-yellow, but on firm pressure the red colour disappears and the lesion becomes of a bright sulphur yellow colour. The large lesions are made up of very closely set angular plaques blended to form a single plaque, but the elements of these can

be made out by a slight grooving of their surface. There is no itching. There is no sugar and no albumin in the urine. A microscopical section shows numerous xanthoma cells, many of them large and multinucleated.

The eruption belongs to a group of cases which have been described as occurring in children and sometimes called congenital xanthoma, in spite of the fact that they do not always appear at birth nor even soon after. Some authorities have distinguished these juvenile cases from adult cases of xanthoma and have regarded them not as inflammatory but as new growths of nævoid character. McDonagh published a paper in the *British Journal of Dermatology*, and recorded five of these cases, and supported the view that they are congenital endothelioma. Other observers have thought that it is not possible to put these juvenile cases into a different class from the adult cases, and Louis Török in a paper in the *Annales de Dermatologie* (1893, iv, No. 11, p. 1119 *et seq.*) collected all the published cases of xanthoma to that date. Thirty of these were juvenile cases and forty were adult cases. The age of onset of the juvenile cases varied from birth to 16 years.

Mr. McDonagh: I believe, that xanthomata can be divided into two groups: (1) inflammatory, (2) new growth. Without having a section as a guide I incline to the view that the case presented falls in the former category, because the lesions are only slightly raised, because each is divided into several loculi, and because they have undergone no naked eye change since their appearance. If the article I published in the Journal[1] be referred to at the passage in which the new growth xanthomata are described as being nævo-xantho-endotheliomata, it will be seen stated that the lesions are raised, even on the surface, are hæmorrhagic to start with, and become xanthomatous before they spontaneously disappear. In both types the colour which gives rise to the name "xanthoma" results upon a certain chemical degeneration undergone by the protoplasm of the endothelial cells.

Case of Linear Morphœa.

By J. L. Bunch, M.D.

The patient, aged 26, is a young soldier in the South African Army and was sent to me by Captain Monaghan. He was wounded in the left hand two and half years ago, and nine months afterwards a patch of well marked linear morphœa, or sclerodermia, began to show itself

[1] *Brit. Journ. Derm.*, 1912, xxiv, p. 85.

at the outer margin of the right orbit and gradually spread backwards as a band about an inch wide on to the scalp, and the hair has quite disappeared in the area of the lesion. The skin is thickened, hard and sclerodermatous, and the patient is much concerned about the disfigurement which it causes. Two months ago a similar patch commenced symmetrically on the left side and is growing backwards from the orbit. The patient should have been repatriated some time ago, but has had his leave extended in order to see whether any improvement could be effected in his condition. In such cases I have found that radiant heat gives better results than fibrolysin, X-rays, or radium.

Case of Shaving-brush Anthrax of Face; Recovery.

By GEORGE PERNET, M.D.

The patient is a young man, aged 26, who was brought to my out-patient department at the West London Hospital, on October 24, for a small, somewhat crusted sore about the size of a shilling, on the left side of the face. The sore had begun six days previously. There was some œdematous swelling in the neighbourhood and some adenitis about the corresponding angle of the jaw. Dr. Sidney Greene, of North Kensington, who brought the patient to me, stated he had stained some of the discharge and found a large bacillus, which from his description sounded like *Bacillus anthracis*. But I did not see his preparation until a week after, when I had no doubt about it. I sent the patient on to Dr. Elworthy, who did not find the bacillus in the sore, but he succeeded in obtaining cultures. From the shaving-brush Dr. Elworthy obtained positive results in the guinea-pig. The patient, previously to using the new shaving-brush, had cleaned it in a soda solution and dipped it in hot water before using. Clinically the sore did not look like anthrax when I saw it, but like an ordinary pus infection lesion. I ordered ung. hydrarg. ammon., 10 gr. ad 1 oz., which he has been using since. The lesion improved and the accompanying swelling and adenitis went down. By November 7 the sore had healed, leaving a small depressed cupuliform scar with a slightly raised edge. Dr. Elworthy[1] has obtained

[1] Dr. Elworthy was the first to show the shaving-brush origin of anthrax, some years ago, in a fatal case which occurred at the West London Hospital. Reginald R. Elworthy: "An Outbreak of Anthrax conveyed by Infected Shaving-brushes," *Lancet*, 1916, i, p. 20.

positive results from other brushes of the same parcel. I can only account for the favourable upshot in this case on the assumption of lowered virulence of the bacillus.

The PRESIDENT: Several cases on the face have been recorded which were traced to the use of a new shaving-brush. I think we ought to remember that some authorities regard local anthrax as a mild disease in which the lesion should not be cut out.

Section of Dermatology.

President — Dr. ARTHUR WHITFIELD.

Case of Hæmorrhagic Lupus Erythematosus.

By J. H. SEQUEIRA, M.D.

D. L., AGED 37, a married woman, was admitted to the London Hospital on November 28, 1919, suffering from lupus erythematosus. Her father and mother, aged respectively 79 and 75, are in good health. She has four brothers and one sister alive and well. One sister died of phthisis and another sister died in childhood—cause unknown. There is no history of tuberculosis in the family on either side with the above exception. The patient has suffered from tonsillitis three times and had rheumatism at the age of 18, when she was ill for three months. At the age of 10 she had measles. She has been married several years but has no family and has had no miscarriages. In June, 1919, an eruption appeared on the nose. It began as a small spot and spread until the whole of the tip of the organ was involved. In the second week in October, 1919, other spots appeared on the fingers of both hands and also on the toes. These gradually involved all the digits.

On admission to hospital the lower third of the nose was covered by a red scaly well defined patch, characteristic of lupus erythematosus. The eruption on the hands was hæmorrhagic. The purpuric areas were distributed as follows: On the left hand the whole of the dorsum of the little finger, the terminal phalanx of the ring finger, two large areas involving the greater part of the middle finger, the whole of the index and the ulnar side of the thumb. On the right hand the whole

[1] At a meeting of the Section, held December 18, 1919.

of the little finger was affected and there were discrete hæmorrhages the size of a sixpence on the middle of the ring finger, the terminal part of the middle finger and a large area affecting the whole of the index finger up to the terminal phalanx, and there were two small areas on the right thumb. These were all on the dorsal aspect. There was slight extension of the hæmorrhagic patches on the palmar aspect. The toes were also extensively affected, a large number of purpuric areas being present on the dorsal surface. The buccal mucosa was unaffected and the lips were also free. There was no eruption on any other part of the skin.

The eruption gradually spread and on December 9 the fingers were more extensively affected and spots appeared on the dorsal aspect of both feet.

On December 12 a large bulla containing clear fluid appeared in the middle of the right calf.

On December 15 more hæmorrhagic areas appeared around the toes, extending to the sole of the foot.

The temperature was quite normal throughout and there was no albuminuria. There was no evidence of disease of the thoracic viscera.

The patient has been treated by intravenous injections of quinine, 5 gr. of the hydrochloride being injected at weekly intervals.[1]

These cases are, in my experience, extremely rare. Looking over the records at the London Hospital, as I had to do for another purpose some time back, I only found two cases, one of which was under the care of my predecessor, Sir Stephen Mackenzie. In July, 1912, I had another case, of which I exhibit a water-colour drawing (done by Miss Green) showing hæmorrhagic patches, not only on the fingers, but also on the face.

At present, in the same ward, there is a very acute case of lupus erythematosus, in a patient aged 28, who had a remarkable pyrexia of nearly three weeks' duration, the temperature going up to 103° or 104° and 105° F., and as there were toxic symptoms one almost despaired of her life. But her temperature has now come down by lysis, and she is recovering rapidly, and although the patient was most seriously ill there have been no hæmorrhages into the skin.

[1] The patient, after being kept in bed, greatly improved as a result of the rest and the quinine injections, and has since left hospital.—J. H. S.

DISCUSSION.

Dr. G. PERNET: I agree with Dr. Sequeira that this is a very malignant case of lupus erythematosus. The case on which I based my monograph on primary lupus erythematosus acutus[1] ran a somewhat similar course to this. I regard the prognosis in these cases as very bad. My own case was a male, the other nine to which I referred were females, and all the cases except one (a female) died. The patient who recovered was treated with quinine, which seems to be the only effective treatment for the condition. I made a point in my thesis of referring to the absence of albuminuria in my series. This differentiates from the better known albuminuric type. There were hæmorrhages in the skin in some of the cases I recorded.

Dr. GRAY: I do not think all the cases of the condition which have hæmorrhagic bullæ necessarily die. Six or seven years ago I showed here a case[2] which had hæmorrhagic bullæ, and she entirely recovered from the acute attack, but had one or two sub-acute attacks subsequently.

Case of Superficial "Card-like" Rodent Ulcer.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a man, aged 43, and states that the disease began with a small lesion, the size of a pinhead, which bled freely. He has had no local treatment, beyond ointments, and from the date of its being first noted, eight years ago, the diseased area has slowly spread, so that at present a patch the size of about 1 in. by ¾ in. is to be seen on the left cheek near the angle of the mouth. This patch is partly made up of atrophic scar and very superficial rodent tissue of card-like thinness. In addition to the local lesion there is a hard glandular enlargement in the submaxillary region on the right side. The features of interest are the curiously superficial character of the thickening and the apparently spontaneous atrophy which has taken place in parts of the site of disease.

DISCUSSION.

Dr. J. H. STOWERS: I agree with Dr. Little's diagnosis in this case. It will be instructive to learn the effects of treatment at a later date. I presume that the lesions will be submitted to the influence of radium as the most reliable method at our disposal at this time.

[1] Pernet: "Le Lupus Erythémateux aigu d'emblée," 1908.
[2] *Proc. Roy. Soc. Med.*, 1913-14, p. vi (Derm. Sect.), p. 42.

Dr. GRAHAM LITTLE (in reply): I am not very decided as to what to do here, as the lesion seems to be undergoing spontaneous cure. Apparently it has been more prominent than it is now, and much of the tissue there now is scar tissue.

Dr. GRAHAM LITTLE also showed:—

(1) A Case of Calcinosis with Pigmentary Papules.

(2) A Case of Generalized Superficial Rodent Ulcer.

Which will be duly reported in a later issue.

Case of Multiple Pigmented Moles.

By S. E. DORE, M.D.

THIS patient, aged 20, has numerous pigmented moles scattered over the forearms and face. The moles vary in size and shape, some being hairy and others glabrous, and one or two moles are surrounded by a ring of pigmented hair follicles, producing a stippled appearance.

There is no question as to the diagnosis, but there has been some difference of opinion as to the best method of dealing with the case. I took her to the Radium Institute, but the opinion there was strongly against radium treatment owing to the slowness of the radium reaction and the possibility of stimulating the growths. I also had the advantage of the opinion of a dermatologist, who took the view that electrolysis and freezing with carbon dioxide snow, owing to the disintegration of the tissues produced, were more likely to lead to malignant changes than radium treatment.

My original intention was to remove some of the smaller lesions on the arms by electrolysis or carbon dioxide snow as one would do in the case of single moles on the face, but in view of the difference of opinion expressed, I thought it would be interesting to hear the opinions of members of the Section on the treatment of such cases.

DISCUSSION.

Dr. J. H. STOWERS: Treatment by limited but repeated applications of CO_2 is the most promising in this case. Much care should be exercised and intervals of time should be allowed between the applications. I have never seen any unfavourable result from its adoption and in my experience it is more efficient than electrolysis, especially when the deposits are multiple and of considerable size. A limited number only should be dealt with *at one time.*

Dr. H. W. BARBER: Some months ago I saw a man, aged 28, with a mole in the centre of his forehead. Three months previously it had been treated with carbon dioxide snow by his own doctor. When I saw the lesion it was an obvious melanoma, and he already had glands enlarged in front of his ears and in his neck. He died with secondary deposits. Of course, it may have been malignant before the snow was applied.

Dr. MACCORMAC: The case exhibited by Dr. Dore raises many interesting points. He spoke to me about the condition some days ago, and I then expressed the opinion that, generally speaking, interference by means of active applications such as the cautery, CO_2 snow, electrolysis and the like, might be followed by a malignant change with dissemination and death. An instance of this disaster has come to my notice, the patient being a young woman who was treated for a pigmented mole on the cheek by the cautery. It would appear therefore, that such remedies are under certain circumstances capable of producing great harm. On the other hand I cannot see that radium is likely to act in such a manner; of course directly malignancy has arisen this remedy is contra-indicated, and it is possibly in consequence of the consideration of such cases that the treatment with radium has been deemed undesirable. It is well known that melanotic carcinoma is one of the most malignant of all malignant processes, and that widely disseminated metastases may arise while the primary lesion remains but little altered. These facts have undoubtedly influenced those dealing with moles, and while untoward results are fortunately infrequent, the possibility of such accidents has always to be considered.

Dr. J. H. SEQUEIRA: I have treated many cases of pigmented moles by carbon dioxide snow, and I have not seen any untoward results in them. But we ought to realize what the President has previously emphasized, that some pigmented moles appear to be much more malignant than others. How we are to distinguish them I do not know.

Dr. KNOWSLEY SIBLEY: The simplest method of treating cases such as this is to paint the mole over with trichloracetic acid (100 per cent.). After a week or ten days, a dry scab separates and leaves the mole distinctly paler than before. This procedure can be repeated as often as may be desirable. In the case of hairy moles or moles with any isolated hairs, these should first be destroyed with electrolysis, after which the area may be painted with trichloracetic acid if necessary. My experience of treatment by CO_2 has been that the immediate results are apparently satisfactory, but the pigmentation is very apt to reappear after a time.

The PRESIDENT: I have treated many moles by various methods. Radium I regard as a very unsatisfactory means in the case of moles, because unless one goes to such a depth that there is produced a telangiectatic scar one fails to cause the disappearance of the mole. I have recently sent a case with a mole on the nose to Dr. Pinch, at the Radium Institute, and he would not touch it. By radium you must destroy the whole skin at the site if you are

to cure the mole as the action is not very selective. CO_2 does it if you freeze deeply enough. I do not believe in frequent freezings so much as in one deep freezing, by which the whole area is necrosed to a considerable depth. Electrolysis is a very laborious process, and if the moles are hairy, it requires skill. We think of the thousands of moles we have electrolysed and we see the patients afterwards, who show no trouble, but we cannot say that electrolysis disseminates the cells much. Where there is hair, I think electrolysis is the best method, because the hair follicles go down to a great depth. It does not necessarily mean that interference with a mole causes malignancy. In my own case I have snipped half a mole out and left the other half in, and it has made no progress. If you are going to employ a method such as CO_2, which leaves a lot of scarring, I do not see why you should not take a knife and remove the moles that way. I have never yet known a mole which grew down below the level of the true skin, and the scar left is only like that left after rupia—just a flat white scar. It is a very simple method, and, I think, a safe one.

Case of Granuloma Venereum.

By C. Firmin Cuthbert, F.R.C.S.Ed.

You have seen the scar resulting from this case of ulcus molle serpiginosum or granuloma venereum, and I propose to read the notes of the case as it occurred to me and then to offer a few comments.

The patient was first seen by me on May 22, 1918. He had returned from Rio de Janeiro on August 23, 1917, with a spreading phagedenic ulcer of the penis, which was so damaged that the whole penile urethra was destroyed down to the posterior part where a fistulous opening communicated with the bladder. He had control of his urine so that the opening was in front of the triangular ligament and the compressor urethræ was normal. The ulcer had spread down over the root of the penis and on to the pubes for 3 to 4 in., it had also spread into the fold of the groin on both sides. The patient had gonorrhœa twenty years ago with double inguinal buboes, but these healed up and the present ulcerated condition developed from a soft sore some years later. He had had two or three Wassermann reactions done with negative results before I saw him, and subsequently another was done after a provocative novarsenobillon injection, also with negative result.

On November 3, 1918, I cut away all the ulcerated surface and brought the edges together by undercutting the skin and subcutaneous fat well up on to the abdomen to meet the under part of the scrotum,

bringing the urethra out through a stab wound in the scrotum and stitching there. The upper part of the wound broke down and gradually assumed all its previous condition of indolence, but the urethral opening remained patent. After this I tried treatment of all sorts: Malachite green paint 1 per cent., brilliant green ointment, X-ray exposures and applications of collosol argentum, all without material effect.

On February 24, 1919, I gave him an intravenous injection of 2½ c.c. of 1 per cent. tartar emetic, and on February 27 and March 2 5 c.c. of the same solution as recommended by Arragoa, whose paper was kindly sent me by Dr. Graham Little, with the result that rapid healing occurred. He had altogether ten intravenous injections of tartar emetic in doses of 5 c.c. to 12 c.c.

The Clinical Research Association on October 28, 1918, reported that there was no evidence of rodent ulcer or other malignant growth: the infiltration was purely inflammatory, not tuberculous, and had no special characters which might suggest its origin.

Whether this case is to be labelled ulcus molle serpiginosum or granuloma venereum must be decided by the opinion of this Section.

The ulcer arose as a sequence of soft sore; and the additional history of suppurating buboes, which occurred twenty years previously, seems to be rather in favour of ulcus molle serpiginosum. On the other hand, on the authority of Dr. Graham Little, ulcus molle serpiginosum goes on "uninterruptedly from the breaking of the bubo," whereas this ulceration did not commence for many years after the healing of the buboes. Arragoa distinguishes ulcus molle serpiginosum "by the almost universal bubo prior to ulceration," and says that granuloma venereum is unconnected with syphilis, soft sore or gonorrhœa. A soft sore certainly did precede the phagedænic type of ulceration in this case. The two conditions seem clinically to be very similar and their treatment also seems to be much the same.

September, 1919: His ulcer remains perfectly healed.

DISCUSSION.

Dr. GRAHAM LITTLE: Mr. Cuthbert was kind enough to write to me about this case, which, from my description of that disease, he diagnosed as ulcus molle serpiginosum. I replied that one of the features fairly constant in that was that the bubonic ulcer was a continuous process, that there was no interval between the breaking down of the bubo and the commencement of the

ulceration. The presence of an interval between the two processes in this case made me somewhat doubtful as to its nature. I had not then seen either the case or the picture of it. I suggested later that he might try injections of tartar emetic, on the chance that it was granuloma venereum. That was done, and apparently the effect has been that the condition has cleared up.

Dr. CASTELLANI: I have not seen the patient, therefore it is difficult for me to give an opinion of any value on the case. I think, from the history of the case which we have heard, and from the fact that the disease started in a country where granuloma venereum is quite common, and also from the fact that tartar emetic has done good, that it is most probably a case of granuloma venereum.

Case of Mycosis Fungoides.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a man, aged 55, of robust build. He appears to have noted some disorder of the skin, and to have obtained admission to a country hospital, in the latter part of 1917, and from this date there has been a steady increase of the disease. He is at present a typical example of the "homme rouge stage," the entire integument being reddened and infiltrated from head to foot. There is a dusky tint mingled with the redness, almost suggesting an argyria, but with no evidence of that condition. There are no tumours anywhere, but there is great enlargement of glands all over the body, especially in the groin, where they are of the size of pullets' eggs. The skin is moderately exfoliated and itchy, but there is no exudation from it. A small enlarged gland was excised from the chest wall and was found to be loaded with pigment cells to a most remarkable degree. The differential blood count showed no abnormality.

Dr. F. PARKES WEBER: I think this is not mycosis fungoides but will turn out to be a typical case of Kaposi's "lymphodermia perniciosa." I believe that, if the man lives long enough, the blood will show definite signs of leukæmia, though possibly without any very great increase in the total number of white cells in the blood. The almost universal diffuse infiltration of the skin and the enlargement of the lymphatic glands in the axillæ and groins are very characteristic of such cases.

Extensive Eruption occurring after the Prolonged Administration of Organic Arsenic.

By HALDIN DAVIS, F.R.C.S.

A. B., A SOLDIER of the Army of Occupation on the Rhine, contracted syphilis, for which he was admitted into hospital on April 26, 1919, with a primary lesion only. He remained under treatment until the end of June, and received six injections of novarsenobillon intravenously and seven injections of mercury intramuscularly. No secondary symptoms are said to have appeared. In the middle of July he presented himself for treatment at the Venereal Department of the Royal Free Hospital, suffering from small ulcers on the legs. These were regarded as syphilitic in nature, and between that time and December 11 he had twelve injections of neokharsivan, and also took by the mouth a large number of pills of hydrarg. cum cret. During October he developed the rash for which he is now being exhibited to the Section. He has had no iodide. At the present time the rash has faded, and practically nothing now remains of it except residual pigmentation. When it was at its height it consisted of macules and papules varying in size from that of a threepenny-bit to that of a sixpence, regularly distributed over the trunk and limbs, both on the flexor and extensor aspects. The face, hands and feet were free from the eruption, but the skin of the hands and feet peeled, and deep cracks appeared in the palms and soles, which were very painful. The papules were very scaly, in fact at one time the quantity of scales shed was so large as to be comparable with that produced in a case of exfoliative dermatitis. A remarkable feature of the eruption was the intense pruritus. This was so great that it kept him awake at night. The colour of the rash was not so much coppery as violet, in fact it matched that of lichen planus. The patient also has enlarged shotty glands in the groins, axilla, and the epitrochlear region.

Active treatment was discontinued on December 3, the date of his last intravenous injection. Since then he has had no drugs internally, and only simple zinc cream for external use. Since December 11 he has been in bed. The rash has faded rapidly and the pruritus has been greatly relieved. In July the Wassermann reaction was positive, but now it is negative.

The question is, Is this eruption a secondary syphilide, appearing notwithstanding the prolonged antisyphilitic treatment, or is it a case of lichen planus, or is it due to the administration of arsenic continued over so long a period? The patient is certainly saturated with arsenic, for it has been found in his hair, teeth and urine.

DISCUSSION.

Mr. H. C. SAMUEL: I think this is a secondary syphilide. It is extraordinary that it should not have cleared up after the injection. I had a case some time ago in which the rash was, in fact, made worse by salvarsan, and I remember another case in which, in spite of numerous injections of salvarsan, a typical follicular syphilide developed, that proved absolutely resistant to the treatment.

Dr. H. G. ADAMSON: I do not think the keratosis is arsenical. The condition of the palms, the pigmentation left by the eruption and some papules now present on the forearms suggest to me that the eruption may have been lichen planus.

Dr. J. H. STOWERS: It does not seem possible to give a definite opinion about this case as the result of one observation alone, but it appears to me to be an unusual development consequent upon the internal administration of arsenic, the action of which varies in different subjects. I certainly cannot look upon it as a manifestation of syphilis in spite of the suggested similarity.

Case of Granuloma Annulare.

By HALDIN DAVIS, F.R.C.S.

THIS boy, aged 14, has suffered during the last year from a curious eruption on the fingers. It started on the thumb as a shield-shaped infiltration, and since then it has spread to other fingers. There is poor circulation in the hands, though he says he does not have chilblains. He also has a red nose. There is no history of illness. My provisional diagnosis is granuloma annulare, although I am prepared to admit that the appearances differ considerably from typical examples of that condition.

Ringed Eruption of the Finger—Granuloma Annulare—of exceptionally long Duration.

By G. W. SEQUEIRA.

THE patient was a young man, aged 20. On the ring finger of the left hand he presented an irregular nodular eruption, which had been present for seventeen years. It is not ringed in character at the present time, though it may have been at an earlier period, but owing to involution may have resolved into irregularly placed nodules. The skin between the nodules is depressed and atrophic. The affection was first noticed when the patient was aged 3. At the age of 13 he attended the London Hospital under the care of Dr. J. H. Sequeira. He had X-ray treatment, but the patient says it had little or no effect on the size of the nodules. There have been no subjective symptoms throughout. He has always been delicate, and attended a chest hospital for some time when he was 10 years of age.

Dr. ALFRED EDDOWES: There is an apparent want of development in the left hand, which seems to be about half the weight of the right one. I think it is a tropho-neurosis.

Seborrhœic Dermatitis of Unusual Severity following an Attack of Urticaria ab Ingestis.

By G. W. SEQUEIRA.

THE patient, R. B., aged 23, has suffered from the present eruption for the last two years. It commenced suddenly at Christmas, 1917, after partaking of some army tinned rabbit when he was stationed at Eastchurch, Isle of Sheppey. He felt ill soon after eating the rabbit, and had vomiting and diarrhœa, and at the same time came out in an eruption which was very irritable over his chest and abdomen. Three comrades were similarly attacked at the same time. The patient says there were "bumps" like nettle stings, which caused him a great amount of discomfort from the irritation. He was confined to bed for two days on account of the severity of the symptoms. The eruption, he says, spread quickly to other parts of his body, and eventually took

on the characters it now presents, but was much more severe than it is now. He was given fifteen autogenous vaccine injections, but derived no benefit from them; in fact he states emphatically "they made me much worse." Later, he had two injections of collosol manganese, again without benefit. He has always had a scurfy head, and was treated for what was called eczema of the head for several months when he was three years of age. At the age of 14 he had "a red rash all over his body," which was at first thought to be scarlatina, but proved to be something else, probably erythema scarlatiniforme.

Case of Superficial Epithelioma.

By H. C. Semon, M.D.

This patient came to me in March last with an obvious ulcerating neoplasm on the left cheek. The upper portion of this growth can now be seen to be healed, but the lower part is still spreading, though slowly. He has had the condition ten years: it started as a pimple, which he scratched, and it gradually grew. The only symptom all the time has been itching, and that still persists. In the lower border there are still the pearly nodules which are characteristic of the condition.

We are dealing here with the superficial type of basal-cell epithelioma, a slow benign variety, easily treated. I have given this patient four applications of X-rays, two Sabouraud doses at a sitting, with a shield of ½ mm. aluminium, and three applications of CO_2 snow.

Postscript.—When last seen on December 31, 1919, the full effect of the last ray exposure had taken effect, and a flat healthy scar was all that remained to indicate the site of the long-standing lesion.

Section of Dermatology.

President — Dr. ARTHUR WHITFIELD.

Grouped Comedones in an Adult.[1]

By E. G. GRAHAM LITTLE, M.D.

THE patient is a schoolmaster, aged about 45, who gives the history that the eruption made its appearance shortly after he had an attack of "bronchitis," for which he rubbed "camphorated oil" into his chest. The eruption has persisted in much the same condition as now seen since last May, when he saw me for the first time. The eruption consists of two types: (1) An uninflamed comedo, astonishingly like the comedo of acne vulgaris, but differing from it in being much less easily expelled by pressure, and careful examination did not discover the acne bacillus. (2) Many of the comedones have become inflamed and even pustular. The eruption occupies the whole of the front of the chest wall, and is thickly grouped in this area, numbering many hundreds of comedones.

I have not seen an exactly similar condition in an adult but have met with an eruption indistinguishable from it in children, with an exactly similar history, of onset after rubbing in camphorated oil into the chest, and the distribution has followed the probable site of friction. The present case has proved very refractory to treatment with soap liniment, and as I have said, the eruption has changed little in six months. I shall be glad of suggestions as to treatment.

DISCUSSION.

Dr. BARBER: I ask whether Dr. Little has tried an ointment or a paste containing a large proportion of salicylic acid. In these cases in children I have found the best treatment is to exfoliate the skin with a paste containing

[1] At a meeting of the Section, held January 15, 1920.

20 gr. of sulphur and ½ dr. of salicylic acid to the ounce. I am afraid the scarring will persist: it has in my cases, which have all been in children.

Dr. MACLEOD: I have recently had an extensive case of grouped comedones about the neck in a child, which also resulted from camphorated oil. It was more acute than Dr. Little's case and was associated with large pustular lesions. The treatment adopted consisted of opening and evacuating the lesions, boric compresses, and finally calamine cream, and under this the affected area has gradually healed. With regard to grouped comedones in the adult, I have lately had a case in which they were present on the scalp in a man addicted to excessive use of an oily brilliantine.

Dr. S. E. DORE: I think this may be attributed to an infection of the follicles which are plugged with grease, and similar in nature to the multiple comedones of children, due to the wearing of greasy caps, rather than to a specific effect of camphorated oil or turpentine. The treatment is the same as that for ordinary acne, by means of sulphur lotions, &c. I should think X-rays would be valuable, but I have no personal experience of their effect in these cases.

The CHAIRMAN (Dr. J. J. Pringle): I ask the opinion of radiologists as to whether X-rays would not be a good method of treatment to start with in order to diminish the induration; I think it would be a suitable prelude to subsequent treatment by the various methods mentioned by Dr. MacLeod. X-rays are certainly of great help in ordinary indurated acne.

Dr. SIBLEY: I would apply X-rays, and then give him sulphur lotion to rub in, and not apply any more liniments or ointments.

Dr. GRAHAM LITTLE (in reply): I have been using a sulphur and soap ointment, but without much benefit, though there is certainly less inflammation. He came in a high state of discomfort from the inflammation of the chest wall. The condition seems to be very uncommon in adults. In children it is my experience that practically three-fourths of the cases are in boys.

Case of Apparent Diphtheroid Infection of the Skin.

By H. W. BARBER, M.D.

H. A., MALE, aged 48. History: The patient gives no history of previous illness of importance: he has never had dysentery, nor has he been in the East. He was a sergeant-cook in the British Expeditionary Force in France, and was employed in cutting up meat. His present eruption began in February, 1918, as a "boil" on the dorsum of the

left wrist, from which it spread downwards on to the back of the hand, and upwards for a short distance along the back of the forearm. This original lesion is now represented by a scar with keloid thickening. He was first treated at the Canadian Hospital in France, and was subsequently sent home to England, where he received treatment in various hospitals. A second lesion appeared on the right leg in June, 1918: this was treated with X-rays, which seemed to cause it to spread more rapidly.

In January, 1919, he was transferred to Rochester Row Military Hospital, where he came under the care of Mr. C. H. Mills, to whom I am much indebted for bringing the case to my notice. At that time there was a large area of indolent ulceration on the lower half of the inner surface of the right calf, on the outer surface of which was the scar of previous ulceration. At Rochester Row the case was very thoroughly investigated. The Wassermann reaction was repeatedly found to be negative, even after a provocative dose of salvarsan. Moreover, this drug was found to have no beneficial effect. Lumbar puncture was also performed. The ulceration of the right leg healed gradually, but in April, 1919, the left leg became involved. Blood cultures were negative, and examinations for blastomyces, *Sporothrix beurmanni*, and for streptothrix were made without result.

In August, 1919, Mr. Mills kindly referred the case to me. At that time the lesion on the right leg had completely healed, leaving a pigmented scar. Scars of the other affected areas, namely of the left wrist and around the puncture sites, already referred to, were also present. On the lower part of the left leg and around the left ankle was an extensive area of ulceration, the spreading edge of which was raised, œdematous, and bluish-red in colour (fig. 1). Within this was adherent yellow slough, and the central parts were red, raw, and covered with granulations. The patient was admitted to hospital on September 2, and hot saline dressings were applied to the ulcerated area, the patient being kept at rest in bed. By September 17 the lesion had healed, and he was discharged. During this time repeated search for blastomyces and streptothrix proved negative. Shortly afterwards the ulcerative process began again a little higher up the leg (fig. 2), and the patient was readmitted to hospital on October 17. The investigations detailed by Dr. Knott were then begun.

In spite of treatment by eusol soaks, and later by a continuous eusol bath, the lesion spread rapidly up the leg, leaving a huge area of raw granulations, over which the epithelium gradually

grew, thus forming warty, papillomatous projections on the healed surface.

At the end of November an injection of autogenous vaccine, made from the diphtheroid bacillus, was given, and this was repeated a few days later. Both injections were followed by slight constitutional



FIG. 1.

Showing a recently-healed area of ulceration. The lesion spread from below upwards. The warty projections, produced by the granulations over which epithelium has grown, are well seen. Around the ankle the hyperkeratotic scars of old lesions are shown.

reaction, with a little rise of temperature and pulse-rate. The ulceration then healed with amazing rapidity, and the patient was discharged on December 6. Subsequently the ulceration began again on the upper and inner surface of the left calf, as is seen now.



FIG. 2.

Showing an actively spreading area of ulceration. Observe (1) the inflamed, œdematous area, covered by unbroken skin; (2) the ragged edge; (3) the adherent slough; (4) the bright red, prominent granulations exposed by the separating slough; (5) the epithelium beginning to grow over the granulations.

A BACTERIOLOGICAL NOTE (F. A. KNOTT, M.B.LOND., D.P.H.).

When in hospital and previously as an out-patient, several direct films of pus and exudate from his ulcers were sent to the Bacteriological Department, and there examined for the presence of yeasts or mycelia,

but on each occasion it was impossible to demonstrate any such elements. Later, with a view to working out the infection in detail, Dr. Barber suggested that we should attempt a complete local examination. Two or three typical lesions were therefore selected and cultures obtained from the deeper skin layers. Taking a series of points starting on normal skin and passing through the peripheral zones to the centre of the ulcer, all care was taken to clean and then to flake off epidermal scales with likely gross bacterial contamination. The superficial layers were then shaved away with a sterile knife point and finally, after cleaning the surface thus exposed by applying the points of a series of sterile swabs, serum or pus, as the case might be, was collected from each in a sharp pointed capillary pipette. From each specimen, cultures and direct films were made. The findings in these may be summarized. The outermost remaining practically sterile, gave a satisfactory control against gross contamination in the more central. As soon as the red inflammatory zone was reached, there appeared both in culture and in direct films, large numbers of the diphtheroid bacillus about to be described. Further, in the leucocytic area, the exudate showed great quantities of the organism actually in the pus cells. Similar films came from the unbroken pustules, but as the open ulceration was reached, although secondary infections naturally abounded, the diphtheroid could be detected still.

The results so far obtained seemed strongly to suggest that we might be dealing with a true diphtheroid infection, where the germ, though not necessarily causal, was at least active in the deeper layers of the skin. Isolation was therefore proceeded with and a pure culture readily obtained. On p. 45 is set out a very brief description of the organism together with, for purposes of comparison, corresponding details of two probably non-pathogenic diphtheroids, *Bacillus xerosis* and Hoffmann's bacillus, grown under the same conditions at the same time and on the same batch of media. Some accepted characters of the typical true diphtheria organism are also inserted.

The purpose of this comparison is to indicate briefly that in the case of the organism under consideration, from its general morphology, staining properties and sugar reactions, we were apparently dealing with a member of the great diphtheroid family of bacilli and one which falls somewhere, about midway perhaps, between the extremes of the true Klebs-Loeffler bacillus and the small non-pathogenic members at the lower end. Such media reactions give however but a relative indication and cannot take the place of animal inoculation experiments in de-

terming virulence. A small amount of pure living agar culture (twenty-four hours) was therefore inoculated intracutaneously into a healthy guinea-pig. A true diphtheria bacillus would probably have caused death within forty-eight hours: this organism did not. The pig was in low condition for about a week, a painful circumscribed swelling formed, developing into a large abscess which finally burst and continued to discharge pus for about three weeks, after which it gradually healed. The main point to be noticed here is that in the original films of this pus, the polymorphs were crowded with diphtheroid bacilli and that a pure direct culture was readily obtained from the lesion five days after inoculation.

Solid media.—Agar and blood agar: Thick, opaque, round, discrete colonies tending to develop a pinkish colour. Size of colony relatively large.

Broth.—A heavy deposit with only faint turbidity in supernatant fluid.

Blood serum.—Slow growth of unusually large colonies.

Gelatine.—Shake, stab, and surface. Thick opaque growth at 22° C.

FERMENTATION TESTS (FORTY-EIGHT HOURS).

Sugar	Diphtheroid (?) (Ashby)	Hoffmann	Xerosis	Diphtheria (K.L.B.)
Dextrose	++	0	0	++
Galactose	0	0	0	±
Maltose	±	0	0	+
Saccharose	±	±	0	0
Glycerin	0	0	0	±
Dextrin	±	0	0	±
Milk	0	0	0	0

In fresh cultures from patient and inoculated animals the diphtheroid showed some involution forms and polar staining with Neisser's stain, both these properties when well marked being customarily considered as belonging to the true diphtheria organism. In this case they were about 25 per cent. positive—i.e., considerably less than commonly seen in true Klebs-Loeffler bacilli. The *Bacillus xerosis* and Hoffmann's bacillus gave a negative result to both.

In all media the growth commonly seen artificially was of more abundant and more opaque nature than either the Klebs-Loeffler bacillus itself or the lower diphtheroids.

From this culture a second guinea-pig was inoculated. An exactly similar result was obtained but the effect on the animal was much intensified. The general condition fell rapidly, an abscess at once formed, broke on the surface and in the third week had become an open indolent ulcer about the size of a sixpence. At the end of the fifth week, the animal died. A post-mortem examination failed to show any signs of septicæmia, there was only marked wasting without sign of intercurrent disease. Again the early abscess pus showed cells containing many diphtheroids and a pure direct culture was readily

obtained. In the final surface lesion secondary infection had occurred but the diphtheroids could still be detected in direct films.

Other observations consisted in making agglutination tests by the macroscopic sedimentation method with pure cultures of the diphtheroid against the patient's serum, two normal human sera, serum from the two inoculated pigs and serum from a normal guinea-pig. Summarized, the results ran as follows:—

Time in water-bath, one and a half hours. Twenty-four hours' culture. Reading at fifteen minutes.

Dilution	Patient's serum	Normal A	Normal B
1 in 20	++++	0	0
1 in 40	++++	0	0
1 in 100	++++	0	0
1 in 250	++++	0	0
1 in 500	+++	0	0
Control	0	0	0

Dilution	Guinea-pig I	Guinea-pig II	Normal pig
1 in 20	+++	+++	0
1 in 40	+++	++	0
1 in 200	++	++	0
1 in 400	+	0	0
Control	0	0	0

These tests show a very definite agglutinating power developed both by the patient and the artificially inoculated pigs.

Another pair of guinea-pigs were next injected from the pure culture obtained from pig II, but on this occasion a dose of 500 units of antidiphtheritic serum was inoculated into one of them subcutaneously at the same time as the organism. Thus pig III received culture and serum, pig IV received culture alone. In these two animals very different results were obtained: pig IV developed an abscess and signs exactly as in the instance of pigs I and II, whereas in pig III no active abscess whatever was produced, and when after five days a very small swelling which remained at the site of inoculation was opened, no pus was found and cultivations failed to give any more than a scanty growth of *Staphylococcus albus*. In the case of pig IV, however, we found large amounts of pus showing the same characters as before. The diphtheroid grew in pure culture from the first discharge and the pig, though not in this instance dying (it was a larger and stronger animal than pig II), suffered considerable wasting and had an open abscess cavity.

More pigs are being inoculated in series and at the time of writing the virulence appears to increase slightly in successive animals.

From the patient's lesions as the relapses occurred, cultivations were made whenever possible from the contents of unbroken pustules, and the diphtheroid was consistently obtained.

To summarize this outline bacteriological examination, one may perhaps justifiably claim the fact that the diphtheroid organism has been proved to play some active part at least in the inflammatory process. That it is actually causal is not proved but comes, we think, within the limits of probability. Of its characters explanation has already been given and the marked inhibitory action of antidiphtheritic serum is noteworthy. In guinea-pigs the organism is pathogenic to the degree of producing active abscesses and a very high phagocytosis on the part of the polymorphonuclear leucocytes, which, together with the constant appearances in direct films from the patient, point to something more than an accidental contamination. The specific agglutinating power is also important.

In conclusion I should like to express my indebtedness to Professor J. Eyre for his interest and criticisms throughout these experiments.

DISCUSSION.

The CHAIRMAN: I am sure we must all congratulate Dr. Barber upon the admirable piece of work which he has presented to us to-day, and thank Dr. Knott also for the extremely exhaustive and, I think, convincing bacteriological investigations he has contributed towards the elucidation of the nature of this puzzling disease. I hope that those who are in a position to discuss such subjects with greater authority than I will speak upon this very important case. I admit that clinically I formed no firm diagnosis as to its nature, and I think I should have done, as was done at Rochester Row, by sending it to someone who could investigate it to more purpose than I. I remember that Dr. Eddowes brought forward a case of a somewhat similar nature some years ago, and I shall be glad if he will remind us of the particulars.

Dr. ALFRED EDDOWES: It is impossible to see a case of this kind without being greatly interested. I ask whether this patient has shown any decided symptom of ill-health, because the first case of the kind I met with (in 1907) was very ill. The patient was a girl, aged 11, who came to the hospital with an echthymatous eruption. There was a bulla on the lip, and that caused me to examine her throat, where I could see there had been a recent lesion on the tonsil: there was no false membrane, but an appearance as if the throat had been blistered. I treated the case in the out-patient department for a few days, and requested the mother to bring the child again soon. At the next visit the body rash was better, but I noticed that the girl seemed to be very ill, and though 11 years of age, she could not walk well.

Therefore I thought she had something much more serious than echthyma or impetigo, and that the case ought to be carefully gone into. We found that, with the streptococcus, which was probably the chief cause of the bulla, there was a bacillus which looked like the Klebs-Loeffler bacillus. Investigation showed that organism was the true Klebs-Loeffler bacillus, and very fatal to guinea-pigs. The patient got well reasonably soon. After that, I was on the look out for similar cases. The next was one of what had been for a long time considered a recurrent eczema. I pass round a picture of it. In that also we discovered the Klebs-Loeffler bacillus with a streptcoccus, and the patient had injections of antitoxic serum. The photographs show the great improvement which took place as a result. During the next two or three years Dr. Hare, who worked hard at the subject with me, and others, met with nine cases. We tried to find out whether any cases of diphtheria had occurred in connexion with them, and the first result of the inquiry was that no case of diphtheria had apparently conveyed the disease to these patients. Afterwards, however, I heard that a child who had been in contact with such a case went home and died of diphtheria in about nine days. You will find a description of the first cases in the *Lancet* of February, 1908. These cases are extremely important from the public health point of view, and I suggest to those who are puzzled sometimes as to the source of diphtheria cases that they should investigate any case of severe chronic or recurring catarrhal or eczematoid condition of the skin in their midst. There was no ulceration of the skin in any of the cases. The lesions were moist and raw. One case surprised me very much. The child was very ill and had vomited, and the vomited material had run down its chest, inside its clothing, and caused a large red patch there, in which the diphtheria bacillus was found. These cases are more chronic and more difficult to manage than those in which only the streptococcus is present, and are more liable to relapse.

Dr. MacLeod: There was a typical case of veldt sore in the Seamen's Hospital a short while ago under Dr. Manson-Bahr, of which he kindly gave me a photograph. The ulcer, which was situated on the arm, had proved intractable to local treatment, but in the hospital it healed rapidly as the result of injections of antidiphtheritic serum.

Dr. F. Parkes Weber: A paper was read before the Section of Neurology (January 8, 1920) a few days ago on an epidemic of cutaneous sores in Palestine, supposed to be due to the Klebs-Loeffler bacillus (though often stated to be due to a diphtheroid bacillus), sometimes followed by peripheral neuritis. It has been suggested that the so-called "veldt sores"[1] are due to the diphtheria bacillus, and that afterwards the diphtheria bacillus tends to become displaced by septic cocci.

[1] Cf. C. M. Craig, "Desert, Septic or Veldt Sore amongst European Troops," *Lancet*, Lond., 1919, ii, p. 478.

Dr. MACCORMAC: If I correctly understand Dr. Eddowes and others who have spoken about this case, they have referred to instances of true diphtheria of the skin, that is to say, conditions in which the Klebs-Loeffler bacillus has been detected, and is responsible for the lesions. But the case shown by Dr. Barber is not supposed to be of such a nature: bacteriological examination has shown the existence of a diphtheroid organism, apparently pathogenetic. I am particularly interested in this, because I have found many types of diphtheroids, especially among the skin affections of soldiers during the war. Some of these were so closely akin to the true bacillus, that no morphological difference could be detected, even clubbing and beading being well marked. They appeared however to be only of secondary importance, the lesions in which they were found arising from streptococcal infection.

Dr. GRAHAM LITTLE: Twenty years ago I met with an epidemic of cases of the kind among children, at the Children's Hospital, Shadwell. There were eight or nine cases with a very ulcerated condition, and the hospital pathologist uniformly reported a pure culture of diphtheroid organisms. There was a curious resemblance about the cases. In one instance two brothers were attacked, and had the same organism, one of them dying of acute septicæmia. I will look up the notes.

Dr. MALLAM: I cannot help being impressed in this case by the exhibition of pain, which appears to be out of all proportion to the lesion. I, of course, admit that the man has a diphtheroid affection of the skin, but I strongly suspect him of aggravating it by artificial means, especially as, being a pensioner, it is to his advantage to do so.

Dr. SIBLEY: We have all seen diphtheroid skin troubles, in which the lesions have been superficial, and have not left scarring, whereas this man has considerable hypertrophied scarring, especially about the ankles. This makes me ask whether Dr. Barber has excluded yaws?

Dr. G. PERNET: I am reminded of a case under Radcliffe-Crocker some years ago. The patient was a little boy, who came under observation for a gangrenous lesion near the anus, and of the type of dermatitis gangrenosa infantum. It was treated by local injections of 1 in 30 carbolic, and did very well. It was then noticed by the nurse that the child was not swallowing its liquid food properly. This was found to be due to paralysis of the palate, and the child was transferred to a medical ward. The idea of the skin lesion being diphtherial in origin occurred to me. Antitoxin was injected as soon as possible, but unfortunately paralysis of the diaphragm set in and death ensued.[1]

[1] Discussion on G. W. Dawson's paper on "Cutaneous Diphtheria," *Brit. Med. Journ.*, ii, 1910, p. 860.

Dr. BARBER (in reply): In answer to Dr. Eddowes' question as to the patient's general health, the last time he was in hospital, when the condition of his leg was serious, there was considerable general malaise, though the temperature did not rise beyond 100° F. I think this was chiefly due to lack of sleep. When the lesion healed, he felt quite fit. I naturally considered the possibility of an artefact, as I always do in cases in which the diagnosis is not clear, but there are many points against that view. One is that I know of no means whereby lesions of this nature could be produced. Moreover, when the patient discovered that new lesions developed at the sites of the injections, he refused to have any more, and he required a good deal of persuasion before he would allow us either to take cultures or to give the vaccine. I admit yaws had not occurred to me as a possibility, but he has never been in the East, and he has had salvarsan.

Case of Leprosy demonstrating the Value of a Tissue Vaccine.

By A. WINKELRIED WILLIAMS, M.B.

I FIRST saw this case in March, 1914; it was a severe nodular leprosy, so marked that a diagnosis was made as he entered the consulting room. The patient showed marked protuberance of the eyebrows, broad nose, great swelling of ear lobes, nodular masses all over his face with considerable pigmentation, and infiltration of sclerotic conjunctiva. The body and limbs had great numbers of flattened nodules, and areas of deep and brown-yellow pigmentation, also whitish areas. Pain sense deficient, anæsthesia more or less complete on arms, especially ulnar side of forearms, complete anæsthesia of anterior aspect of lower extremities for about one-third above and below the knees. A large painless ulcer over the knee showed no signs of healing. Constant nasal discharge; cough, with profuse expectoration. Ulnar nerves very large and insensitive to pressure. The nasal discharge and sputum swarmed with grouped acid-fast bacilli. A nodule excised without pain from the chin showed typical infiltration with lepra cells and lepra bacilli.

It is now practically impossible to find any distinct leprosy, the nearest approach being some thickened patches in the corium of front of thigh, which feel like fibrous tissue, and the skin is normally sensitive to pain. No bacilli can be found on swabs from nose or throat. There is no nasal discharge nor sputum. The patient is perfectly fit in every way and apparently in vigorous health and is most anxious to get married; and the lady knows of his malady and is willing to risk it. I promised him to get the opinion of the Section whether he might do this.

Personally I am convinced that this improvement *or cure* is the result of a vaccine treatment. The vaccine was made on a plan I devised as follows: One half of a large excised nodule was at once placed on a desiccator over sulphuric acid, the other half examined to see if it showed plenty of bacilli; when desiccated, part was reduced to a dry horny mass; it was ground up with sand, emulsified, and the sand removed by gentle centrifugalization, and the emulsion sterilized like an ordinary bacterial vaccine. I base my opinion on the following evidence:—

For several months before commencing the vaccine treatment I treated him with large doses of ol. gynocardiæ by mouth, boric nasal douches and perchloride of mercury spray and inunction of nodules with an ointment of ammoniated mercury and ichthyol. Practically no improvement resulted, the ulcer on knee showed no signs of healing; there were febrile attacks, with fresh erythema and nodules.

The first dose of vaccine was too small to produce a visible reaction, but a week later the nasal discharge and sputum were greatly reduced; the knee ulcer was healing rapidly, only two small scabbed lesions were left. In four weeks he had a second dose (double the amount of the first); this was followed by a reaction—i.e., no temperature, but the eruption of patches of erythema, which later faded, left slight permanent pigmentation but no nodules.

The ulcer healed completely, the nasal discharge and sputum stopped entirely, swabs from the nose and throat showed no leprosy bacilli and all the nodules became small, many disappearing entirely; sensation to pain began to return and the ulnar nerves became sensitive to pressure. He continued to take a monthly dose of the vaccine.

The patient went to the Essex Home; I saw him monthly and later he had vaccine sent to him.

About two years ago our stock of vaccine went wrong and the vaccine was stopped for several months but he continued the ol. gynocardiæ as before. Then new crops of erythema developed and some became nodular, and the knee scar broke down. I excised a large piece of skin from the arm which included a new nodule and the pigmented site of an old one, but lepra bacilli were absent so I could not make a vaccine. Dr. Turner then kindly sent me a nodule excised from a bad case of nodular leprosy which had been admitted to the Essex Home. A new stock of vaccine was made from this by Dr. Galt. For many months doses were given every four weeks and the patient greatly improved; a slight reaction followed each dose as before. The

case became stationary; a few nodules could be felt still on arms and legs. I then gave the vaccine every two weeks, and later every week, and these nodules cleared up; he now gets no reaction at all from the vaccine.

I showed this case before giving the vaccine in 1914 and again after giving the first doses of vaccine in 1915.[1]

The view generally expressed was that the results of the vaccine were very good, but as Dr. Pernet expressed it: "All sorts of treatment do good at first in the nodular variety, but unfortunately disappointment is the usual event."

I am strongly of opinion that this treatment should have a wider trial given to it. If I had the opportunity of treating other cases, from my experience in this case I should act as follows:—

First, test with minute doses until a reacting dose is given—make this the unit and give it at first every month for two or three months; then if patient is doing well give it in more frequent doses for three, to two to one week's interval between the doses, and I would hope to get a good result more rapidly than was obtained by the very cautious treatment first given.

DISCUSSION.

Professor CASTELLANI: It seems to me there is a most remarkable improvement. As regards the question of diagnosis, I do not think there is any doubt, considering the diagnosis was made four or five years ago when the symptoms were absolutely clear. You probably know that the malady is extremely common in Ceylon: we have in Colombo the largest hospital for leprosy patients, where there are more than 3,000 of them, and in private practice we see cases of the disease practically every day. I believe that even now in this patient the diagnosis of the condition could be made. I do not believe complete cure has occurred: I think his improvement merely means that the infection is dormant. I was very interested in the treatment described by Dr. Williams, because in 1905 Dr. Nichols and I carried out precisely the same treatment in a number of cases in Colombo, India and the Federated Malay States—that is to say, with a vaccine prepared by triturating the nodules. We thought, at first, that this treatment gave good results, but, after a time, we concluded that the improvement had only been a temporary one. In my experience, there is no specific for leprosy: there is no vaccine and no drug which will cure the disease. But I do believe we have an extremely useful drug in chaulmoogra oil, provided it is used in large doses. We must give almost massive doses—20, 40, even 50 minims, three or four

[1] *Proceedings*, 1913-14, vii, p. 170; 1914-15, viii, p. 38.

times a day. I have now under my treatment two cases, and I must say both have done very well on chaulmoogra oil, though, I would say again, it is a palliative treatment, not a cure.

Dr. GRAHAM LITTLE: I support what Professor Castellani says because about fifteen years ago we had an opportunity, at St. Mary's, of observing the results of exactly the same treatment in the case of a leper whom, I think, I showed to the old Dermatological Society of London. He was under Sir Almroth Wright for many months, and this treatment was followed with the technique Dr. Williams has described. There was improvement of a remarkable character, but it proved to be only temporary, and he rapidly relapsed while the injections were being continued. Finally it was relinquished, and other things were tried.

Dr. G. PERNET: I also have tried nastin for leprosy, and found it useless. We must remember that lepers often undergo changes for the better if they are placed in healthy conditions and surroundings. If this patient were under my care I would give him plenty of chaulmoogra oil. The prognosis for maculo-anæsthetic leprosy in Europeans in England is good as compared with the nodular type of the disease.

Dr. WINKELRIED WILLIAMS (in reply): I would point out that this man was under active treatment several months before the vaccine was given and there was no improvement. The first doses of vaccine were given before the patient went to the Essex Home, i.e., under the same local and general conditions, and rapid improvement at once set in. When the vaccine was left off for some time he relapsed, but there was improvement at once when the vaccine was renewed; when the doses were increased the improvement was so great that practically all evidences of leprosy disappeared. These facts, I think, show that the vaccine is the active agent in the good results in this case.

Case of "Erythème induré des jeunes Filles," associated with Folliclis and Phlyctenular Ulcers.

By GEORGE PERNET, M.D.

THE patient is a girl aged 14, who first attended my out-patients' department at the West London Hospital on January 6, 1920, for erythema induratum (Bazin's erythème induré des jeunes filles,—des scrofuleux) chiefly of the right calf, the lesions being rather flatly nodular with a tendency to evalescence on the surface. On the backs of some of the fingers there are typical so-called folliclis lesions. She has been under Mr. Harman for some time for phlyctenular ulcers, and she suffers from chilblains. Her mother attended the West

London Hospital some years ago for tuberculosis of the ankle. A brother is in an army hospital in Kent for (?) abdominal tuberculosis.

The patient is employed at a tyre works and stands all day at her occupation. It is proposed to put her on chaulmoogra oil. At present she is having cod-liver oil and maltine. The duration of the erythema induratum is said to be fourteen days, but it must be more than that, judging from appearances.

DISCUSSION.

Dr. A. EDDOWES: The most interesting point in the case is the presence of the nodules (folliclis) on the fingers, and I think the case clearly shows the direct connexion between this condition and lupus erythematosus, because similar nodules are frequently seen on the fingers when lupus erythematosus exists in the most typical form on the face.

Dr. MACLEOD: I consider that certain of the cases labelled "erythema induratum" are definitely tuberculous, and think that they are due to the presence of tubercle bacilli, probably about the subcutaneous veins. I do not regard them as tuberculides in the true sense of the word.

The CHAIRMAN: In connexion with the point raised by Dr. Eddowes, I would remind him that lupus erythematosus of the fingers presents a completely different picture. I have never seen "folliclis" lesions such as this associated with typical lupus erythematosus of the face. I think that co-existent multiform tuberculous manifestations on the skin, such as this girl presents, are not uncommon: and if I find a "tuberculide," or a manifestly tuberculous lesion on one part of the skin, I invariably examine other parts.

Case of Multiple Rodent Ulcer.

By AGNES SAVILL, M.D.

I FIRST saw this woman in November, when she came on account of the eruption you see now. The patches were covered with scales, and the first time I saw her I did not diagnose the condition, but I gave her the ordinary treatment for seborrhœic dermatitis. A fortnight later a big patch on the abdomen showed clearly a raised ridge round it; I regarded that lesion as rodent ulcer, and watched the others. She came rather irregularly. When I saw her towards the end of December there was no doubt about the diagnosis of the other patches on the chest and back. I would ask what is the best form of treatment. Should one use X-rays? If so, what should the dosage be? and should the X-rays be filtered or unfiltered? Or is radium better? Following the

application of sulphur and mild mercurial ointment all the scales have come off, and a little thyroid has made her feel better in a general way. Her age is 43, and her ovaries were removed two years ago on account of cystic disease. These patches have developed since that operation. The family history is not a good one; she is one of three, two of whom died young, and she herself has never been strong.

DISCUSSION.

The CHAIRMAN: Although the lesions present slightly different characters in different localities, there can be no doubt about the diagnosis. The chief point raised by Dr. Savill concerns treatment. My personal experience is that radium is by far the most satisfactory method of treating these scattered medium-sized rodent ulcers. I have had a considerable number of cases, and my custom has been to take them to the Radium Institute, where the treatment has been uniformly successful.

Dr. GRAHAM LITTLE: I am surprised, Sir, that you should regard the diagnosis as obvious. I should have agreed with Mrs. Savill in regard to the difficulty of recognizing the lesions on the chest, if I had not seen the lesion lower down, on the abdomen, which I think must be a very rare type of rodent ulcer, the "flat cicatricial epithelioma" of Darier. The case is exceedingly like one which I regarded as unique,—a case I showed at our last meeting, that of a woman aged 45, who has about fifty lesions of this type scattered over the surface of the body, everywhere except the face. My first opinion in this case was that it was lupus erythematosus, and it was with astonishment I found evidence of definite epithelioma in the sections. Curiously, during the last week I have seen another case exactly like that I speak of in a lady aged 50, who was seen eighteen months ago by Dr. Adamson; he has since informed me that he was then hesitating between the diagnosis of (1) lichen planus annularis, (2) rodent ulcer, (3) lupus erythematosus. I think all the three cases I have now seen are rodent ulcer of an exceptional type. It is an extraordinary coincidence that I should have seen three cases of the kind in so short a time, and yet been seeing skin cases all these years without having inspected a similar one at all.

Two Cases of Scarring Alopecia or Folliculitis Decalvans.

By J. H. SEQUEIRA, M.D.

(Shown by Dr. O'DONOVAN.)

Case I.—Mrs. F., aged 51. Whooping-cough, chicken-pox, measles and scarlet fever when young. Married when 21; husband died of pneumonia twenty years ago; two children, killed in the war. This

patient always enjoyed good health until she had an attack of typhoid fever in 1900. About this time (she cannot be more precise) her hair began to fall out, at first above the left ear, and the bald area has steadily increased. She complains now of headache at odd times and of a good deal of itching of the scalp. In appearance the patient is a stout, quiet, brown-eyed woman, who looks her age and wears a wig. The *Calvarium* is divided into four hairless quarters by long wisps of hair which form the borders of large islands of polished white skin. The margins or borders of these bald areas show a narrow (1 cm.) zone of pigmented brownish very slightly scaling skin; in this are close set dilated or plugged follicles from some of which protrude short or longish hairs. The open follicles have a pink interior. Heart and lungs clear; no adenopathy; the teeth are artificial. There is a scar of an attempt at *felo-de-se* in the left orbital fossa. Wassermann reaction negative November 26, 1919. Widal reaction negative November 25, 1919. On November 20, 1919, a portion of the scalp, including a bald area and some reddening follicles, was removed under cocaine and adrenalin for *microscopic examination*, which showed a chronic dermatitis of the scalp involving and destroying the hairs and sebaceous glands.

Case II.—Mrs. E. P., aged 62, married; husband died of consumption twenty years ago; six children, one miscarriage, one son died of consumption. No infantile illnesses remembered. Tailoress before marriage, at age of 20. Climacteric at age of 50. This patient says she has been noticeably bald for two years, but her hair has been falling out for at least five years. When the hair first began to fall out she had been going through much worry, and a feeling of insects crawling in the skin of the scalp preceded the falling of the hair. She had no treatment before attending Dr. Sequeira's clinic on February 25, 1919 (No. 2178). On examination, this patient is a bright featured, brown-eyed, sparely built old woman. The head generally is bald and shining with a tonsure-like marginal fringe of hair, grey in front and black in the nape of the neck. There are a few straggling hairs on the apex of the occipital bone, and here and lower down on this bone are hairs set in swollen slightly raised reddened bases. The eyebrows are natural, there is no abnormal hair on the face. Wassermann reaction, November 25, 1919, negative. A piece of skin from the margin of a bald area was removed under local anæsthesia for microscopic examination on November 5, 1919, and by a happy chance a section was obtained going through a hair root and showing intense inflammatory infiltration around the hair root; it is shown at the meeting.

DISCUSSION.

Dr. GRAHAM LITTLE: Why should we relinquish the old title, "Folliculitis decalvans?" Until we know a definite ætiology it would be most convenient to have one name to describe the condition.

The CHAIRMAN: I always call it so.

Dr. S. E. DORE: I have watched cases in which, clinically, there has never been any folliculitis, that is to say, no visible circumpilary ring of inflammation in the commencing patches of alopecia and I think these cases come into the category of Brocq's "pseudopelade" as distinguished from Quinquaud's folliculitis decalvans.

Dr. G. PERNET: I think it is to some extent a question of degree. Some cases are very quiet in their evolution.

Case of Cornua Cutanea of the Feet.

By A. M. H. GRAY, M.D.

THE patient is a woman aged 65, who has suffered from "corns" on the feet for some twenty years. For the last ten years some of these have developed into horns, which fall or break off from time to time, and she has now quite a large collection of them. On two occasions she has had them removed by operation.

At the present time definite curved horns are present in the following situations: Arising from the inner and posterior aspect of the ball of each great toe and curving upwards towards the fold between the great toe and the foot; on the centre of the anterior part of the sole of the right foot curving inwards and backwards across the sole (this horn is broken off about $\frac{3}{4}$ in. from its base); on the ball of the right foot and on the outer part of the sole of the left foot, about 2 in. behind the little toe are two horn-like projections about $\frac{1}{4}$ in. high and surrounded by a deep fossa. Similar but smaller lesions are present on the dorsum of several of the toes and in the centre of the anterior part of the left sole.

There is no evidence that the patient has ever had gonorrhœa but the Wassermann reaction is positive.

It would appear without any question these horns have started either from callosities or corns. I have endeavoured to trace a similar occurrence from the literature and have failed to find a single instance of such.

Dr. RUPERT HALLAM: I have recently seen two similar cases, both soldiers who had suffered severe frostbite of the feet whilst serving in the trenches. In both these men there had been very considerable destruction of the skin and subcutaneous tissue over the plantar surface of both feet.

Case for Diagnosis.

By S. E. DORE, M.D.

THIS young woman, aged 29, has a circular inflammatory patch in the middle line of the front of her neck, of eight years' duration, consisting of a ring of raised papules resembling vesicles, which have apparently spread centrifugally leaving a peculiar cribriform scar in the centre. It appears to me to be a tuberculide of a follicular type, but I should be glad of any alternative suggestions as to the diagnosis.

DISCUSSION.

Dr. GRAHAM LITTLE: I ask whether that ring lesion was a patch, or has it been a ring from the start? It seems to me like a patch of tuberculous lupus which has healed in the centre spontaneously, as sometimes happens.

Dr. A. EDDOWES: I suggest that this is a very rare form of tuberculide, which I have called *lichenoid*. At the site of the nodules there is necrosis, and pits are left, very similar to the pitting after chicken-pox. I have seen a similar condition, in which the affection was chiefly in lines along the top of the shoulder. In this case there is a healed line of pitted scars leading up to the circle of spreading nodules. In my published case there were lines of what looked like lichen *planus;* there was a distinct family history of tuberculosis, and other tuberculides were present on the patient.

Dr. S. E. DORE (in reply): I do not think this has been a ring from the start. I have only seen her recently, and I have not been able to watch the progress of the condition. It is a ring of papules, not vesicles, and I think it has spread from a central patch. She has had no treatment, and the scar is the result of spontaneous healing.

Section of Dermatology.

President — Dr. Arthur Whitfield.

Case of Anæsthetic Leprosy.[1]

By E. G. Graham Little, M.D.

The patient is a Hindu medical man, aged 27, resident in Great Britain for the past five years, just qualified at Edinburgh University. He was seen by me at the end of 1915, when he was shown at this Section for a nodular and macular eruption of the leg and foot, which was of very doubtful nature, and was variously regarded as lichen planus and tuberculosis. At that date there was no anæsthesia, which only supervened in the latter part of 1918. Sections were made from one of the nodules in 1915, and stained for bacilli with a negative result. The condition then, and more or less now, was that on the right leg, just above the ankle-joint, there was a dark bluish staining of the skin, in mesh-like areas rather suggestive of lichen planus, and at the same time there was an eruption of a few nodules of the same tint on the leg and on the face. These have all been removed since he was shown, and sections from one of them have been examined for lepra bacilli with negative result. Scrapings from the discoloured skin and from the nasal discharge have been also examined for bacilli with so far a negative report.

The anæsthesia is very complete in the affected area on the right leg, so complete that he shows at the present time some burns which he says were caused by dropping a lighted match on the foot, yet evoking no sensation of heat or pain. The nervous condition has been kindly investigated by my colleague, Dr. Wilfred Harris, who thus reports on this factor :—

"*Sensation.*—Loss to tactile, to painful and thermal stimuli over lower half of right front of leg, extending on to foot as far as clefts of

[1] At a meeting of the Section, held February 19, 1920.

toes, outwards as far as back of malleolus and up side of fibula, sweeping across skin in mid-leg and across inner side of leg and down mid-Achilles tendon to just inside heel and along mid-sole, avoiding ball of great toe, but including pad of great toe. Thus the external popliteal cutaneous branches are involved, though not completely, and also the long saphenous. The distribution suggests a peripheral rather than a trunk nerve involvement. The right Achilles-jerk is almost absent, showing involvement of the internal popliteal or sciatic. The right abdominal skin reflex is very +, with some sensitiveness to deep pressure there. I could feel no enlargement of any nerve trunks, such as ulnar or peroneal. No muscular wasting or weakness."

Case of Circinate Erythema.

By E. G. Graham Little, M.D.

The patient is a well-to-do artisan, aged 33, well set up and otherwise in good health. He was sent to me by Dr. Pain, of Dalston, under whose care the man has been for the past six months. He has suffered almost continuously during this time from a very extensive eruption of circinate erythema, recalling Colcott Fox's cases published in the "International Atlas of Rare Skin Diseases." The patches are of very various size, some as large as the palm of one's hand, some the size of a sixpence. All have the same character of a vivid erythematous edge, raised for from $\frac{1}{8}$ to $\frac{1}{4}$ in. from the skin level, and enclosing a buff-coloured, but otherwise unaltered, area, the edge enlarging rapidly by centrifugal extension. There are no actual wheals but factitious urticaria is well marked.

The man has been a teetotaller all his life. His teeth have almost all been removed on the hypothesis that the eruption was due to septic teeth, but there has been no corresponding improvement in the skin. His motions are normal. There has never been any blister formation, and the lesions are, in fact, singularly homogeneous. The trunk, both back and front, the neck, the upper arms, are the principal sites affected, and here the lesions are too numerous to count. The mucosæ seem unaffected.

The patient has apparently improved enormously, in fact, with disconcerting completeness, since I saw him four days ago, when I put him on 20 gr. doses of calcium lactate three times a day.

Case for Diagnosis.

By Alfred Eddowes, M.D.

This patient first noticed a rash about his waist, causing some irritation, four days before landing in this country from a South American boat. The ship's doctor ordered a sulphur bath, but the eruption spread. After landing he consulted another medical man, who diagnosed scabies, and treated it for a fortnight. Patient then consulted me. I thought that scabies had existed, but as there were present several lesions so closely resembling specific roseola, together with enlarged glands, headache and throat catarrh, I sent him on for the opinion of an authority on that condition. This gentleman found a negative Wassermann, and, after careful examination and consideration, formed the opinion that the condition was entirely due to scabies. By this time the eruption had reached the face, and I was quite convinced that scabies did not account for all the lesions. Those on the face were first nodular and red. Those on the hands were wart-like, and in the centres of some of them could be seen firmly adherent, almost transparent, small shot-like bodies—apparently composed of very compact, horny cells. By this time many of the lesions had disappeared, but some left pigmentation and even scars. One lesion under the eye and another between the fingers have ulcerated this week. Under the microscope scrapings showed a number of brush-like epithelial plugs, a few rather thick mycelial elements and a few perfectly round and rather large "spores."

DISCUSSION.

The President: I should have thought this was definitely syphilitic. I would take a large sample of blood, and submit part of it to two or three Wassermann experts, and, I think, the Wassermann reaction will be found to be positive.

Dr. A. M. H. Gray: There are one or two features about the case which would certainly make one investigate it from the point of view of secondary syphilis, particularly the history and the glandular enlargement. But the eruption at the present moment does not seem to be quite typical of that condition. The lesions are most marked on the extremities, chiefly hands and forearms, and on the face. They are small papules, and are undergoing deep central necrosis, leaving distinctly pitted scars. In the necrotic forms of secondary syphilis the eruption is usually generalized and not localized as in this case, and the patient is much more ill. To my mind the skin lesions are more like a tuberculide than a syphilide.

Case of Morphœa of Acute Onset.

By E. G. Graham Little, M.D.

The patient is a girl, aged 12. She has been under the care of my colleague in charge of the Children's Department at St. Mary's Hospital, Dr. Reginald Miller, who sent her to me for a diagnosis of two patches which have made their appearance quite acutely in the last three weeks. These patches are somewhat oblong in shape, with the long axis of the lesion at right angles to the vertebral line in the erect posture, about 1½ in. by ½ in. in size, and situated one along the line of Poupart's ligament in the groin on the left side and one about ½ in. to the left of the mid-sacral line. The patches are still surrounded by a faint halo of erythema, which is perceptibly fading. The skin enclosed in this halo is typically glistening and sclerodermic. There is no alteration of the skin elsewhere.

DISCUSSION.

Dr. G. Pernet: I have recently had two girl patients with symmetrical morphœa of the back. I had gone some time without seeing a case until then, and now I see another here. I have been asking myself whether morphœa cases are more common in children lately than they used to be. I should like to know what is the impression of others on the point.

Sir James Galloway: Dr. Little has not told us the exact history, but last session we had several cases of sclerodermia shown, and the discussion went on the lines of defining, if possible, any segmental distribution of the eruption. In the present case it would require much imagination to be quite certain that those patches of sclerodermia involved or followed any definite segment of the body. Another point which arose in the discussion was, that several cases, including one which Dr. Pernet had brought forward, had a curious story of some previous peripheral injury in the region affected, the sclerodermia having later developed in such area. The general impression of the meeting was, that those injuries were merely accidental coincidences. It would, however, be interesting to know if Dr. Little suggests a segmental distribution, and whether there is said to have been an injury in this case.

Dr. F. Parkes Weber: I think this variety of morphœa—namely, in the form of circumscribed, more or less symmetrical superficial patches—occurs most frequently about the age of the present patient, and usually in females. I would like to know whether members present can give any idea of the

prognosis in these cases. Is this variety of morphœa known absolutely to disappear, either under thyroid treatment, or without it, or under any other treatment? Or does it ever become perfectly quiescent, never subsequently increasing to any appreciable extent? I regard the morphœa which is limited to a long stripe down one leg (reminding one of the distribution of herpes zoster) as a different clinical variety (it does not disappear, but tends after a time to become quiescent).

Dr. J. H. SEQUEIRA: I may be able to answer one or two of the points which have been raised by Dr. Parkes Weber. In reference to prognosis, I remember one case in which a patch did not increase in size, for, at any rate, twenty years, and in the same patient smaller patches disappeared, apparently spontaneously, without leaving any noticeable atrophy. I agree that, clinically, these cases are rather different from those in which long bands extend down a limb. The only case that I can bring as evidence on the question of trauma as a cause of sclerodermia raised by Sir James Galloway is one which involved a great part of the front of the leg, a band on the thigh, with sclerodermic patches round the lower part of the chest, which followed definitely on an injury. The patient was employed at the Bow railway works, and he caught his foot in a rail and fell across other rails, hitting thereby his chest and thigh. The question of compensation for injury arose, as the sclerodermia ultimately crippled the patient. He had chronic ulceration, which did not heal for many months.

The PRESIDENT: There was one case, not of this type, one of severe sclerodermia, which I remember, and it had a very striking history of injury. The patient suffered from caisson disease, from which he recovered, but he had a symmetrical sclerodermia, which developed a few months afterwards. I always regarded caisson disease as one in which sclerodermia was liable to be set up, if any trauma will cause it. I agree with Dr. Sequeira that the prognosis in this type of case is good: the lesions extend for a short time, and then decrease. I have one or two cases which came to hospital fifteen years ago, and later came for other troubles, and I found the sclerodermia had not noticeably extended. I do not remember having seen the lesions disappear, though they get softer.

Case for Diagnosis; ? Cutaneous Sarcoid of Boeck.

By A. M. H. GRAY, M.D.

THIS patient has been shown to the Section on two previous occasions, once, in December, 1914, by the late Dr. Dudley Corbett, and again, in January, 1919, by Dr. Stowers, and on neither occasion was a definite diagnosis arrived at.

He is a Belgian, aged 28, and has resided in this country since 1914. The present eruption first appeared in October, 1910, as a few non-irritable spots on the chest and abdomen. The rest of the body, however, rapidly became involved and in two or three days the rash had spread to the areas now affected. At that stage the spots were not as confluent as they are to-day. When the rash appeared first he was seriously ill with an acute affection of the throat, which he believes was diphtheria, and in which he nearly lost his life.

The rash nearly disappeared in the following summer but returned the following winter, and though he has at times got much better under treatment the eruption has never completely subsided and at present is worse than it has ever been before.

When he was shown here in 1914 he was suffering from gonorrhœa with epididymitis, but he also gives a history of having had an attack of phimosis with purulent discharge from under the prepuce at about the same time as the eruption first appeared, but whether the phimosis appeared before the rash or not, he cannot remember. In any case he denies having run any risk of venereal infection before that date, though he admits it subsequently to his arrival in this country.

There are no other points of note in his earlier history, except that he had measles in infancy. Nor is there any history of illness in the family that has any bearing on his case. His father and mother and nine brothers and sisters are all alive and well.

As regards the eruption it has occasionally been irritable but in the main has given rise to no symptoms, except that when ulceration has occurred it has been painful. As regards his general health he considers it excellent, but he has suffered from indigestion, especially flatulence, lately, and this he attributes to the medicines that he has been taking.

The eruption involves almost the whole cutaneous surface of the body including the scalp. It is less marked at the extremities of the limbs than elsewhere and the nails have completely escaped. The mucous membranes, including that of the prepuce and the glans penis, are also not involved. The lesions are, I think, all of one type. They commence as pinhead-sized papules, which are nearly always follicular, and which may attain the size of a sixpenny-piece. The early lesions are pale, almost skin-coloured, but as they increase in size they become a coppery-red colour, closely resembling a nodular syphilide, though they have a more waxy appearance. On the legs and feet they are a deep purple, even the tiniest lesion having this colour and some show definite hæmorrhages. The lesions are in many places discrete, especially on

the shoulders, hands and feet, and to a certain extent on the chest and abdomen. In other places, especially the scalp, face, and back, they tend to be confluent. Where confluent they tend to spread centrifugally, clearing up in the centre and leaving a pigmented atrophic area behind. In some places fresh lesions appear in a circle around the site of a previously-healed lesion. Where the lesions are large and have formed themselves into circinate patches, there is a tendency for ulceration of a superficial kind to occur; this ulceration is mainly traumatic, I believe, and not due to central necrosis. On the scalp, face, ears and limbs where the individual lesions are small, although grouped, this does not occur, while on the back it is marked. The individual lesions show little or no tendency to become vesicular or pustular but they mostly show some umbilication, owing presumably to their follicular character. In a few places the smaller lesions have an appearance very like lichen planus.

In addition to the skin lesions the patient shows involvement of the bursæ and tendon sheaths. The bursæ around both elbows, both knees and the tendon sheaths in front and behind the left wrist, on the left foot and behind the right outer malleolus, are converted into tumours varying from the size of an orange to that of a walnut, and of firm, elastic, non-fluctuating consistence. They are quite painless. On the subcutaneous surface of the right ulna several hard nodules can be felt firmly adherent to the bone. The lymphatic glands in the groin and axilla and also those in the left epitrochlear region are greatly enlarged and soft, like those met with in leukæmia. Those in the neck are large, while none can be felt in the right epitrochlear region. No physical signs have been made out in the chest, abdomen, or nervous system.

The blood-count shows: Red cells, 4,100,000 per cubic millimetre; hæmoglobin, 74 per cent.; colour index, 0·85; white cells, 9,800 per cubic millimetre, made up of lymphocytes, 15·2 per cent.; large mononuclear, 7·8 per cent.; polymorphonuclear 73·6 per cent.; eosinophils, 2·6 per cent.; mass cells, 0·8 per cent.

A Wassermann test done on February 6 was negative and a second done thirty-six hours after an injection of novarsenobillon (0·45 grm.) was also negative. X-ray photographs of the wrists and right forearm show no bony nor joint changes.

A portion of skin was removed from the left shoulder, hardened in formalin and sections made. The nodule is seen to consist of a lobulated mass of granulomatous tissue which is spreading along the subpapillary

vascular plexus and also downwards along the vessels to the deep plexus, surrounding the hair follicles and sweat glands but not involving them. The vessels themselves do not appear to be involved, as there is no endothelial proliferation and they remain patent even when surrounded by a mass of inflammatory tissue. The main mass is well supplied with capillaries. This main mass consists chiefly of epithelioid cells and lymphocytes, the latter of which are most numerous at the periphery. Many of the epithelioid cells are undergoing degeneration and leaving spaces which are surrounded by processes from other epithelioid cells, giving the whole mass a sponge-like appearance. Plasma cells are rarely met with. In the deeper parts of the section some very well-defined giant cells of the Langerhans type are found, and others less well defined are seen in many places.

Sections stained by Ziehl-Neelsen failed to show any acid-fast bacilli, and no Gram-positive organisms were seen.

There are still some other investigations to be made. I propose to get a guinea-pig inoculated with one of the lesions, and also to do some tuberculin tests.

Certain suggestions for diagnosis have been made, of which, I think, the four most important are: syphilis, lichen planus, xanthoma, and a granuloma of unknown origin. As regards syphilis, this was the first diagnosis made at St. Thomas's Hospital in 1914 but was discarded on further observation. It was the first diagnosis I made, and I think there are some clinical points in its favour, especially the involvement of the bones, bursæ, and tendon sheaths. The repeated absence of a positive Wassermann reaction and the histological character of the lesions is however against it. I do not think it is lichen planus: he has occasionally complained of a little itching when he gets hot in the summer, but there is no persistent itching, and the histology is not similar to that of lichen planus. If it had been xanthoma, I should have thought that, by now, we should have had some signs of typical xanthoma lesions. I have not stained sections with Sudan [1] or osmic acid, and therefore cannot be certain about that. Personally, I should be inclined to group it with the cutaneous (or Boeck's) sarcoids, both on clinical and histological grounds, and that is rather supported by the fact that it has definitely improved under novarsenobillon injections, though not with the rapidity one would expect if it had been syphilitic. I hope it will be possible to report further on the case at a later date.

[1] Sections from a nodule hardened in formalin and cut in gum have since been stained with Sudan III and no fat globules found in the epithelioid cells or neighbouring structures.

DISCUSSION.

The PRESIDENT: This is a case of extraordinary interest. I saw it a good while ago, with the late Dr. Corbett, before it was exhibited here. The eruption has considerably changed in the interval. At the earlier date there were a considerable number of lesions which were transparent in the centre, and on pricking the lesions fairly deeply colloid material was exuded; and I do not remember that the annular grouping was so marked when I first saw it. There is a history of more than one relapse of the disease, and, that being so, the grouping might be a later phase of it. I think this is a hitherto undescribed disease, and I suggest that Dr. Gray finds a suitable name for it, so as to separate it off. I confess the sarcoid group leaves me rather cold: I do not see any great value in it.

Dr. F. PARKES WEBER: I think there is a rare clinical group of cases in which very severe rheumatoid (arthritic, bursal, or teno-synovial) symptoms are combined with a relapsing skin eruption. The lesions around the joints have sometimes been roughly described as rheumatoid arthritis. The skin condition has sometimes been described as psoriasis, and doubtless sometimes is true psoriasis. I know very little of the ultimate result in these cases. I suggest Dr. Gray's case belongs to this group.[1]

The PRESIDENT: Dr. Weber is not, I suppose, contending that every case of so-called psoriasis associated with rheumatoid arthritis is really not psoriasis? [Dr. WEBER: No.] Because I think the dermatological world is quite aware that a certain form of joint trouble, which we call osteo-arthritis or rheumatoid arthritis, is one of the few constitutional accompaniments of true psoriasis. I do not think the present case has anything to do with cases which look like psoriasis.

Dr. F. PARKES WEBER: I feel convinced that the skin-eruption in the severe class of cases to which I have referred is not always true psoriasis. I admit that genuine psoriasis may be associated with trouble in the joints, bursæ, and tendon-sheaths.

Dr. G. PERNET: One point in favour of xanthoma in this case is the presence of the growths about some of the joints. In typical xanthoma cases one sometimes finds masses of this kind, particularly about the tendo Achillis. The term "sarcoid" is vague. As to the kind of case mentioned by Dr. Parkes Weber, I agree with the President that it is psoriasis associated with rheumatoid arthritis. As to acne agminata, I examined histologically Dr. Crocker's case of that, and found it was really a sweat-gland lesion.[2] I found no tubercle bacilli. I believe Schamberg has confirmed that.

[1] One might also refer to cases in which a severe type of xanthoma has been associated with chronic changes about the joints or in the tendon-sheaths. Cf. J. M. H. MacLeod, *Brit. Journ. Derm.*, Lond., 1913, xxv, p. 344; R. Cranston Low, *ibid.*, 1910, xxii, p. 109.

[2] H. Radcliffe-Crocker's "Diseases of the Skin," 3rd ed., 1903, fig. 70, pp. 1096-97; also histological description, Pernet, *Brit. Journ. Derm.*, 1902, xiv, p. 131.

Dr. GRAY (in reply): I asked Dr. Castellani to see this case, and had hoped he would have said something as he had seen a somewhat similar case. He was inclined to agree with the provisional diagnosis I have made. I think Dr. Pernet is a little hard on the term "sarcoid." Sarcoids have been worked out by some distinguished dermatologists such as Darier, and they are now classified into fairly definite groups. The urine of this patient is quite normal.

Case of Generalized Rodent Ulcer.

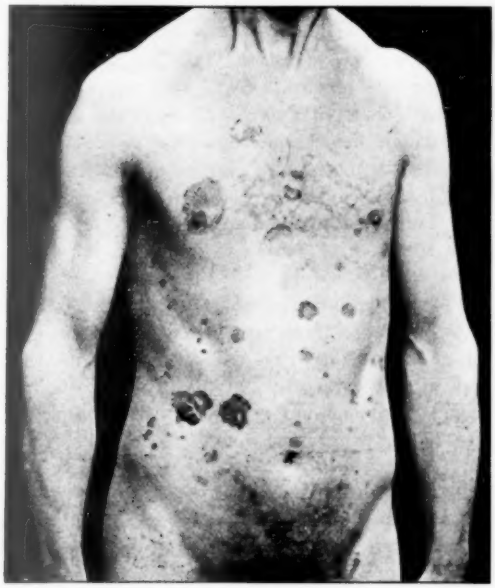
By A. M. H. GRAY, M.D.

THE patient is a male, aged 51, a builder by trade. He has had psoriasis on and off since 15 years of age. Twelve years ago a nodule appeared on the right cheek, which was subsequently diagnosed as rodent ulcer and three years ago was treated by radium. The site of this lesion is now represented by a depressed atrophic scar. Some two years after the appearance of the nodule on the cheek, another appeared on the front of the chest, and since then some ten other lesions have appeared on other parts of the trunk. He says that most of these have been very much like the psoriasis patches in appearance and that he was at first only able to distinguish them by the pain produced in them by the ointment he was using for the psoriasis and by the fact that his clothes hurt them, which necessitated his wearing very soft underclothing. Apart from this he has had excellent health except for an attack of "arthritis" in his right hip five years ago.

The family history is interesting. His father had psoriasis and he believes there have been other cases of psoriasis in the family on his father's side. His eldest brother had a rodent ulcer of the face; his maternal grandfather had two nodules, on the lip and on the face, said to be cancer, and a maternal uncle had a "growth" removed from the face and afterwards lived to a great age. His wife died of cancer.

At the present time he has small psoriasis patches scattered over the trunk and limbs. These lesions are not very numerous, they are of the discoid type with slight scaling, of a pale pink colour and rarely exceed a sixpenny-piece in size. When first seen by me, about a month ago, he had eleven rodent lesions, all on the trunk, including one in the left groin; the scar on the cheek representing the site of a twelfth lesion. All except one are situated on the anterior surface of the chest or abdomen, the one exception being in the right loin posteriorly.

All these lesions, with two exceptions, show the same characteristics. They have an irregular outline, and are of deep brownish-red colour: they seem to be slightly depressed below the surrounding skin and in some places show areas of atrophic scarring: otherwise they have a finely nodular surface covered by small thin crusts. One lesion however, the earliest to appear, situated over the inner end of the third right rib, has a raised pearly-white margin on its outer side, sharply rounding an area of atrophy, and on this border dilated vessels can be seen: in fact



it has all the appearances which we are accustomed to associate with the superficial type of rodent ulcer. The inner part of this lesion has the same characters as those described above. The other lesion, which differs from the majority, is one situated on the right side of the abdomen ; from its centre protrudes a polypoid mass, as is well seen in the photograph. I have excised this patch and the one immediately adjacent to it, and in both cases microscopic examination shows a basal-cell epithelioma.

The case is of special interest as this is the third case of this condition that has been shown at three consecutive meetings of the

Section, the previous cases being shown by Dr. Graham Little and Mrs. Savill, and these three cases are apparently the only three which have been published.

A further point of interest was to discover whether the lesions started in a patch of psoriasis, as I had previously published a case in which this undoubtedly occurred,[1] but the patient could give me no definite information on this point.

DISCUSSION.

Dr. GRAHAM LITTLE: At Dr. Gray's invitation, I saw this patient before he came here, and I can emphasize the extreme resemblance of the case to the one I showed here two meetings ago. This makes the fourth case of the kind I have seen. In Mrs. Savill's case, shown at the last meeting, there was a very chronic seborrhœic dermatitis occurring through several years of the patient's history, and it is interesting that we should have psoriasis now, in a similar ultimate condition. I have a reference to rodent ulcer developing over seborrhœic patches, but I have not had experience of that. The extensive distribution on the trunk in this case is suggestive of such an association. I do not think any similarity can be traced between this type and the equally disseminated type of epithelioma adenoides cysticum of Brooke. In none of the four cases I have seen has there been the pearly lesion so characteristic of the latter condition. In one case the patient is said to have had the earliest lesion at the age of 11—an extraordinarily early age. We have all seen cases of a single flat lesion of rodent, but this very disseminated type, especially on the trunk, constitutes a new group of rodent.

Dr. A. EDDOWES: I am old enough to remember terrible cases of rodent ulcer, and one of the striking features about them was, that whereas on the upper part of the face, and even the lower part, they burrowed deeply, sometimes causing the teeth to fall out, and the antrum to be opened up, yet when they attacked the neck they only destroyed skin. I have seen them travel superficially to the clavicle. If you hid the face and looked at the neck only, the lesions would be seen to be very like the lesions in this case and that of Dr. Agnes Savill's which has been referred to. Therefore I do not think it follows that they are of different causation.

[1] *Brit. Journ. Derm.*, 1912, xxiv, p. 325.

Extensive Vascular Nævus.

By A. M. H. Gray, M.D.

The patient is a male, aged 20. He has a vascular nævus which is almost universal (the only area which has escaped being the right side of the front of the chest); the nævus has been present unaltered since birth. It is an ordinary "port wine" stain, but has a curious reticulation. The mucous membrane of the whole mouth is involved, the lesion extending back on to his palate and pharynx. Six years ago he developed a septic spot on the middle toe of the right foot, and it took three years to heal up. While that was still unhealed, he began to get ulcers on other parts of his body, on the elbows, knees, and about the ankles. He now has an ulcer on the left heel, which is probably a bed-sore. He has difficulty in walking, which is due to his having been in bed a long time. No nerve lesion has been found by neurologists. He has nine brothers and sisters but nobody else in the family is similarly affected. I have not been able to obtain a history of a maternal impression.

Leucodermia.

By W. Knowsley Sibley, M.D.

This young woman, aged 25, whose hair is black, shows very extensive areas of leucodermia. Six years ago she noticed white patches on the sides of her neck, and, almost simultaneously, some half a dozen bald patches appeared on her head. The hair which has since grown on these areas is white. She has leucodermia over the greater part of her body; at present it extends down as far as the middle of her thighs. She has an extensive patch in both axillæ, but the hair in this region has neither come out nor changed colour.

It is unusual to see cases of leucodermia associated with this condition on the scalp. The point is, do these cases in which you get areas of white hair growing always start as alopecia areata? One sometimes sees, apart from obvious leucodermia, people with patches of white hair on their heads. These are termed cases of leucodermia, but I think if these scalps are examined one seldom finds any evidence of this disease. They are cases of canities areata.

I consider there are three groups of canities areata, one starting as alopecia areata, in which the new hair grows of a lighter colour, and in most cases eventually becomes of the same colour as the other hair. Occasionally it may remain lighter for a long time, or even permanently. The second group comprises those cases in which the fair tufts of hair appear on the scalp without preceding alopecia areata. These cases are more permanent, and it is rare that the hair changes colour again; usually the white tufts in the scalp remain permanently, and eventually the hair of the whole head becomes white. There is also a third rather unusual group, hereditary in origin, and extending to several generations. One sees a tuft of white hair, or hair of a lighter hue than that on the rest of the scalp, but in all of these I consider it is wrong to apply the term leucodermia, as I do not think there is any leucodermia about them, any more than it would be correct to speak of people whose hair has turned white with advancing years, as suffering from this disease.

DISCUSSION.

Dr. A. EDDOWES: Some years ago I called attention to leucodermic alopecia, and stated that it was as symmetrical as leucodermia on the body. I showed a remarkable case at the British Medical Association meeting at Cheltenham, in which a man had lost all his hair through alopecia areata. He also had leucodermia, and melanodermia surrounding it in typical form. He had, moreover, patches of what might be called morphœa, and larger patches which might be termed sclerodermia: so he presented a museum of these conditions. When leucodermia of the scalp does not appear to be symmetrical it will be found that it really is so, if we make allowance for the fact that the scalp does not sit symmetrically upon the skull.

Dr. F. PARKES WEBER: I do not see why this case should not be regarded at present as an ordinary one of vitiligo with irregularly symmetrical areas of distribution on the scalp and on the body. In the past history of the case, however, the patches of alopecia are remarkable.

The PRESIDENT: Alopecia areata associated with leucodermia has been described many times; it is not uncommon. The coincidence is used as evidence by those who hold trophoneurotic theories of alopecia. I disagree with Dr. Sibley as to its being rare in leucodermia when it is on the back of the neck: you always find some. There may be a leucodermia extending to the nape of the neck and bleaching some of the hairs, not all. I do not think there is any reason to recast our methods of observation and say that these patches of canities are not leucodermic when associated with leucodermia. I confess that to diagnose whether leucodermia of the actual skin of the scalp

is or is not present, except in very dark people, would be very difficult, because the scalp is a very non-pigmented piece of skin in most people.

Dr. SIBLEY (in reply): What I referred to was canities areata, cases of which are not at all uncommon, in which one cannot determine that the scalp is in any way affected with leucodermia; there is no evidence of leucodermia on any other part of the body, and these lesions are as a rule asymmetrical. In the leucodermic cases the lesions are at any rate irregularly symmetrical.

Two Cases of Tar Carcinoma.

By J. H. SEQUEIRA, M.D.

I SUPPOSE most men who are working in general hospitals have received an order that in future they are to notify affections caused by tar. I, therefore, thought it would be useful to members if I brought up a couple of cases.

Both men have been employed in gas works. The first man came to me in March last year with a growth on the forehead. That was excised and sections made, and it was found to be squamous-celled carcinoma. He has had a number of tumours which have dropped off, leaving scars. Early this year he came again, with a lump the size of a nut on the back of his left forearm, where there are scars of previous lesions. I am exhibiting a photograph of the second tumour which developed in four weeks. It has been excised. I show an excellent section of this lesion from the back of the wrist: a trabecular, squamous, prickle- and horny-celled carcinoma.

The second patient came to the London Hospital having rapidly developed a tumour on his face. It is his duty to throw pitch into barges. He has several scars on his face and on his hands. The lesions have dropped off leaving atrophic scars. The tumour on the left cheek is of three weeks' duration. It is rounded, raised and slightly crusted. Its transverse diameter is 15 mm. and it is raised about 7 mm. above the surface. It is freely movable over the deep tissues. There is a small gland palpable in front of the left ear, but I hope this is not malignant.

The late Mr. Harold Barnard investigated a number of these cases, which show the following stages of development: The workers often get splashed with hot tar, and dermatitis follows; at a later date they get tar molluscum or tar tumour. But the tar worker does not worry

about these lesions very much, because they usually drop off. Now and again, however, especially in elderly men, they do not fall off but develop into squamous-celled carcinoma.

DISCUSSION.

The PRESIDENT: Many years ago I had to write an article on this subject, and I had the inestimable advantage of going round with Mr. Barnard over a very extensive coal-tar works. He had a very thorough knowledge of the subject. As a rule, what causes the trouble is the splash of the hot tar compound. Creosote oil in the gas works is an impure form of creosote, and the men are carefully kept away from the stills when they are full. In that way this coal-tar works stamped out tar dermatitis. The earliest lesion one can see is a spot of erythema, looking not unlike lupus erythematosus, with a distinct tendency to telangiectasis. That interested me, because it is exactly what one sees on the backs of the hands of people who have been in the Tropics, described by Sir Lenthal Cheatle as "biotripsis." On this erythematous patch one of two lesions may develop. If the erythema is on the glabrous skin, there is first a common wart, but if it occurs where there are horny follicles, there is a horny folliculitis, and the lesions aggregate and produce the tar molluscum. That may fall out, leaving a cribriform hole beneath. Then there is the stage which Dr. Sequeira shows to-day, the production of a carcinomatous lesion. Mr. Barnard assured me that not only does the molluscum involute occasionally, but that lesions like that on this man's face may occasionally necrose extensively round the infiltration, and drop out. On comparing these with common squamous epitheliomata which are not due to tar, we were both struck by the very stiff belt of plasma cells and lymphoid cells which underlie the epithelial infiltration. We both thought the reaction was enormously strong, and that it choked the vessels, and so led to necrosis. It is only occasionally that these necrose and fall out. At any rate, the man is much better off if you excise and sew up the wound.

Dr. J. H. SEQUEIRA (in reply): We had a case from Lincolnshire in which a comparatively new tar industry (spraying roads with tar when laying tar macadam) produced a lesion of this kind.

Postscript.—The younger of the two men shown at the meeting had a wart below his left eye, and the general opinion was that it ought to be excised. In a week's time the patient wrote to say that the tumour had dropped off.

Two Cases of Trade Eczema.

By W. J. O'Donovan, M.D.

Case I.—G. T., male, aged 21. Previous health good. Has worked at a chemical factory in East London for two months. All sorts of colours are made here and he attributes his disability to "red oil." His fingers have been stained for six weeks, and he has had an irritating rash of the forearms and face for four days only. On examination: The patient is a sturdy well built labouring man. His hands are stained a red ochre colour up to the wrists (the linings of his coat sleeves are similarly red), the palms of the hands are smooth, glistening, and they sweat profusely. The patient's forearms are swollen and red up to 2 in. above the elbows and the upper arm shows many discrete red papules. The axillary glands are slightly enlarged. On the face and neck are numerous discrete and confluent red papular areas. The patient says that other workers are similarly affected, some of whom wear rubber gloves as a precautionary measure. (This patient was admitted to hospital—the arms were swabbed with acetone, as a dye solvent, and in four days he was well.)

Case II.—F. S., male, aged 36, pastes imitation and real leather on to fancy articles and occasionally touches them up with Bismarck brown. This is his second attack in the past twelve months. The pads of the patient's fingers are stained brown, the backs of the fingers are dry and fissured. The palms show occupation callosities and sweat freely. The flexor aspect of both forearms is pink, the extensor aspect shows discrete red papules. On the face is a blotchy irritating erythematous rash. The patient's eyes are unaffected; there is no adenitis.

DISCUSSION.

The President: Before the setting up of acute eczema, from almost any irritant on the palms, hyperidrosis is the rule. I know it is the case with spirit of biniodide. These cases with generalized rash may perhaps be due to absorption, but the irritant may soak deeply into the epidermis, and may, I think, get washed out on Saturday night and Sunday by the hyperidrosis. I think the dyes are best removed by grease. Perhaps these cases are more prevalent now because we are using English dyes, which are much less refined than those we had before the war.

Dr. A. EDDOWES: I should have thought that if these dyes were excreted, the resulting dermatitis would be pretty general in distribution and especially marked in the armpits and groins. The absorption I should say has occurred on the exposed parts which are now inflamed.

Dr. F. PARKES WEBER: What is the effect of Turkish baths in such erythematous cases when the nature of the irritant is known? Do they increase the dermatitis and erythema and make it more generalized, or is the sweating followed by relief (with or without preliminary temporary exacerbation)?

The PRESIDENT: T.N.T. poisoning, according to Dr. Benjamin Moore, was due to skin absorption, and the absorbed T.N.T. gave rise to liver trouble. We devised a means of preventing the T.N.T. getting at the sweat. I should have thought the action of a Turkish bath was not the same as that of bathing with water, because the Turkish bath brings out the grease from the skin.

Section of Dermatology.

President—Dr. Arthur Whitfield.

Lichen Planus.[1]

By S. E. Dore, M.D.

This woman, aged 43, was sent to me a fortnight ago with thickened pigmented patches on the legs and left thigh, accompanied by severe itching. The disease began on the right shin twenty-two years ago, and has gradually spread, so that there is now a large continuous patch occupying the upper two-thirds of the leg on the anterior and outer surfaces and extending posteriorly to the popliteal space which has become affected during the past five years. On the left leg there is a similar but smaller patch which began ten years ago, and there is also a small patch on the outer surface of the left thigh of four or five years' duration. The affected areas are of a dark brown or sepia colour, and consist of closely aggregated flattened and acuminate papules with much thickening of the skin—the coalescence of the papules producing a somewhat warty or mammillated appearance resembling at first sight a linear nævus. At the outer margin of the affected areas, especially on the right leg and also in the popliteal space, the papules are more discrete and arranged in parallel rows, but this peculiar arrangement is lost in the patch on the thigh, where they resemble more closely ordinary lichen planus. In parts there are irregular pale areas, apparently consisting of scar tissue due to scratching.

I had the advantage of Dr. Graham Little's opinion on the case, and he regarded it as a characteristic acuminate lichen planus. The President also pointed out that the lesions were not follicular.

[1] At a meeting of the Section, held March 18, 1920.

DISCUSSION.

Dr. MACCORMAC: Some of the features presented by this case suggest the possible diagnosis of an atypical form of Darier's disease, although the eruption is not on the usual situations. I suggest that a section be made as being likely to provide more precise information than can be obtained from inspection.

Dr. GRAHAM LITTLE: I support Dr. Dore's diagnosis. I think it is acuminate lichen planus, and I suggested to him it should be shown here because I think cases showing acuminate lesions without any plane papules are very rare.

Later Note by Dr. Little.—Since seeing this case I have received from Dr. Wallhouse the report of a case he described at the Atlantic City Meeting of the American Dermatological Association which I attended, and the excellent photographs which illustrated the paper portray a condition very like the case under discussion. Wallhouse records his case under the title "Parapsoriasis lichenoides linearis," but expressly mentions that his case was, in fact, diagnosed as lichen planus by a number of competent dermatologists.

Case of Chronic Diphtheritic Granuloma.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a South African, a major in the Indian Army, who was sent to France at the outbreak of war, and served abroad until two and a half years ago, when he was invalided home with the present disease. He was at the Third London General Hospital where he was seen by Dr. Pringle and Dr. Adamson, and finally by Mr. Ernest Lane, who sent him to me a few weeks ago. At the present moment he shows large medallion-like raised plaques, with warty centre and ulcerative edges on the side of the right foot and the dorsum of the left toes, and a later and similar plaque has developed on the left shoulder. There is, further, a history of an exactly similar plaque on the left wrist, which has completely disappeared without scar or pigmentation. He has in addition a peculiar granular warty ulceration in the orifice of the left nostril and a similar condition affecting the whole of the back of the hard palate and the fauces.

My colleague, Dr. John Matthews, has been good enough to make an exhaustive search for organisms in the patches, and has demonstrated the presence of a bacillus which he cannot at present decisively declare to be the Klebs-Loeffler bacillus, but from its staining reactions he considers it to be probably that organism. Further investigations will be made in the case and a subsequent report offered. A complicating feature in the case is that the patient has had several attacks of gonorrhœa, with arthritis, and the possibility has certainly occurred to me that the plaques are examples of gonorrhœal hyperkeratosis (although I admit that I have never seen the mucosæ affected) and that the diphtheritic or diphtheroid infection is a secondary contamination. My colleague, Captain Douglas, tells me that this was a very frequent experience with wounds in France. Some clinical confirmation of the diphtheritic infection is afforded by the patient's statement that in two instances cases of true diphtheria developed in country houses in which he was a guest some time after his arrival there.

DISCUSSION.

Dr. MATTHEWS: I have but little to add from the bacteriological side. It is difficult to suggest that the diphtheria bacillus is alone the cause of the condition. I imagine the diagnosis might be helped by seeing what result followed giving some diphtheria antitoxin: if the condition cleared up under that there would be no reason to call the case gonorrhœal hyperkeratosis. The bacilli stain typically with Neisser stain, but do not show the usual involution forms. They are short bacilli, not like the usual strains of the Klebs-Loeffler. The guinea-pig experiments will not throw much light on the diagnosis, only on the degree of virulence. Clinically, I have no opinion to offer.

Dr. J. J. PRINGLE: I first saw this officer at the military hospital to which I was attached in the beginning of October, 1917. He had been admitted in the previous May suffering from dysentery with ulceration of the tongue. He was referred to me on account of an extensive granulomatous lesion on the back of the left wrist, attributed, but I thought erroneously, to radiant heat applied for the relief of gonorrhœal arthritis of the joint. The growth was similar in appearance to some of the lesions which he now manifests on the foot, and probably, I think, identical with them in nature. It was carefully examined, after biopsy, for tuberculosis, syphilis, blastomycosis and streptothrix but with negative results and only some banal streptococci were discovered. At that time we were not on the look out for diphtheria organisms in lesions of the sort, so no special research was made in that direction. His Wassermann reaction was negative. The condition for which I saw him slowly subsided, leaving perfectly normal skin, but while in the same hospital I again saw him for epidermophyton invasion of the inguino-crural regions and interdigits of

the feet, which yielded rapidly to routine treatment. He again came under my observation in February, 1919, on account of (1) a relapsing, vesicular and bullous eruption between the toes of only seven months' duration and not due to epidermophyton; (2) a hard, granulomatous-looking mass in the left nostril with an appearance like hypertrophic rhinitis in both nostrils; and (3) a curious granular, rather warty-looking eruption over the fauces and palate, which I notice has now spread extensively to other parts of the mouth. From this the spirillum of Vincent's angina was stated to have been isolated, and he had been treated with ten injections of a stock polyvalent vaccine without obvious benefit. Dr. Adamson, who saw the patient with me, agreed that despite the permanently negative Wassermann reaction and the absence of syphilitic manifestations a short course of arseno-benzol might be of some service even for diagnostic purposes; but after only two injections the patient contracted influenza with some pneumonic complication, and nearly died. I must admit that none of the lesions on the feet or back suggest any gonorrhœal affection of the skin with which I am acquainted; nor am I aware that gonorrhœa can produce the condition of mucous membrane present in the nose and buccal cavity, the appearances of which are utterly unfamiliar to me.

Dr. BARBER: This case resembles in some ways, the one I exhibited to the Section two months ago, but my patient never had any lesions of the mucous membranes. Moreover, the ulceration was much deeper than it is here, and the whole skin sloughed, leaving behind raw granulations. Still, the manner in which the lesion spread in this case much resembles that in mine. I should like to know if testing the agglutinating power of the patient's blood against the organism Dr. Matthews has isolated, would afford a positive result as evidence in favour of the organism being the cause of the ulceration. My patient's serum agglutinated the diphtheroid bacillus, which we isolated, in a dilution of 1 to 500, and the serum of the guinea-pigs in which we produced the disease, and from which we recovered the organism in pure culture, also developed very strong agglutinative properties. With regard to the use of antidiphtheritic serum, after I had shown my case we gave the patient during a period of five days three injections of 3,000 units each. These injections produced a large local serum reaction, but did not seem to have any result on the disease. We tried at the same time injecting two guinea-pigs, one with the culture of the organism the other with the same quantity of culture *plus* a certain quantity of antidiphtheritic serum. The guinea-pig which received the serum was very much smaller than the other, and one might have expected that it would develop a greater reaction than in the larger; but there was practically no reaction at all, and it formed no pus. But the larger one, as in the case of others we had inoculated, developed an abscess, which broke down, and formed a lesion not unlike those exhibited by the patient. I think the diagnosis suggested by Dr. Graham Little and Dr. Matthews is probably right, particularly because there are mucous membrane lesions. But from the experience of my case, I doubt whether they are dealing with the true diphtheria bacillus. It is more likely to be a diphtheroid.

Case of Delhi Boil or Sore (Syn.: Oriental Sore; Aleppo Boil).

By J. H. Stowers, M.D.

I am much indebted to this lady, the daughter of a medical man, for kindly attending this afternoon and giving me the opportunity of demonstrating her case to you as it is a proven example of a disease rarely seen in this country. It is an instance of the tubercular type of the specific granulation known as the Delhi boil, or sore, and due to the presence of the protozoon *Leishmania tropica*.

The patient was born in Scotland, is aged 34, married, has one child (aged $6\frac{1}{2}$), and enjoys good general health. For the last nine years she has resided with her husband at Lahore where he holds a civil appointment. The patient has not known or been in contact with any person suffering a similar ailment nor has she the recollection of being attacked by any fly, mosquito or parasite. I am informed that at the beginning of December, 1919, she first noticed a minute lesion under the right lower eyelid which was red and slightly inflamed, soon becoming nodular. There was no itching. A second and smaller nodule situated on the lower and outer side of the former, developed about the beginning of January, 1920, and later, about the end of that month, a still smaller lesion was discovered on the extensor surface of the left forearm at the junction of the middle and lower thirds, all exposed parts of the body. The nodules on the face, when I first saw them, were circular, red in colour and considerably infiltrated, the larger having a diameter of $\frac{1}{2}$ in., the diameter of the smaller being $\frac{3}{8}$ in., with but a limited line of separation between them. In the larger nodule a minute central punctum or depression was visible. The third lesion on the forearm was less developed and nodular, suggesting rather a trauma than a specific granulation, but a collection of superficial and somewhat loosened epithelial scales were visible.

As the history of the case suggested a possible specific infection, the conditions hardly corresponding with those seen in ordinary septic sores, and no conclusive clinical evidence of tuberculosis cutis being present, I asked Dr. Castellani to see the case with a view of making a detailed microscopic investigation which he kindly undertook with the result that he has been able to demonstrate the presence of the

protozoon (*Leishmania tropica*) the Leishman bodies being unusually developed and numerous. I may mention that the scrapings were taken from the smallest lesion on the forearm. I have to thank Dr. Castellani for his valuable help in this direction and for the exceptionally beautiful microscopic specimens which he has brought down for your inspection. Dr. Castellani has kindly undertaken to describe them to you.

I am now commencing to treat the case with injections of oxide of antimony.

Dr. ALDO CASTELLANI: Dr. Stowers has given you, very clearly, the clinical features of the case, and I may, perhaps, be allowed to say a few words on the microscopical investigations which he carried out. From the lesion on



the left arm a scraping was made, and several films were prepared. They were fixed with absolute alcohol and ether, in equal proportions, and then stained by the Giemsa's method for fifteen to twenty minutes. The microscopical examination of these films revealed an enormous number of Leishmania bodies. These bodies are oval or roundish, and their length is 3 to 5 microns, their breadth 2 to 3 microns. They often contain, as you will see in the specimen under the microscope, two masses of chromatin, a large one, the trophonucleus, and a small one, the micro-nucleus or kineto-nucleus. From the kineto-nucleus there takes origin at times a very delicate process which terminates in the acute end of the body of the parasite. You all know there are several species of this parasite, the principal ones being *Leishmania*



STOWERS:

Case of Delhi Boil or Sore (Syn.: Oriental Sore; Aleppo Boil).

Donovani, which causes Indian kala-azar; *Leishmania infantum* which causes infantile kala-azar; *Leishmania tropicalis*, causing Oriental sore, and *Leishmania Americana*, causing the so-called espundia of South America. Under the microscope you will see a film from our case, which contains a number of typical Leishmania-like bodies. I would like to call your attention to a certain feature of the specimen, and that is the really enormous number of parasites. This is due to the fact that the scraping was made from a lesion which was in the very first stage of development, when there was no sign whatever of ulceration. Had the scraping been made at a later stage of the lesion, very few of these parasites would have been seen in any one field.

Xanthoma Diabeticorum.

By W. Knowsley Sibley, M.D.

L. F., a single woman, aged 32, a dressmaker by occupation; her father and mother are both living and well. She is the sixth child in a family of ten. With the exception of three brothers who were killed in the war, all her brothers and sisters are living and well. The patient has never had jaundice, nor any illness she can remember, and attributes her present condition as the result of repeated shocks in the air raids. She is very pale, thin, and obviously ill, and has a good deal of cough. She states she has lost 2 st. in weight during the last twelve months. About a year ago she lost her health and began to feel weak and very tired. In October last a papular rash came out on her palms, elbows and chin, the palms being very uncomfortable and painful on pressure. Later on a few similar spots appeared on the front of the knees. She has been rather thirsty for the last six months, and has coughed a good deal, with some expectoration.

On examination there are some râles at the left apex and general rhonchi over the lungs. Tubercle bacilli have not been found in the sputum.

On admission to hospital a fortnight ago, the urine specific gravity was 1045; no albumin, but a considerable amount of sugar was present, and she was passing some 260 oz. a day. The specific gravity is now 1034, and the quantity reduced to 130 oz. An extensive discrete papular eruption was present over both elbows extending down half of the dorsal surface of the forearm. The lesions consisted of firm papules, somewhat yellowish in tint, and some were of a semi-translucent appearance. A few isolated similar papules were present over the

patellæ, and some half a dozen on the chin. None were present on the eyelids. About the palms, especially of the right hand, yellowish lesions abounded under the skin and had much the appearance of a severe dysidrosis. A few ill-defined papules were present under the base of some of the toes.

After a fortnight in hospital with a modified diet, and doses of alkalies taken internally, the lesions rapidly improved and with the general improvement in her health are apparently entirely disappearing.

Under the microscope there is a section of one of the papules, taken from just above the elbow, showing ordinary xanthoma.

The special feature is that it is far more common in men than in women. In the first thirty-six cases reported only five were in women. There is also the interesting fact that, although she is one of a large family, none of them have had a similar condition, and this patient had never previously had an illness. I think she correctly attributes the onset of her diabetes to shock produced by the air raids. She came to hospital not knowing she had sugar trouble: in fact she had been told by her doctor that there was nothing wrong with her. The rash is disappearing in an extraordinary rapid way on the ordinary treatment for diabetes—the giving of alkalies internally and modifying the diet.

Case of Xanthoma.

By J. L. Bunch, M.D.

This case is in fairly sharp contrast with that just shown by Dr. Sibley. Here the lesions are limited to the eyelid; they are of the soft xanthoma type. They have persisted for a good many years, in fact the patient does not know how long she has had them, and she has not consulted anybody about them, as they have not worried her. She thought they were moles. You will see what I take to be degenerated xanthoma lesions on her, which have taken on a fibroid type. Her sister, whom I shall show soon for another complaint (morphœa) has a lesion on the right eyelid of a different nature, that is, fibroid and somewhat papillomatous: I do not think it was ever xanthoma. Another point in which this case differs from Dr. Sibley's is that the patient gives a history of xanthoma being present in several members of the family on her father's side; relatives on her mother's side are

not so affected. In regard to treatment, I am sure that as she has no diabetes dieting will not have any effect on the lesions here, and I propose to leave that alone.

Case of Morphœa Guttata (White-spot Disease).

By J. L. Bunch, M.D.

This is the sister of the patient I have just shown. You see a lesion on the right eyelid, and it is obviously papillomatous. I am not showing her because of that, however, but for the morphœa guttata lesions, of which she has a large number. On her back first, from thence they have spread to her chest and arms. So far, her face has escaped. She is aged 47, and has had these lesions twelve months. A lot of small lesions have coalesced to form larger ones. I have not previously seen a case of so-called white-spot disease in which there has been such a marked coalescence of lesions as here. Some of the lesions have a reticulated appearance. There are two more or less circular patches on the back of the neck, each about 1½ in. in diameter, surrounded by numerous smaller spots, varying in size from a pea to about a threepenny-piece. The lower patch is obviously formed by the coalescence of numerous small lesions, such as are seen situated outside the large plaques, and are still discrete. The colour is a dead white, and to the touch the spots and plaques are definitely sclerodermatous. She has had no lichen planus, nor any lesions in her mouth. She is anxious lest they should spread to her face.

I have under my care another patient with the same complaint, and of about the same age as this woman. The other is very much like the case which Dr. Haldin Davis showed some time ago, and in that case there is the coincidence that she has myxœdema, and has been taking thyroid extract for it during some time. It is usually held that thyroid is a good remedy for sclerodermia, but the other patient has contracted the white-spot disease during the time she has been taking thyroid. The other patient has recently had X-rays, and with little benefit; so I have ordered radiant heat baths for her. These have caused a marked erythema of the shoulders, and the lesions seem to be softer. No fresh lesions have appeared while she has been having the baths and X-rays.

Case of Acute Lupus Erythematosus.

By E. G. Graham Little, M.D.

The patient is a young married woman, aged 24. She has been married for four years, has never been pregnant, and has always been a delicate girl. She suffered from tuberculous glands, and was operated on for an empyema at the age of 7. A brother died of phthisis at the age of 24, in 1912, and both parents died in her childhood. This patient shows signs of pulmonary tuberculosis in both lungs, but apparently active disease has ceased, and there is no sputum. She first showed the present eruption seven months ago on the bridge of the nose, and an alarming spread of the eruption was noted in the last month. She was brought to my department on the 15th inst. The present distribution of the eruption is extensive, the greater part of the face being now occupied by a very congestive lupus erythematosus, large patches of which are also found on the upper arms, the elbows, the dorsum of the hands, and all the fingers, the feet, and the mucosa of the hard palate. She complains of arthritic pain, so that she cannot walk easily, or even turn in bed, and she feels extremely ill. There is very moderate pyrexia, not exceeding 100° F. at intervals. A further report of this case will be submitted later.

DISCUSSION.

Dr. J. J. Pringle: Is there any history of personal or family tuberculosis in this case? When I made an analysis of my acute cases of lupus erythematosus, some years ago, I found that a very large proportion of them had either active tuberculosis or a tuberculous family history.

The President: I take a bad view of cases of this type, and I shall be surprised if that patient is alive in two years' time. Recently I have had in hospital a case in a girl with a condition much like this, and she had a very septic mouth when she came in, but I could not say whether her mouth condition was secondary to or the cause of her disease. I do not think the disease was due to the mouth sepsis, because the lupus erythematosus got worse after we had cleaned her mouth. She had a marked hectic fever which persisted after the sepsis of the mouth was over. I was away ill, and during that time she died in the hospital. A most complete post-mortem examination was made, but no cause of death, beyond the chronic toxæmia, has been found. There was no sign of tubercle, no pneumonia, nor anything wrong with any

organ. She simply showed symptoms of profound poisoning, and died. Before she became so ill, we investigated her thoroughly, and on several occasions attempted to cultivate her blood, but, apparently, there was no true septicæmia. I do not say that all such cases are fatal, but I do not consider the present patient will live very long. My patient was on high doses of quinine during her whole stay in hospital.

Dr. F. PARKES WEBER: Do any members know whether the Calmette or the von Pirquet reactions have been tried in any cases of severe and acute lupus erythematosus like the present one? The trial is hardly likely to be made at the present time. But it would be interesting to hear whether such patients show extreme hypersensitiveness towards tuberculin or not.

Dr. GRAHAM LITTLE (in reply): I agree with the President and Dr. Pringle as to the serious prognosis in these cases. I remember two such cases which died in a much shorter period than two years. I admit to bed cases of the kind, because they need much care and good nursing if any arrest is to be brought about. We have to pay especial attention to the possibilities of intestinal toxæmia, to give large doses of quinine and keep the patient in bed. I shall be pleased to furnish a further report of the case, with a more complete investigation. I have never done the particular reaction tests of which Dr. Weber spoke.

Case of Dermatitis Herpetiformis (Hydroa Gestationis).

By E. G. GRAHAM LITTLE, M.D.

THE patient is a married woman, aged 40, and of good physique. She has had sixteen children, with no incidents until the last confinement, on April 18, 1919, six weeks after which an acute eruption of dermatitis herpetiformis appeared, which has persisted, and, indeed, become aggravated. When she came to my department a few days ago there was a very extensive eruption covering the whole of the trunk, and consisting of two types of lesion, the common grouped vesicles and the rarer vesicating rosette-like rings which Hallopeau has called dermatitis herpetiformis "en cocarde."[1] These circles, at first small, the size of a shilling or less, rapidly extend centrifugally, and both the patches and the vesicles are accompanied by the most intense itching. The mucosæ are unaffected, but there are some minute vesicles along

[1] Musée de l'hopital St. Louis, Atlas, p. 75.

the edge of the left lower eyelid, but not on the actual conjunctiva. The eyelashes here have disappeared, probably having been scratched away.

The gestation cases are certainly among the rarer forms of this disease, and on the whole are of better prognosis than those not connected with pregnancy, if this condition can be avoided. I showed some years ago a very severe case of this type in the wife of one of our porters at St. Mary's Hospital, and she has had no repetition of the rash, nor I believe of pregnancy. The eruption may be delayed for a short time after delivery, and in fact it would seem to be commoner for the rash to come out after delivery than before.

Section of Dermatology.

President—Dr. Arthur Whitfield.

Case of ? Mycosis Fungoides.[1]

By E. G. Graham Little, M.D.

The patient is a man, aged 54, a native of Wiltshire. I saw him for the first time on January 9, when he was sent to me by Dr. Kempe, of Salisbury. He had at that time a very rugose infiltrated inflammatory condition of the groins and, to a less degree of the axillæ, but the rest of the body was free. The condition was extremely itchy, and had lasted about ten months. He gave a history of the eruption having come on after wearing a pair of riding breeches which he had bought from an Australian soldier twelve months before. I was misled by the history and distribution and suggested the diagnosis of eczematoid ringworm, although a careful search for fungus was unsuccessful. I prescribed an ointment of salicylic acid and oil of cade with collosol iodine oil for the most tender areas about the genitals. I saw him again on March 23, when a totally different eruption had come out over the greater part of the trunk, the thighs and calves—an eczematoid, exudative, evil-smelling generalized eruption, causing agonizing itching, with much glandular enlargement and extreme general depression and feeling of illness. The infiltrated pad-like masses in the groins and axillæ had increased in thickness and extent. I now formed the opinion that it was a case of early mycosis fungoides, very like a case which I showed here a couple of years ago, in which the earlier diagnosis, suggested by a general consensus of opinion at the meeting of the Section at which the patient was shown, was that of an extensive seborrhœic eczema, but which developed unmistakably into mycosis fungoides. I should be glad of opinions on this difficult case.

[1] At a meeting of the Section, held April 15, 1920.

Case of ? Lupus Erythematosus (Anomalous type). ? Folliculitis Ulerythematosa Reticulata of McKee.

By H. C. Samuel.

The patient is a married woman, aged 34. She has two children, a boy, quite healthy, and the little girl, aged 5, who is being exhibited to-day also. I saw the mother about two years ago; there were then large areas of telangiectases upon both cheeks. At that time I thought the condition was an early stage of lupus erythematosus. At the same time the little girl was being treated for small scattered papular lesions which lasted a short while and when involuted left behind small round scars. Some of these scars are still visible on the cheeks, and she has recently developed another papule on the right cheek. I regard these spots as papulo-necrotic tuberculides. The girl's face, too, shows the same telangiectatic condition as the mother's, and I have no doubt that in time the same appearance will develop in the child as in the mother. Both have poor circulations (note the girl's legs); both suffer from chilblains.

The mother is quite certain that until the age of 7 she herself was noted for the great pallor of her complexion, and it was not until that age that a so-called "healthy colour" developed (probably nothing more than telangiectasis). Recently I have noticed on both cheeks a large number of pitted scars (quite small) so that the affected parts show a reticulate appearance, the small pits as it were alternating with the network of dilated venules. Some more recent pitting has just developed farther back on the cheek. There has never been any suppuration, nor any sign of inflammation or scaling, and I cannot help regarding the case as an anomalous form of lupus erythematosus. Dr. Graham Little showed a case here a few months ago which, so far as I remember, resembled this one in some ways. In his case there was pitting and a sievelike appearance on the face without previous suppuration, but in my case there is much more skin affected, and the general arrangement of the pitting is more reticulate; in addition there is a large network of telangiectasis which *preceded* the pitting, covering nearly the whole of the face. Dr. Little's case was in a boy who was, I believe, free from telangiectasis, the lesions being confined to the sebaceous glands.

I am not familiar with this type of lupus erythematosus, but to my

mind I think it ought to be put into that category; it exhibits two of the cardinal signs—i.e., telangiectasis followed by superficial scarring without preceding ulceration or suppuration, but the scarring is in the nature of small reticular pitting and the area affected is one we associate with lupus erythematosus. The weak circulation and the family history of association with tuberculosis are all in favour of that diagnosis. Whether this case corresponds with McKee's folliculitis ulerythematosa reticulata I do not know and shall be glad to learn.

Two brothers and several maternal relations suffered from tuberculosis, and then from the child's papulo-necrotic lesions.

DISCUSSION.

Dr. G. PERNET: I think this case comes into the same category as one which I published some time ago, the patient being a girl. I gave to the condition the name, "Atrophodermia reticulata symmetrica faciei."[1] Dr. McKee, of New York, wrote an article on the same sort of condition, and gave a long string of synonyms.

Dr. GRAHAM LITTLE: I think this case is more likely to be lupus erythematosus, on account of the flushing and the congestion. Moreover, the cases of reticulate atrophy were all in very young children. And in this case there is no comedo formation, which was one of the characteristic features of McKee's disease.

Mr. H. C. SAMUEL (in reply): I have classed this case with lupus erythematosus, and the only way in which the pitting differs from lupus erythematosus is, that there has been no previous scaling. If there had been adherent scales one would have expected that appearance. I called attention to the marked association with tuberculosis in this patient, and I would ask whether that is a feature of McKee's cases or whether it favours the diagnosis of lupus erythematosus.

Case of Mycosis Fungoides.

By E. G. GRAHAM LITTLE, M.D.

I SHOWED this man at the December meeting,[2] as a possible instance of mycosis fungoides. At that time he was of a dusky-red hue from head to foot, his whole skin being infiltrated, and there was much dark pigmentation, though not quite so deep as now. He has very much

[1] Pernet, *Med. Press and Circ.*, May 31, 1916, p. 487.

[2] *Proceedings*, 1920, xiii, p. 24.

enlarged glands, in the groin and elsewhere. At that meeting I think my diagnosis of mycosis fungoides was generally accepted, but Dr. Parkes Weber preferred to regard it as lymphodermia perniciosa, which is probably akin to mycosis fungoides. I could not, and I cannot now, explain the pigmentation, which has been continuous throughout. His occupation does not support any idea of arsenical poisoning: he is a fitter, and I have given him no arsenic, and I cannot learn that he has been taking any. The further developments which have taken place have been in the abdominal enlargement, and the noticeable œdema of the scrotum and penis. A week ago I saw him, and he was then complaining of pain in the abdomen. The skin of the abdomen, as well as that of the rest of the body, was intensely indurated, but I could not detect any visceral enlargement, and there was no visceral dullness. In the week's interval he has changed considerably. In September his blood count was practically normal, there was no leucocythæmia of note. He had then 14,000 white corpuscles; to-day's figure is 13,300. I ask for suggestions, and for explanations of this out-of-the-way picture. When I first saw him there was also intense pruritus, which seemed relieved by simple tar applications.

Dr. F. PARKES WEBER: I still think this must be a case of lymphodermia perniciosa of Kaposi, that is to say, that it belongs to the class of leukæmia as distinct from mycosis fungoides. In all probability, if we were able—which we are not—to examine either the enlarged lymphatic glands or the enlarged liver of this patient, we should already find evidence of leukæmic infiltration. I maintain that this case will turn out to be one of leukæmia, of the primarily cutaneous type, namely lymphodermia perniciosa, as first described by Kaposi. In the present case the pigmentation is obviously merely a temporary so-called "epidermic pigmentation," and I do not think it affects the question of diagnosis.

Case of Lupus Pernio.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a woman aged 65, who states that the condition started eight years ago with an accident, when she fell upon her nose and bruised it considerably. The infiltration and cyanosis have steadily increased from that date. At present she shows a remarkable pad-like deeply congested infiltration of the skin of the nose and both cheeks, but there is no alteration of the surface skin. The fingers and hands are unaltered, and there is no general "chilblain circulation."

I have been somewhat at a loss for a diagnosis, as tuberculous affections beginning at this age are in my experience rare, and the surface is entirely unlike tuberculous disease of the skin. I think Dr. Adamson's suggestion of lupus pernio is probably correct, although the complete absence of the congestion of the fingers is certainly uncommon in this rare affection of which I have had no previous personal experience.

DISCUSSION.

Dr. H. G. ADAMSON: I think this is a very characteristic case of lupus pernio. Lupus pernio is not a common disease. I can only remember having seen two definite cases: One I saw for many years with Dr. Colcott Fox, in the person of a lady. The diagnosis of this case had been made by Besnier, who first described this disease. The other case was in Brompton Hospital, and the patient eventually died of what was clinically acute tuberculosis. In characteristic cases of the disease there is a purple coloured, indurated enlargement of the nose, and sometimes of both cheeks, as in this patient. It may also affect the extremities of the fingers and the ears; in other cases there is enlargement of fingers. If one presses firmly on the indurated swellings, one sees what appear to be "apple-jelly" nodules, and sometimes those nodules appear on the surface and are evident without pressure. On cutting sections, one gets what looks like tubercle; there are extraordinarily large giant cells. It has recently been suggested that it is a special form of benign granuloma. In some cases there are blood changes, and in most of them, if you take skiagrams of the hand you find what appear to be cavities in the bones of the fingers, which probably represent deposits of the same tuberculous character. I suggest that X-ray photographs be taken in this case. Other cases have had enlargement of the spleen and various glands. It has been suggested that lupus pernio is the same disease as the so-called "sarcoid" of Darier. X-ray treatment was the only form that did good in the first case I saw, that of the lady, who now seems to be cured. I think the treatment now generally recommended is arsenic, on the idea of its being a benign lympho-granuloma.

Dr. BARBER: I have had a case rather like this, which I think Dr. Adamson would probably include in the sarcoid group. I have been treating her, during the last three months, with increasing doses of novarsenobenzol intravenously. Her general health has improved very greatly as a result of the injections, and some of the nodules on the nose have diminished in size, though they do not look as if they will disappear altogether. I shall give her X-ray treatment, but the effect of the injections on her general health has been striking. One interesting point in her family history is that her sister, who came to see me at the same time, has ordinary lupus of the cheek. At present there is no clinical evidence of tuberculosis in the lady who has the sarcoid condition.

Case of Lupus Erythematosus treated with Autogenous Streptococcal Vaccine prepared from Enucleated Tonsils.

By H. W. Barber, M.B.

During the past year I have been collecting cases of lupus erythematosus in order to investigate them from the point of view of focal streptococcal infection. I had hoped to bring to-day three or four patients, in some of whom the effect of treatment has been even more striking than in this patient.

Wm. H., aged 34, was in the Navy for seventeen years. History: As a child he had scarlet fever, and measles three times. Otherwise his health was good. His eruption first appeared in August, 1911, on the right cheek near the eye: he was then on board ship. A patch then came out on the other cheek, and gradually the disease spread, until his ears and post-auricular regions became involved. The last patch to appear was that on the bridge of his nose. In January, 1919, he came under the care of my clinical assistant, Dr. Scott, who was then at the Naval Hospital, Southend. Dr. Scott treated him with CO_2-snow, with some success, but the condition relapsed. He was then referred to me, and he was in Guy's Hospital from June to September, 1919. An exhaustive examination for *latent tuberculosis* was made without success. *The fæces* were cultivated for streptococci but none were found. Treatment with CO_2, and a carbolic and lactic acid mixture locally were given and temporary improvement followed, but relapse again took place. Mr. Mollison kindly examined the patient and stated that there was very marked sepsis in the tonsils. The tonsils were therefore enucleated by Mr. Zamora on October 13, 1919. Cultivation from the tonsils gave a pure growth of *Streptococcus longus*. Since this time the patient has been under the joint care of myself and Dr. John Eyre.

October 20, 1919: 5 million vaccine given. Considerable focal reaction.

October 28, 1919: 7·5 million. Very marked focal and general reaction.

November 11, 1919: 5 million. Improvement noted.

November 18, 1919: 7·5 million. Marked reaction. Improvement noted.

November 25, 1919: 5 million.

December 4, 1919: 7·5 million.
December 16, 1919: 7·5 million. Very considerable improvement.
December 30, 1919: 10 million.
January 12, 1920: 10 million.
January 27, 1920: 10 million.
February 10, 1920: 10 million.
February 24, 1920: 10 million.
March 9, 1920: 12·5 million.
March 23, 1920: 12·5 million.
April 6, 1920: 10 million. Marked reaction in thirty-six to forty-eight hours.

Under the vaccine treatment (with no local treatment except soothing applications) an extraordinary improvement has resulted. All the injections have been followed by focal reactions in the patches, sometimes very intense, and often with general reactions.

The patient states that the effect of the vaccine on the patches is felt for about a week.

As I have said, I have had other cases too, some as bad as this man, others not so bad. One of the most successful that I have had was that of a lady aged 34, who gave a history of repeated colds, but was otherwise well. Her rash appeared last July, so when she came under my observation in October, the condition was fairly recent. She had typical lupus erythematosus, of the superficial type, involving the nose, cheeks, ears, and there were several patches on the hands. I took her into hospital, and investigated her case in the same way. The results in regard to the fæces and the teeth were negative. At first, when I examined her, I thought her tonsils were healthy, but Mr. Zamora demonstrated that liquid pus could be squeezed out of both of them. The tonsils were enucleated, and following the operation she had, for several days, a very high temperature, and developed a severe streptococcal infection of her throat. During the febrile period she had much focal reaction in her skin, the erythema spread, her hands burned, and she felt ill. She has had autogenous vaccine since, and has done extraordinarily well, and is now practically cured. She never had local treatment.

I shall not give details of other cases, but in all the patients I have investigated the focus of infection has been either about the teeth, in the tonsils, or in the intestines. One of my patients has improved much more than the one I show you, though when I saw her first the condition was extremely acute; I feared it was going to be a case similar to that which Dr. Little showed last time. She had severe oral sepsis,

and I was afraid that if I took out all her teeth at once her skin condition would probably flare up, and that she might die of septicæmia. Therefore we proceeded gently in regard to the teeth. She bore the operations well, and the improvement in the rash following the mere extraction of the teeth was remarkable. She had a little increased irritation for twenty-four hours, but during the next few days the inflammation diminished very much. She not only had streptococcal infection of her gums, but also a long chain streptococcus in considerable numbers in her fæces. She is attending Dr. Eyre's out-patients' department, and is gradually getting well. I think her tonsils are infected too, but she is not anxious to have them out, and as she is doing well I propose to let her go on without that operation. I have one case of lupus erythematosus associated with atrophic rhinitis; I think it very probable that he has also got *Streptococcus longus* in his intestines, but I have not yet been able to take him into hospital to investigate him. In one case, which was referred to me by Dr. Semon, the teeth were extracted in 1915, and he has a set of false teeth. His tonsils were not obviously septic, but his fæces yielded many colonies of *Streptococcus longus*, and a vaccine has been prepared from that organism.

BACTERIOLOGICAL NOTE—BY F. A. KNOTT, M.B.

It is possibly of interest to mention that these vaccines contained exclusively the microorganism *Streptococcus pyogenes longus*. In each case several cultivations were made from the deep tonsillar crypts; more perfectly, of course, when enucleation had been performed. The patient's mouth was also examined for evidence of pyorrhœa, and if any purulent exudate could be seen, swabs were taken. A fæcal specimen collected after administration of mild laxatives was also bacteriologically examined in each instance.

From all the mixed cultures thus obtained, streptococcal colonies were picked off and those which exhibited most of those characteristics commonly associated with pathogenic streptococci were combined for each individual patient into a simple autogenous vaccine. Briefly, the criteria considered in selecting the bacterial strains included: smallness of the colonies, length of the chains, production of marked acidity in litmus milk, and of some degree of hæmolysis when grown on blood-agar, the typical growth in nutrient broth and an absolute insolubility of the culture in fresh dilute bile. All other common catarrhal organisms were discarded.

In every case investigated, the pathogenic type of streptococcus was grown in abundance from both the tonsils and teeth. In more than 50 per cent. of them it was found in abnormal numbers in the fæces, and as Dr. Barber has already explained, the reactions following the inoculation of these vaccines were so marked that it was impossible to give larger early doses than 2·5 to 5 million autogenous streptococci, and in most cases the subsequent increase in doses had to be made with great caution.

The result of initial treatment, the excessive focal reactions then produced and the persistent effects which followed almost all later doses were particularly noteworthy when compared with the customarily observed immediate results of administering autogenous vaccine in other subacute streptococcal conditions such as rheumatoid arthritis, chronic sinuses, cystitis, &c. Also it is to be noted that as both the dose and the total number of inoculations increased, the focal reaction hardly decreased at all in intensity, and this is exemplified by the fact that in the last dose given to the patient W. H., which came at the end of a long series, the previous focal reaction had been so marked that a reduction of dose strength was considered advisable.

It may also be mentioned that similar doses of these vaccines were administered both to ourselves and to patients with lesions of origin presumably other than streptococcal, and that on no occasion was a reaction obtained in any sense comparable to those seen in Dr. Barber's cases.

DISCUSSION.

Dr. GRAHAM LITTLE: The case of mine to which Dr. Barber refers has been a month in St. Mary's Hospital, and, on Dr. Barber's suggestion, I have had examinations of the blood made, and they are entirely negative. There is nothing at all wrong in regard to the teeth or tonsils; the tonsils have been examined by the laryngologist to the hospital. The pathologist was specially warned to look for the streptococcus in the blood. The case, however, shows undoubted symptoms of pulmonary tuberculosis, and there is a family history of that disease. The patient is now very ill, and the disease is spreading with alarming rapidity, fresh foci forming in only a few days. There is also a slight rise in temperature, up to about 101° F. I am at a loss to explain this case on the streptococcus hypothesis, though I admit Dr. Barber's communication is a very important one.

Dr. MACCORMAC: I am not sure if Dr. Barber holds that every case of lupus erythematosus arises from focal streptococcal infection: with such a view I should be inclined to disagree. Influenced no doubt by the teaching

of Dr. Pringle, I have been accustomed to regard most if not all cases of this disease as associated in some manner with the tubercle bacillus. In this connexion it is interesting to recall that Arndt, in 1910, and Ehrmann and Reines, in 1908, succeeded in finding the bacillus in the lesions themselves, and in infecting guinea-pigs with the tissue.[1] Moreover, too much weight should not be attached to the improvement following the injection of the streptococcal vaccine. This result does not necessarily demonstrate specificity as Engman has recorded similar changes after the use of typhoid vaccine, an observation which suggests that the improvement may be due to the introduction of a foreign protein into the circulation.

Dr. H. G. ADAMSON: I think we should hesitate before we accept Dr. Barber's results as a proof that lupus erythematosus is due to the streptococcus. One finds focal reactions occurring after all sorts of treatments of lupus erythematosus—after arsenic, for instance. I gave a series of injections of the typhoid bacillus in six cases of lupus erythematosus, and these injections gave rise to focal reactions. They all improved under it, but most of them afterwards relapsed. Merely keeping a patient in bed will generally lead to improvement. I do not think the mere fact that the patients get improvement after a focal reaction from streptococcus vaccine means that the streptococcus is the specific organism in lupus erythematosus.

The PRESIDENT: Some points have arisen in this discussion which induce me to say a few words. The first point to which I would refer is that of focal reactions after injections, because I have a case very much to the point in favour of Dr. Adamson's view. About five or six weeks ago a lady was sent to me who had, there is no doubt, a perfectly ordinary scabies, dating from last July. She was seen by a general practitioner, who diagnosed scabies. She was offended, and did not believe him, and consulted a skin specialist, who told her she had urticaria, but he failed to cure her. He then sent her to Harrogate, where she got somewhat better under sulphur baths. It was decided, however, that her condition was due to a streptococcus in the teeth and gums, from which the streptococcus was cultivated. Luckily, they did not extract the teeth. A vaccine was made and injected. She and her doctor who originally made the diagnosis of scabies, but whose diagnosis was contradicted by expert opinion, are agreed that for forty-eight hours after each injection of the vaccine the itching entirely stopped, and she had a temporary relief from her skin troubles, so that she thought she was slowly improving. She had the ordinary acarus, and was cured in a week by the ordinary methods. The case is important because it shows we have to be extraordinarily careful how we assume that a vaccine is specific because it temporarily either benefits or aggravates the symptoms. The other point I have to mention is, that I have a case somewhat on all fours with Dr. Barber's, that of a girl with lupus erythematosus. Her father died of laryngeal phthisis, and I was suspicious that

[1] Lewandowsky, quoted in *Journ. Cutan. Dis.*, February, 1919, xxxvii, No. 2.

she might have tuberculosis. I sent her to a laryngologist to be very carefully examined, because I have been at this focal infection work for some time myself, and he reported that the nose and tonsils were quite normal. I then sent her to a physician whom I regard as a great expert in finding early tuberculosis, to have her chest examined to see if he could find apical or root mischief. He did not, but he found a suppurating tonsil, which the laryngologist had overlooked: it was very difficult to see. We had the tonsils enucleated, and over a teaspoonful of pus was taken away from them, and it gave the *Streptococcus longus* on culture, from which we made a vaccine, but held it in reserve. The tonsils were enucleated last September; since then she has had no local and no vaccine treatment, and she has very slowly got better. My idea is, that if the trouble was due to her tonsils she will slowly get well, though possibly more slowly than if she were inoculated with her own vaccine. Perhaps I can get to know how she is getting on, and report to the Section whether the mere removal of the infection has done what was necessary, because any question of protein shock would not enter in such a case as that.[1]

Dr. G. PERNET: I have long held the view that lupus erythematosus depends on a variety of ætiological factors, including tuberculosis. With regard to septic foci, I remember many years ago, one of my earliest cases of lupus erythematosus was in a woman, and I examined her mouth and found her teeth and the mouth generally in a very bad condition. I recommended the removal of all the defective teeth and that the mouth should be put in order. When I saw her again, after an interval, the improvement in the lupus erythematosus was very great. I had done nothing except order calamine lotion, because I had relied on the efficacy of cleansing the mouth to bring about the improvement, which it did. This was before the days of vaccine therapy.

Case of Adenoma Sebaceum.

By W. KNOWSLEY SIBLEY, M.D.

THIS little girl is aged 9. Her family history is apparently good. Father and mother are living and well. She is the tenth of twelve children, all living and well. Her mother states that when this child was 5 months old she had a fright, which was followed by the occurrence of fits, and she remained dull and apathetic for some months afterwards. At present, apparently, she is behind the rest of the family in intellect to some extent, though there is no marked mental change. I show her

[1] Since the meeting I have seen her again, and she is quite cured; it is impossible now to see any marks of the disease.

because of some unusual features. In addition to the usual well marked lesions on her face, she not only has the typical warty growths on the sacro-lumbar region, but she has areas of white skin scattered about: there are two patches on the left shoulder and another on the front of the right thigh. In all these cases of adenoma sebaceum there are various phenomena of the skin, generally described as pigmentation areas, nævi, and fibrous growths, but I have not previously seen a case with patches of white skin. I do not think it is vitiligo: the areas are asymmetrical, and apparently there is no change in the skin except in the pigment being absent. One sees cases with patches of white skin in people whom one could scarcely describe as having leucodermia. What should this condition be called?

DISCUSSION.

Dr. F. PARKES WEBER: I suppose that the epileptiform "twitchings," to which, according to the mother, this patient was subject between the ages of 6 and 18 months, indicates that the adenoma sebaceum is associated with a condition of so-called tuberous sclerosis of the brain—in the present case, as it has been shown to in many other cases.

The PRESIDENT: About fifteen years ago, by the kindness of the late Dr. Colcott Fox, I was handed a nearly complete set of all the lesions found in adenoma sebaceum, by a medical officer at one of the idiot asylums, whose name I regret I forget. In these institutions it is not at all an uncommon condition. The asylum doctor divided the lesions into adenomatous lesions, that is true adenoma sebaceum, angeiomatous lesions, which are often mixed with adenoma sebaceum, forming the "Pringle type": hairy moles, flat fibrous patches, white skin, brown skin. In the white skin cases there is very little to show, microscopically, in a fair-skinned patient, and the white fibrous patch is also difficult to distinguish too, as under the microscope it shows merely a little thickening.

Case of So-called "White Spot" Disease.

By GEORGE PERNET, M.D.

THE patient is a girl, aged 18, employed at a laundry. Two years previously, according to the mother, a small reddish spot appeared on the lower part of the left side of the back of the neck. Since then it has slowly extended, and now forms an oblong white patch measuring

about 1 in. transversely by ½ in. It is atrophic and crinkly. In the central portion there is a group of follicles with some perifollicular keratosis. Scattered about the upper part of the back are several small spots, about a ¼ in. in diameter, which have appeared recently. They are atrophic, slightly below the level of the skin, and exhibit a delicate border, very slightly raised. Here and there a dilated vessel can be seen showing through. At the bend of the left elbow there is also a similar spot, which requires looking for, and appears to be made up, when the skin is put on the stretch, of two spots which have coalesced. In the central part of all these spots, on examination with a lens the follicles are marked out. The patient is well in herself. The only thing that can be made out is that several of the teeth are badly decayed. I have entitled this case "white spot" disease for convenience. The relations of this condition with sclerodermia and morphœa have to be considered. This is the first case of the kind I have seen among my West London Hospital out-patients for the last five years at any rate.

An Uncommon Form of Erythema.

By Alfred Eddowes, M.D.

This young lady is a landscape gardener. In spring she is liable to patches of eruption such as we see now on the inner aspect of left upper arm. This single patch has lasted a month and more. It is erythematous, more or less œdematous, irritable and bright red; occasionally much raised, hot and tense, and then shows well-marked capillary pulsation. During last week its position has altered slightly in a downward and inward direction—i.e., nearer to the elbow. The surface epithelium is but little changed—perhaps thrown into more marked plots and furrows, studded here and there with small dry papules, which seem to leave slight pigmentation as they fade—suggestive of lichen. She says she does not perspire easily. The irritability of eruption disappears at the periods, but the swelling is greater. She knows about the *Primula obconica*, and says that that plant does not come in contact with her skin, though she works with uncovered arms.

Against the theory of traumatic causation is the little observable change in the horny layer. To some extent the case resembles erythema elevatum diutinum, but on the other hand it exhibits urticarial

symptoms and signs such as we see in acute circumscribed œdema. The more I reflect on the recognized forms of erythema and urticaria the more difficult it seems to give this case a place in our nomenclature, and that is saying a good deal.

DISCUSSION.

Dr. F. PARKES WEBER: Surely there must in this case be some local cause: the exudative eruption resembles the local serum-rashes following injection of diphtheria-antitoxin and other sera. It may be a case of special susceptibility of the skin towards the local action of some plant-irritant.

Dr. EDDOWES (in reply): If it is due to a local irritant I am surprised that an eczematous surface or some disturbance of epithelium is not seen, such as I have produced in my own skin by contact with *Primula obconica*. I cannot think a drug or plant, such as hyacinth or daffodil, would produce this curious erythema without some indication of disturbance of the horny layer.

Case of Demodex Impetigo.

By ARTHUR WHITFIELD, M.D. (President).

THE following is a preliminary report only. In the autumn of 1919 a lady was sent to me with a somewhat resistant impetigo contagiosa of the nose and cheek. The natural complexion was clear and fine and comedo was absent. The impetigo lesions were unusually circinate in character, so much so in fact as to simulate a tinea to a certain extent, though the fact that the edge showed an even formation of bulla rather than closely set miliary vesicles made me confident that it was not ringworm. I examined part of this blister, and was surprised to find that there were numerous examples of *Demodex folliculorum* adhering to the epidermis. Shortly afterwards I saw a second case also circinate and discoid, and again found the demodex present in large numbers.

The patient of whom the present photograph was taken came to me at the hospital. I identified him clinically as of the same type, and prophesied to the post-graduates present that we should find the demodex. They were so numerous that in one field of $\frac{2}{3}$ in. objective and × 7 ocular we found seven parasites. Numerous comedones were

then examined and smears from the unaffected skin as he was seborrhœic, and no demodex was found in these specimens.

A fourth case was seen soon after which did not show the circinate lesion quite so obviously, and again the demodex was found in the lesion but not elsewhere.

In ordinary impetigo I have been unable to find the demodex.

One of the cases was treated with mild sulphur ointment and was cured in a week. I have not found that sulphur is a good treatment for ordinary impetigo, though I am aware that Unna has claimed that it is. I therefore think that the demodex has an ætiological relationship with the lesion. It may be that it carries the streptococcus into the epidermis, or it may be the actual irritant. This is difficult to decide, since the first contamination of a bulla, however produced, is the streptococcus, and its presence therefore does not prove that it is the prime cause.

I have from time to time seen patients who have given a history of a scabby dermatitis of the face, "caught while treating a dog with the mange," but I have not seen the disease in its active state. One form of mange in dogs is accompanied and probably caused by a demodex as far as I know indistinguishable from the human *Demodex folliculorum*.

So far as I have been able to find out, this disease has not been described hitherto. The lesions already described as associated with the demodex, as far as I can ascertain, are: discoloration of the skin, especially around the mouth, obstinate acne, rodent ulcer, and epithelioma.

I have provisionally named this disease "demodex impetigo."

DISCUSSION.

Dr. A. M. H. GRAY: I should be glad if the President can tell us, off-hand, at what age the demodex appears on the skin. Is it ever found in children or does it appear first at puberty?

The PRESIDENT (in reply): I have not seen demodex in children at all. The first of my cases was a married lady, the second also was a married lady, and the third is this young man. The last case I saw at the hospital; he was 25 to 30 years of age. Last Tuesday I was looking for the demodex in a child who showed very slight tendency to ring formation, but there was no demodex there. In these cases the edge of the bulla was very clear; it had a pearly edge, and it was easy to strip a piece off and look on the under surface of it. I agree that one sees marked circinate impetigo in young

children, but I have not seen a case of this kind in a child since I began this investigation. I have had great difficulty in finding demodex in the ordinary adult patient, and I believe that frequency of occurrence is much exaggerated.

Note.—Since the meeting Dr. Pernet has kindly drawn my attention to the fact that in the British Museum publication on demodex reference is made to the fact that demodex has been described by Dr. Hermann Lawrence, of Melbourne, in cases of ringed impetigo.[1] To him, therefore, belongs the priority of observation.

[1] *Med. Journ. of Australia*, Sydney, 1915, i, p. 442.

Section of Dermatology.

President — Dr. ARTHUR WHITFIELD.

Chronic Fibroid Subcutaneous Syphilomata of the Legs, associated with Chronic Peri-urethral Induration in the Penis.[1]

By F. PARKES WEBER, M.D.

THE patient, T. C., aged 60, is a healthy-looking, well-built Englishman, who has generally enjoyed good health. He gives a history of gonorrhœa, once only, at the age of 20, but, as far as he knows, has never had syphilis. His blood serum, however (April 12, 1920), gives a strongly positive Wassermann reaction. There is no history of gout, but he has occasionally suffered from lumbago-like attacks. A peri-urethral nodular induration can be felt in the distal half of the penis. This induration is apparently in the fibrous tissue of the (imperfect) septum between the corpora cavernosa, but more to the right side than to the left. It causes the distal portion of the penis to be bent dorsalward and to the right during erections, but at the present time does not prevent sexual intercourse, and gives rise to hardly any pain even during erection and sexual connexion. This trouble apparently commenced two years ago; about that time there was sexual impotence, from which, however, he has recovered. A course of subcutaneous injections of fibrolysin was tried, but seems to have made no difference.

The patient likewise has multiple subcutaneous, rather hard, nodules on his lower extremities (chiefly on the right lower extremity) —painless and hardly tender to pressure—varying from the size of a hazel nut to that of a medium-sized chestnut—and all of them situated between the ankle and a level just above the knee, excepting one, which is on the outer side of the middle of the right thigh. They began to

[1] At a meeting of the Section, held May 20, 1920.

appear seven or eight years ago, but were, according to the patient, at first temporarily got rid of by some kind of medicine. For *biopsy* purposes one of these nodules was excised on April 13, 1920, from the inner side of the left lower extremity, just above the knee. It was lying deep in the subcutaneous fat and quite close to, though not actually adherent to, the capsule of the knee-joint. On cutting into it, it seemed macroscopically to consist of hard white fibrous tissue, irregular in outline (not encapsuled). Microscopic examination showed a dense mass of fibrous (scar-like) tissue surrounded by and including numerous foci of chronic inflammatory small-cell infiltration (lymphocytes, fibroblasts and some plasma cells). No hæmorrhages and no necrotic areas were seen in the sections, nor was there any decided obliterative thickening of the small blood-vessels. This examination, in which I was kindly assisted by Professor S. G. Shattock, proves that the subcutaneous nodules are not true tumours (fibromata), but are due to some kind of chronic inflammation, probably syphilitic. They are probably tertiary syphilitic gummata—or *syphilomata*, if the word *gummata* is wrong—though they have apparently lasted for years and have become fibrous without undergoing a true gummatous or necrotic change.[1] We might call them chronic fibroid syphilomata. Recently potassium iodide has been tried for two or three weeks, without obvious result.

I should also mention that the sight of the right eye has been gradually failing for the last twenty years. Dr. C. Markus reports that there is old disseminated chorio-retinitis of the right eye, with posterior cortical cataract connected with it. With that eye the patient can count fingers at a distance of 2 in. There is no disease of the left eye.

There is no evidence of any disease in the thoracic or abdominal viscera, and the urine is free from albumin and sugar. The blood-vessels and the nervous system appear to be normal. There are no gouty tophi and there is no Dupuytren's contraction of the palmar fascia. His family history is good. In regard to his ten children by his second wife, six are living, two were killed in the great war, one (a girl, aged 16), died of heart disease, and one (a boy, aged 5), died of meningitis. He likewise had, he says, two children by his first wife, and both of them are still living.

[1] To contrast with this, I may say that recently I have microscopically examined ("biopsy") a *chronic* cutaneous gumma (not ulcerated) from a middle-aged man, very resistant to antisyphilitic treatment. It contained no hard fibrous tissue, in spite of its chronicity.

REMARKS.

The chief interest of the case seems to be the association of the subcutaneous chronic fibroid syphilomata of the legs with the chronic peri-urethral induration in the penis. This latter condition has been variously ascribed to syphilis, gonorrhœa (a chronic after-result allied to gonorrhœal chordee), gout, chronic diabetes mellitus (sometimes only what in England is termed "gouty glycosuria"), local traumatism or chronic irritation, &c.; sometimes, according to C. Mauriac, it may arise spontaneously. The fact that it does not (or does not completely) yield to antisyphilitic treatment does not necessarily prove that it is not sometimes originally due to syphilis. When tertiary syphilitic lesions have undergone a chronic fibrous metamorphosis they cannot be expected to yield readily, if at all, to antisyphilitic treatment. In many middle-aged individuals—especially those who have during most of their life been accustomed to a "generous diet," and have a "gouty," "rheumatic," or "arthritic" disposition, and are subject to attacks of sciatica, lumbago, muscular rheumatism and "fibrositis" of various kinds, tertiary syphilitic lesions doubtless tend to organize so as to produce fibrous tissue instead of breaking down or becoming reabsorbed, as ordinary gummata do. Such persons may develop chronic tertiary syphilitic diffuse fibrosis of the lungs, leading to chronic bronchitis, or bronchiectasis, or they may become the subjects of more or less diffuse fibrous changes in the liver (so-called "syphilitic cirrhosis"), or, as in the case in question, they may, I suggest, present chronic fibrous syphilomata of the subcutaneous tissue of some part or parts of the body.

It is in a similar class of patients that chronic periurethral indurations, and the scleroses more or less involving the corpora cavernosa penis (especially their fibrous sheaths), are most apt to appear. As a typical example I may instance nearly the first case of the kind that I saw (1894). The patient (M. W.) was a solicitor (aged 61 in 1894), a well-nourished active man with plenty of brain work to occupy him. He had suffered from tertiary syphilitic troubles, for which he had been treated at Aachen about 1886 and 1887. There was likewise a history of attacks of gout. In 1895-96 he complained of lumbago and "stiff-neck," with "thickening," he said, in the nape of the neck, and Sir W. R. Gowers would certainly have claimed some of the symptoms as examples of what he (Gowers) was the first to call "fibrositis." It was in 1894 that the patient complained of his erections having lately always been

crooked; and some infiltration on the left side of the penis could be felt. In 1895 there was a firm induration along the median line of the dorsum of the penis, apparently due to a thickening of the fibrous tissue between the corpora cavernosa. He complained of crooked erections at night, the penis curving to the left, doubtless owing to some sclerosis in the left corpus cavernosum. In 1896 I noted that the induration could be felt along the dorsum of the penis, near the root in the middle line, and that during erections the organ curved to the left, but more upwards than before. His urine was free from albumin and sugar, but he was peculiarly susceptible to cold weather, so that urgent calls to micturition sometimes made travelling by train difficult. Sir Jonathan Hutchinson, with whom I saw the patient in 1896, gave no very hopeful prognosis in regard to the local condition, but advised a prolonged course of treatment by small doses of mixed iodides, with exercise and general hygiene against the chronic rheumatic tendency. In 1900 I heard that the patient looked in excellent health, but I do not know what happened in regard to the local condition. He is still living and working, aged 87. The patient's eldest son, whom I saw in 1894, when he was aged 17, had what was probably a form of harmless cyclic albuminuria, which we should now term "orthostatic albuminuria."

Another man, aged 64, whom I saw ten years ago, had typical chronic peri-urethral induration, notably in the fibrous tissue between the corpora cavernosa, a little proximal to the glans penis. He was a robust-looking man, of full, rather plethoric, habit, who had lately been prevented from taking much active exercise. He suffered likewise from psoriasis, and gave a history of nephrolithiasis. He had once had a chancre, which was regarded as an "ulcus molle," but gave no certain history of syphilis.

In a stout and plethoric-looking man, aged 48, whom Mr. Charles Gibbs showed at the Clinical Society of London in April, 1905, as an example of "Multiple Circumscribed Fibrous Masses in the Corpora Cavernosa," there was a history of a small sore on the penis at the age of 18 or 19.

I have not here space to refer to the numerous accounts of similar cases that have from time to time been published and to all the extensive scattered literature on the subject. This curving of the penis during erections is generally in an upward direction, or to one side, or upwards and to one side, whereas in typical gonorrhœal "chordee" the curving is frequently downwards. The erection may be curved even before any

distinct thickening can be felt in the corpora cavernosa or in their fibrous capsules or in the (imperfect) fibrous septum between them. Owing to difficulty in sexual intercourse it may cause the patients much distress. Sometimes erections are painful. In some cases the portion of the penis distal in the induration does not take part in the erection. In some cases actual impotence, or the fear of coming impotence, gives rise to severe mental trouble. The condition was discussed by La Peyronie (1743), physician to King Louis XV of France, and I believe it was he or another of the earlier French writers on the subject who made use of the extraordinary expression "strabismus penis," but I heard from Sir William Osler that a patient, who had doubtless never heard of La Peyronie, described the condition from which he said he was suffering, as a "squint" of the organ in question.

Amongst French authors of the nineteenth century who have written on the subject are Ricord (1847), Nélaton (1859), and Verneuil (1882), Delaborde (thesis, 1887). Two papers in the English language that have been much quoted on the Continent are those of Kirby (1850) and Cameron (1879). From the American surgeon, W. H. Van Buren, who described the condition in his treatise (with E. L. Keyes) in 1874, the affection has even been termed "Van Buren's disease."

Sir James Paget, in the second edition (1879) of his "Clinical Lectures and Essays," wrote: "In gouty persons the production of fibroid tissue thickening and contracting the outer layer of the urethral wall, and thus producing stricture, seems to be a process of the same kind as the thickening and contraction of the palmar fascia. . . . Or it may be more nearly compared with that very troublesome and generally gouty thickening and hardening of portions of the fibrous sheath of the corpus cavernosum, which produces distortion of the penis in erection, and sometimes frightens with fright of cancer."

Sir Jonathan Hutchinson (1894) said that Sir James Paget had anticipated almost all that he himself had said on the subject. In one of Hutchinson's cases (Case V) there was a history of previous syphilis. In two other cases to which he referred (Cases VIII and IX) the condition of the penis was associated with Dupuytren's contraction of the palmar fascia. Though Paget and Hutchinson both suggested that the fibroid thickenings in the penis were of gouty nature and were to some extent analogous to Dupuytren's contraction of the palmar fascia (which nobody regards as syphilitic in origin), I believe that only two cases (besides those referred to by Hutchinson) have been recorded in which the changes in the penis and the contraction of the palmar

fascia were met with in the same patient (cases of H. Neumark, in 1906, and R. O. Stein in 1909).

A. Poncet, in 1912, claimed "gouty nodules of the penis" and "plastic indurations of the corpora cavernosa" as manifestations of what he and Leriche called "tuberculose inflammatoire," but apparently without bringing forward any satisfactory arguments to support his claim. Le Fur (1912) thought that tuberculosis was the cause in one of his cases.

The results of leukæmic infiltration and leukæmic thrombosis in the penis belong to the category of so-called "persistent priapism" and I think that they, and thrombosis of the corpora cavernosa from other causes, cannot be confused with the cases at present under discussion.[1]

Amongst the terms under which the condition in question has been described are: "Induratio penis plastica" (plastic induration of the penis); "plastic induration of the corpora cavernosa" (Delaborde) or, as one might say, "chronic indurative cavernitis"; "nodules of the corpora cavernosa" (Nélaton); "multiple circumscribed fibrous masses in the corpora cavernosa" (C. Gibbs); "chronic circumscribed inflammation of the corpora cavernosa" (Donald Armour); "gouty nodules of the penis or of the corpora cavernosa"; "'ganglion' of the penis" (Ricord); "Van Buren's disease"; "cavernitis senilis" (M. Horovitz).

Possibly some of the sixteen or more cases in which there has been actual bone formation in the human penis (Huitfeldt, Hörz, Cinquemani, &c.) are really examples of secondary bone formation in cases of chronic plastic induration of the penis. The bone in the case of a man aged 49, described by A. G. Gerster and F. S. Mandlebaum (1913), was supposed to be due to the pressure (and chronic mechanical irritation) from straight-front corsets on the dorsum of the penis. These authors denied any atavistic relation of the pathological penis bones in human beings to the penis bone normally present, according to Gerhardt of Breslau, in the anthropoid gibbon; also in some other vertebrate animals (dogs, &c.).

Operative treatment by excision has been tried in a few cases of plastic induration of the penis. E. Galewsky and W. Hübener, in 1902, reported the first operation of the kind (which had been performed three years previously, namely in 1899). The result was satisfactory.

[1] Cf. F. P. Weber, "Persistent Priapism from Thrombosis of the Corpora Cavernosa," *Edin. Med. Journ.*, N. S., 1898, iv, p. 267; also F. P. Weber, in the discussion on Dr. Gordon R. Ward's paper on "Nodular Leukæmia," *Proc. Roy. Soc. Med.*, 1912, v (Sect. Med.), p. 105.

The excised nodule on microscopical examination was found to consist of fibrous keloid-like tissue. Stopczanski (1908) likewise reported a good result from excision of an indurated nodule, which a subsequent microscopical examination showed to contain islands of cartilage, but no bone. In Huitfeldt's case and in Cinquemani's case (both already mentioned) a " penis bone " was successfully excised from men aged 50 and 53 respectfully. Operation, however, could hardly prove beneficial in most cases.

Radium treatment seems to have produced good results in a few cases (Dreyer, 1913; Gulewsky, 1916). Electrolysis was useful in Le Fur's hands (1912).

Prolonged treatment by intramuscular injections of fibrolysin (after subcutaneous injections had been abandoned) was apparently successful in one of L. Waelsch's cases, but fibrolysin did no good in most cases.

A few examples of spontaneous cure (Jadassohn, Schäffer, Callomon) are referred to by Muthmann.

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DISCUSSION.

Mr. MCDONAGH: The condition seen in this man's penis is not an uncommon one, although the form presented is a little unusual. The lesion goes by the name of induratio penis plastica and most frequently starts as a thickening of the tunica albuginea. At first the growth is in the shape of a signet-ring but frequently the broad part becomes ribbon-shaped and a process may dip down from the centre owing to an involvement of the septum pectiniforme. In my opinion syphilis is never the cause. The lesion is most apt to affect people who show an idiosyncrasy for fibrous tissue changes; the same patient may have a Dupuytren's contraction, or it will be found that a relative of his has been so affected. In over 50 per cent. of the cases sugar is demonstrable in the urine or the patient has hyperglycæmia. Microscopically the lesion is an ordinary inflammatory fibrosis. The cause is possibly an

increased "acidness" of the blood because in early cases colloidal iodine and intramine will bring about its dissolution.

Dr. J. J. PRINGLE: Has any one present observed or even read of the development of organized fibrous nodules, like those in the patient exhibited, from syphilitic gummata.

Dr. PARKES WEBER (in reply): I have not heard of similar cases of fibroid transformation of subcutaneous gummata, but I do know that some of the most insidious forms of visceral syphilis become fibrotic. It is the fibrosis of the lungs due to syphilis which is one of the most dangerous and insidious forms of visceral tertiary syphilis. And it is not very rare to find fibroid patches in the liver due to tertiary syphilis. I think the present case is a very rare instance of tertiary syphilitic subcutaneous lesions having become partially fibrotic, as syphilitic lesions sometimes do in the liver and the lungs; the peri-urethral fibroid induration (so-called "induratio penis plastica") may perhaps in this instance be accounted for in the same way.

Case of Lymphangioma.

By J. L. BUNCH, M.D.

THIS little girl has had lymphangioma since birth. The condition is slightly modified by some treatment she has had, hence the lesions do not show up so well as they did. The angiomatous element is very evident. It has taken the ringed form: it is circinate, the centre is clear, and the little lesions are distributed more or less as a circle around. Judging from the cases shown at this Society, and from the literature, these cases are in any form rare, and the circinate variety as seen here is extremely rare.

With regard to treatment, it is necessary to destroy each lesion separately.

Case of Multiple Granulomata.

By J. L. BUNCH, M.D.

THIS man is aged 23, and he has recently been demobilized. While in the Army he had injections of stock vaccine for the indurated acne, which you see so extensively present on his face. During the latter portion of the time when he was having those injections he developed a number of granulomatous lesions on the limbs. He had his last

injection six weeks ago. In addition, he has had several injections of collosol manganese, and that, he thinks, made the lesions worse, and induced the appearance of some on his face, also that after those injections his nose became involved. He denies syphilis absolutely, says he has never been in the way of contracting it. "Wassermann" has been negative twice, as was also the von Pirquet reaction. A section of the lesions does not show anything more than the ordinary granulomatous structure: there is no evidence of tuberculosis. During the month he has been under my care, he has been on small doses of iodide, but, so far as I can see, with no modification of the lesions. Previously to his coming under my care he took no drugs. He says the lesions on the arm are exactly in the site of some of the injections of the acne vaccine. He also has a lesion in the situation of an old appendicitis scar, and on the leg apart from any injection site. The most marked lesions are on injection sites, but he has a number elsewhere too, probably due to secondary staphylococcal infection. The cultures do not show any signs of sporotrichosis or diphtheroid organisms (as in Dr. Barber's case) and I was rather puzzled by the nature of the lesions since he is positive that he has taken no iodide nor bromide, he is not aware that he has suffered from syphilis, and he has not served in any country where Delhi boil, bouton de Biskra, or similar lesion, is common. He has had no "606." I shall be very glad if any member can give me suggestions as to treatment.

DISCUSSION.

Dr. BARBER: I am interested in the occurrence of lesions at the site of the vaccine punctures. Some time ago I showed here a case of, apparently, diphtheroid infection of the skin, and in that case, too, the patient was quite definite that some of the lesions had arisen at the puncture points, also around the spots of lumbar puncture. In reference to a case later shown by Dr. Graham Little and to my case, has Dr. Bunch had the lesions examined for diphtheroid organisms with direct films and also on culture? In some ways these lesions are reminiscent of my case.

Dr. WILFRID FOX: I think this is an ordinary septic granuloma, due to staphylococci. The only treatment I have known to do good is to scrape them and get them clean.

Dr. H. G. ADAMSON: I do not think this condition is tuberculosis. The lesions are not of the type one is familiar with in multiple lupus. There are no "apple-jelly" nodules. They more resemble the type of lesion known as scrofuloderma, but in scrofuloderma the lesions are always secondary to some

more deeply seated tuberculous trouble, such as a tuberculous gland, tuberculous joint, or bone disease, or a tuberculous abscess; and there are no such deep lesions in this case. I think the diagnosis rests between sporotrichosis or blastomycosis and a bromide or iodide eruption. Since the eruption has not improved on administration of iodides it is unlikely to be sporotrichosis, and I suspect it to be an iodide eruption.

The PRESIDENT: I feel very strongly that this is a halogen eruption. My immediate diagnosis was bromide eruption. If he has been taking iodide since Dr. Bunch has seen him he will have iodide in the urine. I suggest he be taken into hospital and treated with nothing but lotio calaminæ, and no drugs. An artificial eruption may be induced with bromide or iodide.

Dr. F. PARKES WEBER: During the war the general knowledge of the effects of drugs in producing skin lesions and in otherwise malingering was greatly increased. I heard of two French prisoners of war having taken enormous doses of urotropin in Germany, and their recurring attacks of hæmaturia so impressed the authorities that they were during the war allowed to return (were repatriated) to France as suffering from incurable disease (? renal tuberculosis). Potassium iodide with its occasional effects on the skin is likely to be much better known than urotropin. The skin lesions in the present case would certainly suggest the rare "granulomatous" form of iodide or bromide eruption, which does not disappear for some weeks after the drug has been discontinued.

Postscript.—(July 23, 1920.) This patient has been cut off all halogens for some weeks without any improvement. Previously to his coming under my care he absolutely denied taking any drug internally.

Case of Acanthosis Nigricans.

By HENRY MACCORMAC, C.B.E., M.D.

THE patient is a woman, aged 62. She states she first observed a darkening of the skin of the neck about six months ago, and that this has spread gradually downwards. During the last few weeks she has become aware of some degree of weakness, but otherwise she appears in fairly good health. No internal carcinoma has been detected. The urine is free from albumin and sugar, there is no vaginal discharge, or pain or discomfort after food. The blood-pressure equals 135 mm.

On examination the eruption is found on the back and sides of the neck, under the breasts, in the flexures of the groins, arms and legs, and between the buttocks. It is also present about the umbilicus and over

the small of the back, and the hands show the characteristic roughened appearance. In the situations where most developed the process consists of a sooty black discoloration with exaggeration of the normal folds of the skin, resulting in a dark, lined, and slightly raised condition, but without any marked degree of infiltration. In a few situations small warty growths are seen, especially in the axillæ and about the buttocks; this is, nevertheless, not a marked feature of the case. On the inner sides of the thighs there are fibromata, which it is stated have existed for many years, and do not appear to have any direct relationship to the disease under consideration.

A small piece of skin was removed from the neck for microscopic examination, but the sections do not show any appearances which might not be expected from observation of the involved skin: the characteristic features consist in a general increase of the Malpighian layer, with an excessive deposit of pigment both in its basal layer and also in the cells of the dermis. The horny layer is exaggerated and collected into large masses in those places where the folding of the skin is found.

DISCUSSION.

Dr. HALDIN DAVIS: It may not be generally known that this condition was originally described by Dr. Addison in the course of the observations which finally led to the recognition of the syndrome now universally known as Addison's disease, long before it was differentiated as acanthosis nigricans. I found in Addison's original publication on pigmentation associated with disease of the suprarenal bodies, a picture of a patient clearly suffering from acanthosis nigricans. There was also a description of the post-mortem examination of the same patient in which the abdominal carcinoma usually associated with the condition is fully described. Addison did not, however, distinguish between acanthosis and the disease which now bears his name.

Dr. F. PARKES WEBER: In some people there is a warty nutmeg-grater-like, dark, thickening of parts of the skin, notably about the ankles during cold seasons, which disappears with the onset of warmer conditions. I regard such cases as very *minor varieties of acanthosis nigricans*, and suspect that they are connected with some form of chronic intestinal toxæmia. In the present *well-marked case* of acanthosis nigricans it would, therefore, be interesting to know whether the condition is more marked in very cold or in very hot weather.

Dr. GRAHAM LITTLE: This patient exhibits a symptom on which Dr. MacCormac has been silent, but which I regard as of essential diagnostic import, namely, the condition of the palms of the hands. These are notably thickened and have a roughened and hard surface rather like shark-skin. The

same symptom was noted in a case which I saw many years ago, while acting for Dr. Pringle and which I reported in the *British Journal of Dermatology*. The patient subsequently died in the country, where he was a farm labourer, and his doctor wrote to me that the symptoms preceding death pointed strongly to cancer of the rectum.

Dr. BARBER: One point about the general appearance of this patient is her beard. I saw at Guy's Hospital a woman with a beard, whose general condition was not recognized. When she died she was found to have malignant ovaries, and the same may be present in the woman just shown.

The PRESIDENT: I have been through all the older literature, and, to the best of my recollection, there have been some cases of this condition in which post-mortem examination revealed no malignant disease.

Case of Xanthoma Tuberosum.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a female infant aged 3 months. The eruption was noted at birth, and there have been no new lesions since then. The eruption consists entirely of very hard reddish-brown tumours about $\frac{3}{16}$ in. in diameter, (and about $\frac{1}{8}$ in. above the general skin level) scattered irregularly about the scalp, the face, the upper part of the chest, and less numerously, on the forearms. There are well over fifty such swellings. They give rise to no discomfort, and the child is otherwise in excellent health.

One of the tumours was excised and examined microscopically. The section shows a uniform infiltration of the whole corium right up to the level of the epidermis with a very homogeneous type of round connective tissue cell. The cells seem to be arranged in rows rather like the arrangement of cells of pigmented nævi, and the tumour is undoubtedly of nævoid character. The appearances of the section in fact reproduce very faithfully the appearance of the sections of a case described by McDonagh in a paper in the *British Journal of Dermatology* for 1912, in which other cases in the literature are examined, and the name nævo-xantho-endothelioma is proposed for this group. The cases are certainly very rare; they are obviously scarcely to be distinguished from urticaria pigmentosa except by histological tests.

Mr. McDonagh: These cases are very rare, as this is the only case I have seen since I published my paper on the subject in 1912.[1] As a result of investigations I came to the conclusion that the lesions were congenital, often familial, and were of the nature of nævo-xantho-endotheliomata. Clinically, the lesions begin as vascular nævi, then become yellow and flat on the surface, entirely disappearing in time. Histologically the primary lesion is a vascular endothelioma, later the endothelial cells swell and undergo fatty or xanthomatous changes and later still new fibrous tissue and blood-vessels take the place of the degenerated endothelial cells. This class of case should not be confounded with inflammatory xanthomata, which are multiple, loculated lesions and yellow from the start. The clinical picture and microscopic section shown by Dr. Graham Little to-day are replicas of the case and sections I exhibited before the Society in June, 1909.[2]

Case of Onychogryphosis.

By E. G. Graham Little, M.D.

The patient is a boy aged 6, in whom the nails of the right and left thumbs are converted into claw-like appendages raised from the matrix, curved with the convexity outward, and about ½ in. long. On the left medius there is an earlier phase of the same condition, the nail being thickened, separated from the matrix but not yet with the curious curvature of the thumb nails. The left hallux is similarly affected.

The disease dates from twelve months ago. Scrapings from the thickened area under the nail were examined for fungus, so far with a negative result. The child lives in a mews, a circumstance which suggests the probability of ringworm infection.

Epidermolysis Bullosa.

By Haldin Davis, F.R.C.S.

This child aged 3, has been in hospital three weeks, and is consequently a good deal better than he was when first seen. The condition was noticed at or immediately after birth: the parents believe it was present at birth, and there has been steady progress of the illness since. The nails are very badly formed, and some of them are not formed at all.

[1] *Brit. Journ. of Derm.*, 1912, xxiv, p. 85.

[2] *Proc. Roy. Soc. Med.*, 1909, ii (Sect. Derm.), p. 142.

The teeth, also, are very carious and generally unsatisfactory. There has been some improvement since he has been protected in hospital, but even while there some fresh bullæ have come out, stained with hæmoglobin. The patient does not show any Nikolski's sign. This is the only case in the family.

Pityriasis Lichenoides Chronica.

By HALDIN DAVIS, F.R.C.S.

THE patient, a woman, aged 33, says that her skin has always been somewhat rough, particularly on the upper arms, but that the present eruption has appeared only during the last two years, and has been steadily progressive. It consists of a number of very small reddish papules, usually surmounted by a scale, which can be detached by the nail, and when this is done a slightly oozing surface remains. The papules are distributed almost universally: on the trunk, arms, legs, and to some extent on the backs of the hands and the dorsa of the feet. Some few have made their appearance on the face. There are none on the scalp, and the hair and nails are unaffected. In some places the eruption resembles psoriasis with very small lesions, in others, notably at the back of the neck, it is more like lichen planus, but nowhere is the colour like that of lichen planus. The Wassermann reaction has been tested and is negative. I have ventured to make the diagnosis of pityriasis lichenoides chronica. It greatly resembles a case which I saw some time ago in an American who had been seen by Dr. Wise and other American dermatologists, and the same diagnosis was made by them.

DISCUSSION.

The PRESIDENT: I agree with the diagnosis. I think it is of the pityriasis lichenoides chronica type which Brocq put into his group of parapsoriasis.

Dr. HANNAY: I have recently had the opportunity of following Dr. Brocq's clinic in Paris, where some of these cases were demonstrated. When I first saw this eruption I had no hesitation in putting it into the group of parapsoriasis as described by Brocq and so fully discussed by Civatte in his thesis.

Three Cases of Syphilis with Negative Wassermann Reaction.

By Wilfrid Fox, M.D.

Case I.—A man, aged 26 (No. 1,249 on the Register), came to St. George's Hospital in August, 1919, with a primary sore on the penis. The treponema was found by Dr. Hunt on August 14. Combined treatment was started at once and six injections of novarsenobillon, dosage, 0·6 grm., and six injections of mercury were given weekly. The course ended at the beginning of October and on October 15 the Wassermann reaction was negative. On October 29 the patient returned to hospital showing a deep ulceration of the tongue and right tonsil, and a papulo-squamous rash on the face. A second course of combined treatment similar to the first was given from the end of November to the beginning of January. On January 14 the Wassermann reaction was negative. On January 16 ulceration of the scalp and right temple accompanied by severe basilar headache was noted. The previous throat lesion and tongue ulceration had healed up. Since January he has been treated with large doses of iodides, collosol sulphur and intramine, without any marked improvement. The ulcerations are now, however, slowly healing under intravenous injections of cyanide of mercury. The Wassermann reaction which was taken on May 19, that is to say, the day before the cases were shown, proved positive.

Case II.—Male, aged 39 (No. 1,235 on the Register). He came to the hospital on August 8, 1919. He had been previously treated in the Army from January to March, 1919, with eight injections of novarsenobillon and eight of mercury. He was given one course of injections during August and September, six of N.A.B. and six of mercury at weekly intervals. On October 8 the Wassermann was negative. On February 14, 1920, it was also negative and again on April 20. Fourteen days later, however, he came back to the hospital with well-marked tertiary ulcerations on the penis.

Case III.—Female, aged 52 (No. 1,098 on the Register). This patient was infected twenty years ago and came to the hospital on June 13, 1919, suffering from deep punched-out ulcerations of the face, around the nose and eyes. They had been present for the last nine years. On June 16 the Wassermann reaction was negative. Combined treatment of novarsenobillon and mercury, six injections

at weekly intervals, were given during June and July. The patient however did not stand the treatment well and the dosage had to be cut down to 0·3 grm. of N.A.B. On July 31 the blood reaction was weakly positive, thereby demonstrating a latent Wassermann reaction. There was no improvement in the lesions worth mentioning. The treatment was continued during September and October in such degree as the patient could stand it. On November 7 the Wassermann reaction was negative and again on January 23. Since that date iodide, sulphur, and tonics have been employed; the lesions, however, still persist.

DISCUSSION.

Dr. MACCORMAC: I have recently been repeating the Wassermann reaction at frequent intervals in cases of syphilis under treatment, and my findings are in agreement with those of Dr. Fox. It is of course well known that treatment has but little or no effect upon the reaction in late cases, that is those of the "tertiary" group, although the symptoms may, and often do, clear up under treatment. It is however less generally recognized that active early—i.e., secondary, syphilis may exist with a completely negative blood test. Such cases are rare, but they do occur, and where too much reliance is placed upon laboratory methods, without sufficient consideration of clinical evidence, the observer may be misled.

Mr. HALDIN DAVIS: I ask those who see more syphilis than I do if the cases of syphilis which exhibit clinical relapses with a negative Wassermann reaction are not more resistant to treatment than are those which relapse and exhibit a positive Wassermann?

The PRESIDENT: In the last 800 odd cases in our venereal clinic we have had a single case of this type—it is the only one of active syphilis which gave a negative Wassermann. I exclude tabes from these. The patient said he was treated in the Army by the method of rapid injection of salvarsan and mercury, and he relapsed very quickly. When he came to us, he had a negative Wassermann, with a ringed papular eruption which certainly was syphilis, but we have been unable to remove it. We have given him full doses of N.A.B. and full mercury and iodide treatment. I have wondered whether in these cases it is a different organism, or a special form of spirochæte. In Dr. Fox's class of cases not only do they relapse quickly, but treatment does not do much good. Is it possible that some other organism is inoculated in symbiosis at the same time?

Dr. FOX (in reply): In the first case we found the treponema in the primary sore. I agree that they are difficult cases to treat. We have found that sulphur does good after a long course of the usual dual treatment. Also that the intravenous injection of the cyanide of mercury is better than the intramuscular.

Section of Dermatology.

President—Dr. Arthur Whitfield.

Two Cases of Dyschromia of the Face.[1]

By E. G. Graham Little, M.D.

Case I.—The first is that of a lady, aged 29, the wife of a medical man. She has lived for the past twenty-one years in Devonshire, having been born in India where she passed the first seven years of her life. The pigmentation began four years ago, and was associated with no change of habits that could account for it. The colour, which has been rather deeper in summer, receded somewhat in winter. She was pregnant last year, with no change in the pigmentation during pregnancy nor after it. There is no greying of the hair, and no vitiligo, nor abnormality of pigmentation elsewhere, except that the nipples since pregnancy are a little deeper in colour than before. The pigment is distributed very irregularly, producing a mottled light-brown coloration of the forehead, parts of the cheeks, notably along the upper lip (without any hirsuties), and slightly on the chin. The mucosa is unaffected. She is otherwise in excellent health, is fond of outdoor sports, and a good exponent of several.

Case II.—The second case is that of a young man, aged 22, demobilized from the Army in January, 1919, when he went into his present business, that of tyre-moulding. This work entails his handling solutions of calcium chloride, and he says that other men are affected in the factory, but Dr. Parkes Weber has been good enough to seek the opinion of Dr. Legge, of the Home Office, who states that calcium chloride workers are not the subjects of pigmentation so far as his experience goes. The patient noted the discoloration only four months ago and is positive that the whole condition is not of longer duration. He is not unduly exposed to the sun nor to any artificial light. The

[1] At a meeting of the Section, held June 17, 1920.

pigmentation is fairly regularly distributed over both cheeks, is of a walnut brown colour, with minute white areas, about $\frac{1}{8}$ in. in diameter, interspersed in the dark patches. The skin of the orbits is of a slightly darker tint, but just below the eye there is a free area, the upper moustache area is especially dark, the forehead is free and the mucosa unaffected. There is no abnormality of pigmentation elsewhere, and the man is otherwise perfectly healthy.

I am unable to offer any explanation of the causation in either of these very similar cases, and shall be glad of any suggestions.

Case of Trichotillomania.

By E. G. Graham Little, M.D.

The patient is a little girl, aged now $4\frac{1}{2}$, the daughter of gentlefolk living in Surrey. There are two other children, aged 8 and 5, who are perfectly healthy and have shown no similar symptoms. There is no history of epilepsy, or night terrors, and the child is quite a merry little person, with no vicious habits. I saw her for the first time in August, 1919. At that date the scalp over the right parietal area was nearly wholly bald, very much like an X-ray alopecia. The hair follicles were intact, but the hair had been extracted from its root. The child could be observed to pull the hair herself, at all hours of the day, but not in her sleep or at night. She sleeps excellently, on an average twelve hours at night and two during the day. More recently she seems to have contracted another habit—namely, that of breaking the nail and sucking the fingers. I could detect no local irritation nor change in the scalp in the side affected, this being in all probability determined by proximity to the right hand. The habit seems to have been formed as early as the age of 2, and the hair grows quite normally after plucking, and is again attacked. I suggested the wearing of a muslin skull-cap, but probably this has been difficult to ensure. I have never seen nor read of a case developing at such an early age.

The President: Some years ago I had a patient in King's College Hospital whom we treated by a simple method: we had the hair mown down very short by the apparatus the hairdressers use, and we kept the hair to about $\frac{1}{8}$ in. for about two years, and during that time the habit was forgotten, and a cure resulted. I think this case can be cured in the same way in regard to the scalp. The nails constitute a different proposition.

Case of Epidermolysis Bullosa.

By E. G. Graham Little, M.D.

The patient is a Jewish boy, aged 5, a native of the Transvaal, where he is under the care of my old pupil, Dr. Batchelor, who sent him to me. The condition is congenital, and a previous child was born with the same condition and died, aged fifteen days. The parents are healthy, and there is no hereditary history. The nails of the hands and feet are entirely absent and are stated never to have been formed. The fingers are tapering and slightly atrophic at the tips. There is a patch of congenital dark pigmentation covering the outer border of the hand on its dorsal surface and the greater part of the fifth finger. There are numerous large and small bullæ, many of them hæmorrhagic, on the hands, the forearms, the face and trunk; and bullæ form with extreme frequency and rapidity in sites of pressure. The teeth decayed very early and were all extracted while he was in the Transvaal. The conjunctivæ are normal, there is no shrinkage, but the mucosa of the tongue is distinctly thickened. Speech is retarded, the child being very backward in this respect but otherwise apparently of fair intelligence. A blood count taken in London shows no abnormal quality.

Dr. Krupenia: The first child survived a fortnight, and the father is healthy, except that he suffers from some ulceration on both legs. The mother has had one abortion, otherwise she is healthy. I could not learn of anything or see anything which would suggest specific trouble. The child was accidentally wounded by a bullet in the head: the bullet was extracted, and apparently the wound healed up well.

Case of Recklinghausen's Disease with Acromegaly.[1]

By A. W. Ormond, F.R.C.S.

This is a case of Recklinghausen's disease in a patient with acromegaly. The main interest consists in the association of neurofibromatosis with the patient's obvious pituitary symptoms. His hands and feet are very large, and I understand that all his life he has been physically

[1] This case was seen at a meeting of the Section of Ophthalmology (see *Proceedings*, xiii, Sect. Ophth.), p. 74.

over-developed. He came to Guy's Hospital originally complaining of headache and failure of sight. The pigmentary tumours do not seem to have attracted much attention: it was for the failure of sight he sought advice. He has a partial optic atrophy in both eyes, following neuritis. He has had one or two fits of vague kind, and the interval between the fits has been a long one, only three or four such fits in several years. He gets attacks of headache and sickness, but not of a very severe type. These symptoms, coupled with the optic atrophy secondary to papillœdema, suggests that there is probably some increased intracranial tension.

There is one other small detail which it may be of interest to mention, and that is, that if you examine the optic discs you will find that he has, especially on the right side, a small pale area continuous with the margin of the disc, extending upwards, evidently underneath the retinal vessels. I suggest, though I cannot prove it, that it is a small patch of fibroplasia, similar to those present elsewhere in the skin. When I showed this patient to my ophthalmological colleagues recently[1] some agreed that might be so; others thought it might be due to exudation from the papillœdema which had become organized. I am not impressed with that opinion, because the papill-œdema is very slight, and I do not think he has had an amount of exudation which would cause such a patch.

The case might be explained on these lines, that since early life he has had fibroplasia, which would account for the tumours. And if we imagine that this increased fibrous tissue condition has involved the optic nerves and chiasma, and also the pituitary body, we might imagine that the resulting stimulation of the pituitary body would cause his symptoms of acromegaly. After many years probably that condition has passed away and has been replaced by a state of hypopituitarism. In different cases all the cranial nerves have been involved in this condition. Alexis Thomson records four cases of optic nerve involvement in neurofibromatosis. But the optic nerve is not a peripheral nerve, and therefore, according to the accounts of the condition, should not be implicated. On the other hand, there are many nerves around it, and it is possible that the condition may be due not to the optic nerve itself, but to the fact that it is involved owing to its proximity to other nerves. But Mr. Treacher Collins has described cases in which some nerves have been involved in a general hyperplasia rather than a fibroma. I think all the man's symptoms can be explained on the hypothesis I have advanced.

[1] See *Proceedings*, 1920, xiii (Sect. Ophthal.), p. 74.

DISCUSSION.

Dr. BARBER: Mr. Ormond sent this patient to me on account of the skin condition, and I asked him to come and demonstrate the patient to the Section. The case is not unique, others of the kind have been published, one by Nicolas and Favre. I do not think any post-mortem findings in such cases are recorded.

Dr. F. R. B. ATKINSON: The combination of the two conditions is very rare. Of 768 cases of acromegaly of which records are obtainable, only four were combined with von Recklinghausen's disease. There are no records of post-mortem examinations of such cases. The four cases were all in males.

Dr. F. PARKES WEBER: The patches of cutaneous pigmentation in this case appear to be congenital pigment-nævi. I think that is often the case in neurofibromatosis. Has anyone present made observations on the subject? Because it is still generally supposed that the pigmentation of von Recklinghausen's disease is mostly not congenital. As far as this man knows, his patches are all congenital. One of his tumours feels like a lipoma, but it is probably a very "flabby" neurofibroma. Are any spinal or cranial nerves involved in this case, so as to form thickenings or plexiform neuromata, which can be made out by palpation?

The PRESIDENT: I cannot say anything as to frequency, but one of my cases distinctly developed new pigment patches while he was under observation. He was between 20 and 30 years of age at the time.

Mr. A. W. ORMOND (in reply): There are no other nerves involved in this case. I forgot to mention the mental condition. This man cannot read or write, or do anything in the way of arithmetic: but such disability is often associated with the Recklinghausen symptoms. Many of the cases have not been recognized until the post-mortem examination.

Case of Pityriasis Rubra Pilaris.

By H. W. BARBER, M.B.

THIS child, aged 4, was brought to me about a month ago. The history is that the eruption began on the head four months ago, as scaly patches. At that time there was no eruption on the face or body. Two months later, the face, trunk and limbs became involved rather rapidly. The mother says the rash came out in a few days. He is the only child of his parents, and his general health has been always good, except for an attack of chicken-pox. He was a breast-fed child, and one peculiarity about him is his enormous appetite; all his conversation is

concerned with food, and he would eat all day long if he were allowed. When he first came to the out-patient department the eruption was not so extensive as it is now, and it was much more like psoriasis than pityriasis rubra pilaris, though even then there were some points which suggested the latter diagnosis. On the other hand, he had some papules which were very like those of lichen planus. A fortnight later there was no doubt that it was pityriasis rubra pilaris. The lesions on the trunk were then much more follicular than they are now. On the face and trunk he has now a more or less generalized erythrodermia, but I think the lesions on the hands and feet are typical of pityriasis rubra pilaris. The urine shows a large excess of indican, and gives a marked phenol reaction, which is very rare in children of this age. Possibly it has nothing to do with the ætiology of the condition, and may be due merely to his enormous appetite. I have had his fæces cultivated, but only the *Bacillus coli communis* and a non-lactic acid fermenting organism were obtained.

So-called "Multiple Hæmorrhagic Sarcoma" (Kaposi).

By W. Knowsley Sibley, M.D.

H. W. M., a fairly healthy well preserved man, aged 72, an engineer by occupation, was born at Plumstead of English parents, and he has never been out of this country. His father died at the age of 83 from stricture complications, and his mother at the age of 60, cause unknown. He has three brothers living and well; one brother died at the age of 28 from congestion of the lungs. He had six children by his first wife, who died from ulcerated stomach, and nine by his second who died from cancer of the stomach; she also had four miscarriages. There is no history of cancer in his family. He states he has never had gout or rheumatism, but he had an attack of sciatica twenty years ago, and was not laid up with it. He had dropsy in his legs and feet ten months ago, but no other illnesses.

About eight years ago he states that he slightly cut the back of his right hand in dealing a blow at his son's face, striking a tooth. A small red spot appeared immediately after, and this has very gradually increased in size ever since. Some eighteen months later a pigmented lesion appeared on the back of his left hand, afterwards the feet and ankles became affected and the condition gradually spread upwards,

the face, head and neck being the last to show any tumours. He attributes the condition of his feet and ankles to the attack of dropsy he had recently.

There is some enlargement of the glands in the left groin and to a lesser extent in the right. The other glands are not appreciably enlarged. He presents a large number of tumours scattered over various parts of his body. The largest is a slightly raised mass of deeply



Fig. 1.

pigmented tissue over rather more than half the dorsal surface of the right hand, and extending to some extent between the first and second fingers, but not on to the palm (fig. 1). The outline is well defined with a raised margin. The central part is smoother and not so raised. There are three lesions in the dorsum of the left hand, but these are not raised above the level of the rest of the skin, and appeared more or less as pigmentary infiltrations. Extensive areas of general infiltration are present around both insteps, where there are numbers of warty

tuberous growths, which extend round on to the soles of the feet, and numerous more or less scattered small tumours extend up the legs, giving them somewhat an elephantiasis-like appearance (fig. 2). Several deeply pigmented tumours are present in the right groin, and a few are scattered over the sacral region and also over the left scapular region. Chains of growths are also present on the sides of the neck and affecting



FIG. 2.

the ears, and some are on the face, on the eyebrows and nose. The arms and forearms are also affected.

The isolated tumours vary from about the size of a pea to some three or more centimetres in diameter. Perhaps the most prominent one is on the right arm. They are all deeply pigmented, and many are of a purplish colour. Most are nodulated and some have the

appearance of pigmented nævoid tumours. There is some pigmentary infiltration of the hard palate on the right side and considerable pyorrhœa is present.

Below the level of the knees the lesions are but slightly raised

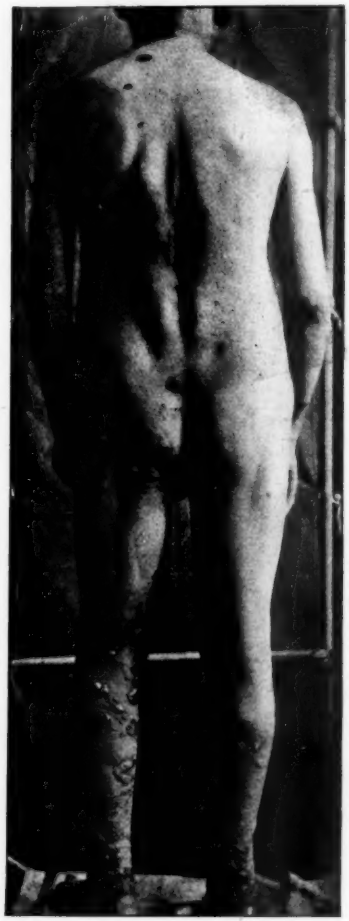


FIG. 3.

above the skin and tend to fuse one into another, presenting a more or less diffuse pigmentary infiltration of the tissues. Above the knees, on the other hand, they exhibit distinct isolated tumours, generally raised above the skin level. About 120 of these can be counted.

The liver is enlarged and extends some two or three fingers' breadth below the costal arch. The spleen cannot be felt, nor is its area increased. The cardiac sounds are normal.

I gave him an alkaline arsenical mixture, containing 4 minims of Fowler's solution, three times a day, the dose of which has now been doubled, and his condition appears to be rapidly improving, especially the appearance of the feet and ankles. He states that some of the tumours are disappearing, especially one or two on his right arm, and the lesion on the dorsum of the right hand is obviously receding. I applied diathermy fulguration to one of the tumours on his left shoulder some fourteen days ago and a half pastille dose of X-rays to three others ten days ago.

Pathologist's Report.—(1) Section of pigmented nodule removed from back: The layers of the epidermis are slightly atrophic. The dermis is composed of masses of spindle-shaped and round cells, which appear to be separated by a fibrous connective tissue; no mitosis seen. There is a marked vascular dilatation and a deposition of blood-pigment apart from the vessels themselves. The growth appears to be of connective tissue origin, associated with pigmentation, with no true sarcomatous cells present. (2) Examination of urine: Result normal. (3) Wassermann reaction was negative. (4) Blood count: Red blood cells, 5,200,000 per cubic millimetre; white blood cells, 6,100 per cubic millimetre; polymorphonuclear cells, 72 per cent.; mononuclear cells, 3·5 per cent.; lymphocytes, 19 per cent.; eosinophils, 4 per cent.; transitional cells, 1 per cent.; basophils, 0·5 per cent.

Dr. McLeod, reporting on the histology of a case he examined a few years ago, said: "Histologically it is not a sarcoma but a growth of organizing connective tissue cells, associated with marked vascular dilatation œdema and the deposit of blood pigment, more nearly allied to fungoides than to true sarcoma."

I would suggest that the name "multiple hæmorrhagic granulo-fibroma" would be more appropriate than that of sarcoma, which has previously been attached to these cases by de Amicis and Kaposi.

Dr. F. PARKES WEBER: The present is almost the only case I myself have seen in which the mouth has been involved. The tongue and lips were involved in Dr. Paulin Martin's case.[1] Martin's patient was a man who had resided all his life in the Vineyard, Abingdon; he died there at about the age

[1] Martin, *Brit. Med. Journ.*, 1904, ii, p. 999. By the kindness of Dr. Martin I was able to add some later details, in a footnote, *Brit. Journ. Derm.*, 1916, xxviii, p. 315.

of 77. The mucous membrane of the glans penis was affected in the case of a man whom I showed before the old Dermatological Society of London, on February 8, 1905.[1]

Case of Extensive Generalized Cicatricial Secondary Syphilide.

By GEORGE PERNET, M.D.

THE patient is a young woman, aged 22, employed at a factory. She was first seen by me on June 1, 1920, at the West London Hospital, when she presented numerous recent circular and oval scars scattered about the back, limbs, face and scalp. There were very few lesions on the front of the trunk. Her throat, palate and tongue were very ulcerated. There was post-sternomastoid and inguinal adenitis. There was a small ulcerated lesion on the inner surface of the left labium minus. She also exhibited the characteristic lesions of scabies. This has disappeared under treatment. The secondary rash must have started some seven months before I saw her, as she stated she had been under a doctor since November 22, 1919; first for a sore throat and then for the rash. She had had a mixture and pills from him. The rash had no doubt been a severe ulcerating and scattered one. The treatment she has had from me has consisted of two intravenous injections of novarsenobillon and same mercury, and the condition of the throat and tongue has practically cleared up. Such a widespread secondary cicatricial condition is rarely seen.

Two Cases of Bilateral (more or less Symmetrical) Morphœa in Children.

By GEORGE PERNET, M.D.

THE two female children are shown together, but they are not related in any way.

Case I.—A girl, aged 14, came to me for scabies. The two morphœa patches were discovered in the course of examination. They are transversely oval and situated practically symmetrically over the angles of

[1] Weber and Daser, *Brit. Journ. Derm.*, 1905, xvii, p. 135.

the scapulæ. They are very superficially sclerosed, and have diminished under massage, the only treatment employed. The patient has a poor circulation and suffers from chilblains in the cold weather.

Case II.—This girl is aged 5. The patches were noticed some three weeks before I saw her in November, 1919. They were very faintly marked, but exhibited a slight amount of lilac bordering. They are transversely oval and situated below the level of the angles of the scapulæ, the right one being a little to the right as compared with the left. They are not therefore so symmetrical as in Case I. The mother volunteered a history of a fall on one side, then on the other, on affected areas, followed by some bruising, two months before the patches were noticed. When I first saw the child both areas were tender to touch. They have greatly improved under massage, the only treatment.

Case of Granuloma.

By Henry MacCormac, C.B.E., M.D.

The patient is a woman aged 35. She states that ten months ago she had pleurisy and, about the same time, a nervous breakdown. I have carefully examined the lungs, and at present no evidence of disease can be detected by ordinary methods. Five years ago, a diffuse reddish-purple infiltration was observed on the left ankle, assuming the present dimensions in a few months. At present this infiltration forms a species of anklet extending completely around the limb, in which are areas of apparent softening. No ulceration has however in fact occurred. This lesion seems to be related to the other types of eruption, but on this point it is impossible to speak with confidence. The more typical granuloma first commenced beneath the right breast, in the form of a nodule the size of a pea. Gradual increase has taken place until the present condition became developed, a dumb-bell shaped area the middle part of which occupies the inframammary region. Next, a similar lesion was noted at the angle of the right scapula, ten months ago; later, others formed in this neighbourhood. All these show the same appearances, an irregular papillated surface, reddened, and discharging pus. Within the last six months two small nodules have made their appearance in the region of the upper part of the right calf. The Wassermann reaction is negative, the cultures made on Sabouraud's

media have so far produced only the common cocci of secondary contamination. From the edge of the growth on the breast a piece of tissue was removed for microscopic examination. In this there are appearances suggestive of the presence of a mould fungus, groups of spores, and also long bodies having the appearance of a mycelium.

DISCUSSION.

Dr. H. G. ADAMSON: The lesions suggest to me actinomycosis. With regard to the supposed mycelium and spore in the epidermis, I think they are merely leucocytes passing through the epidermis. The leucocytes, in making their way through the epidermis become long thin threads. The appearance is common in all sections of inflammatory skin affections involving the epidermis. In any case it would be very unusual to find mycelium in the Malpighian layer.

The PRESIDENT: Dr. Adamson has made the remark on the section that I was about to make. I think, however, there are long, perfectly symmetrical pieces in the epidermis which are a little too parallel-sided and too distinctly truncated by the sectioning knife to be leucocytes. At first I thought they were all leucocytes, but I am not quite certain of this. The sections ought to be stained by Gram's method.

Dr. ADAMSON (in further comment): It would be remarkable if mycelia were growing between the prickle-cells, and they would hardly grow there without producing some reaction. In ringworm it only grows in the horny layers; in others it gets into the corium. I have not read of it occurring in the Malpighian layer.

Dr. MACCORMAC (in reply): I agree with Dr. Adamson that the appearances seen in the section are distinctly suggestive of leucocytes. When I examined the preparations with Dr. McIntosh this point was considered, and we came to the conclusion that what we saw were really parasites.

A Treatment for Lupus Vulgaris.

By H. G. ADAMSON, M.D.

THESE are examples of a method of treatment I have been using for lupus vulgaris for the past eight or nine months—namely, painting with liquid acid nitrate of mercury. I do not know that it has been largely used, though it is mentioned occasionally in text-books. In the literature I can only find mention of two cases so treated: one by Startin and one treated by Mr. Marmaduke Sheild, and recorded in the *British*

Journal of Dermatology. It is well known to be useful in the treatment of tertiary syphilitic lesions. I first used it because I had a case about which I was doubtful in regard to diagnosis, whether it was fungating tuberculosis or tertiary syphilide. It cleared up so completely after being painted with this preparation that I used it in other cases of lupus. Of late years I have been accustomed to treat lupus vulgaris with pyrogallic acid, and I have used brass ointment too, but the ointment I have given up, after a prolonged trial. This little girl had been under treatment four or five years, and she was far from cured. The lupus area was painted once with liquid acid nitrate of mercury, and some of the nodules were touched a second time, and you see the result. There is a smooth scar without any sign of lupus nodules. This young man had a patch of lupus of eleven months' duration, and of about the size of a shilling. The patch was painted once, and the scar here is the result. This application not only takes away all the lupus nodules, but also has a good cosmetic effect. I generally take fine forceps, twist wool round it, and paint over the patch, letting the liquid soak in. The patch becomes of a parchment colour; it is painful, but not intensely so. The patient comes back in a week, with the patch covered with a rather thick crust, and in another week the crust comes off and leaves a healthy scar. When a large area is involved, one treats a small patch at a time. In this way I have treated bad cases, which have been attending the hospital for years, some of them fungating, and they improve at once. I have not been using it long enough to obtain a complete cure in one of these extensive cases, but the way the swelling disappears and the whole thing quietens down is remarkable. I have even applied it inside the nose. A full account of this method of treatment will shortly be published in the *British Medical Journal*.

DISCUSSION.

The PRESIDENT: I take considerable interest in this method. I have used acid nitrate of mercury—but apparently in the wrong way—for many years, and I gave it up. I have used it after scraping operations, and for stopping bleeding. I thought it was inferior to zinc chloride. I commonly knock out a nodule or two with a match by means of this substance, but more as a means of education to students than anything else. But I have not seen such good results as these, and I shall adopt this treatment. I suppose that owing to its being much less exacting than scraping, one could repeat it indefinitely if long enough intervals are given in between.

Dr. GRAHAM LITTLE: This is one of the most interesting exhibits which have been before us for some time. Has Dr. Adamson had to repeat the painting?

Dr. O'DONOVAN: The great importance of this comparatively simple method is, that it will save people up and down the country from being grossly over X-rayed, with the terrible results we sometimes see. The question of the development or non-development of ectropion in cases of lupus of the face is very important; it is a common result of scarring. Finsen light gives beautiful cosmetic results, and does not conduce to the formation of keloid. Was a little ectropion present in this case before Dr. Adamson treated it, or does Dr. Adamson expect it to develop further than its present slight extent?

Dr. ADAMSON (in reply): One of these patients had the application once, the other three times. This patient had a lacrymal abscess, and her canaliculus had been slit up before she came under my care.

Two Cases of Oil Acne.

By AGNES SAVILL, M.D.

I SHOW these two patients together because they illustrate the same condition. The baby is 19 months old, and camphorated oil has been frequently rubbed on the chest, off and on, for so-called bronchitis since the third week of life. For whooping-cough in March last she had an embrocation, which was oily but different from camphorated oil. The mother noticed some blackheads coming up early in May, and numerous inflammatory pustules developed after the embrocation was stopped.

I have brought this man for exhibition so that I may get some indication as to what one should say to his employers. His forearms are covered with blackheads and pustules. He works in a metal firm, and uses much oil. The oil has no smell before use, but after use it smells strongly of tar. The point is as to whether he can protect his arms sufficiently to continue the work. Of the four workers in his shop, only one is immune from this eruption. The foreman has it over his legs, as the oil soaks through his trousers. This man has been working at the same job for eight months, since his discharge from the Army, yet the eruption only dates from three weeks back.

DISCUSSION.

Dr. O'DONOVAN: There are many thousands of men in engineering works constantly exposed to oils, and every little while outbreaks of such eruptions occur, and the doctor is sent for. Experience shows that he examines the pus, finds staphylococci, and recommends the putting of antiseptic into the oils: most

lubricant firms specially advertise oil loaded with antiseptic. Such advice however misses the outstanding point about these works, that they are very dirty, and there are but meagre means of men keeping themselves clean; there are scores of men receiving high wages and crowding round one pail of water to wash in, and often dependent on one ill-kept water-closet. The question of tar in this oil is not important; from the samples shown it appears to have been added after its receipt from the manufacturers. Different eruptions occur according to the specific gravity or chemical composition of the oil in use, and a most helpful summary of present knowledge will be found in a special memorandum written by Dr. Bridge, of the Home Office, and issued by the Committee of Scientific Research.

The PRESIDENT: I do not think it would be difficult to protect this man. When T.N.T. poisoning was prevalent, Dr. Benjamin Moore consulted me, and we devised a varnish, which he said was very useful. I think you could keep this man free from the oil-acne by coating him with a water-soluble varnish before he went into the works, and not letting him wash it off until he had finished work. In the case of babies, if once oil-acne has been set up, even after a year the skin will still be susceptible.

Dr. O'DONOVAN (in further comment): Water varnish was tried on workers in poisonous explosives a good deal during the war. It was found that if you put the wet varnish on people, and they had to wait a few minutes for it to dry, especially in cold weather, this induced them to give it up. Moreover, working conditions are such that varnish on the hands would soon be rubbed off. Sleeves and gloves are of little use: workers remove their gloves to scratch their heads, or for many similar trivial reasons; on re-assuming the glove the inside is contaminated and the worker now has every help in the way of warmth, moisture and close application to facilitate skin absorption. Manufacturers and owners would naturally prefer to supply gloves, since the true preventive measure, namely, free washing accommodation with hot water, soap, towels and maintenance is a very expensive measure; yet we, as a profession, should advocate cleanliness of working and cleanliness of person rather than ineffective palliatives.

Dr. J. C. BRIDGE (Home Office): I agree with Dr. O'Donovan that the most important thing in these cases is cleanliness, not only cleanliness of hands and arms, but also of the oil. I recommend that they change the oil frequently, not run it through the machines over and over again.

Dr. GRAHAM LITTLE: Many of the mischievous effects in these cases are due to the habits of the victims. A man who came under my notice was working with oil on a Thames barge, when the oil barrel burst and he was drenched in the oil. He remained in his soaked clothes for ten hours, and even then, instead of changing his clothes, he wore them for a few days. After that he had a severe, extensively suppurating follicular eruption, and a claim

for compensation was made. I was approached by the insurance people as to how far the result was due to the man's negligence. In addition it was ascertained that the man was at the time of the accident suffering from furuncle in his neck, and had been the subject of furunculosis for five years, off and on. I reported that this was a contributory factor, and that the man had been unduly negligent. The case was ultimately stopped. It is important to impress on the workers that ill-effects are largely their own fault, and that they could prevent all this trouble far more effectually by the practice of strict cleanliness than by the oil being rendered antiseptic or by the wearing of protective apparatus.

The PRESIDENT: I cannot entirely agree with Dr. Little's remark, as I have seen from twelve to twenty private cases of the kind in patients who were scrupulously clean, and in whom, even after massage, the oil-acne has developed, the shoulders and elbows being affected, regions over which ordinary acne does not appear. Therefore I insist that masseurs shall use French chalk, not oil. Once or twice I have had to use X-rays to get rid of the comedones.

Cases of Mycosis Fungoides.

By W. J. O'DONOVAN, M.D.

Case I.—A male aged 54, a cabdriver (formerly brewer's drayman). His parents died at ages 80, father, and 66, mother. His paternal grandparents and great-grandparents were, he says, centenarians. His wife died fifteen years ago of dropsy. No similar trouble in the family; he has usually enjoyed good health save for an attack of acute rheumatism over thirty years ago. Fourteen months ago he was seized with a feverish illness that lasted six weeks, and during that time he was in bed, very weak, shivering on "alternate days," and itching all over. Soon after he got up he had a shorter similar attack of a fortnight's duration, and after that lumps began to form as blotches all over his body. These lumps grew, broke open, discharged clear fluid and disappeared. Itching has been intense throughout, but he was able to drive his cab regularly until admitted for treatment as an in-patient under Dr. Sequeira at the London Hospital on June 15, 1920. The patient is a well covered, stoutly built, dark man with hair and beard turning grey. His nails are stiff, longitudinally furrowed and worn down by constant scratching. There is a wide distribution of tumours in the skin all over the face trunk and limbs, the largest being a lump 5 in. by 4 in.

over the left shoulder blade; many lumps are superficially ulcerated and show red, raw or scabbed surfaces. The nodules are closely and thickly set around the neck. The skin of the legs is red, hot to touch, and much excoriated. Heart, lungs and urine show no abnormality. Pupils and reflexes are natural. No adenitis; receding gums. Leucocyte count, 4,000 per cubic millimetre. Polynuclear neutrophils, 71·5 per cent.; polynuclear eosinophils, 11·5 per cent.; small lymphocytes, 6·5 per cent.; large lymphocytes, 7·5 per cent.; large hyaline cells, 3 per cent.; coarsely granular basophi cells, 0·5 per cent. Wassermann reaction positive (Clinical Laboratory, London Hospital, June 25, 1920). Microscopic examination confirmed the diagnosis. The pruritic onset of this case contrasts strongly with the erythrodermic onset of the following:—

Case II.—The patient is a nurse, aged 50, and single, who for four years has suffered with erythrodermia and was admitted under Dr. Sequeira very ill, raspberry red all over, scaling freely with medium sized flakes, and showing a few scattered nodules up to the size of walnuts on the face, abdomen and left forearm. Wassermann reaction negative. Microscopic examination, April 9, 1920, confirmed the diagnosis and there were no gross blood changes. No adenitis, no visceral enlargement, no fever, urine natural. To-day she is so well that she has come up to chaperon another case. The erythrodermia and exfoliation have gone and the only tumour remaining is a nodule the size of a cherry under the skin of the left forearm; itching, that troubled her a good deal, is now gone.

Treatment.—This great change is due to her whole skin having been exposed twice to half-pastille doses of X-rays, area by area being worked upon on successive days.

Case of Morphœa and Hyperthyroidism.

By W. J. O'Donovan, M.D.

The patient is a female, aged 27. Married three years; one child aged 2. Had scarlet fever when young. Since she can remember her friends have commented on her full neck. In December, 1915, she attended Dr. Sequeira's clinic for morphœa of two months' duration; she then showed ivory white patches below the left clavicle and between the shoulders, with lavender coloured margins. She then had tremor,

an easily elicited tâche, flushes and fits of irritability, polyuria, but no eye signs. To-day the morphœic patches below the left clavicle and between the shoulders have become replaced by normal skin. There are numerous rounded or irregular ivory areas with pink vascularized margins on the front of the chest, more especially on the right side, the largest occupying the suprasternal notch. There is a circular patch in each axilla, 5 cm. in diameter, which sweats freely. A good deal of itching is felt in the front of the chest at night. The patient complains of weakness and depression. There is gross tremor of the extended hands; flushes, sweats and palpitations are common; she is breathless on exertion; 54 oz. of urine are passed daily, no albumin, no sugar; respiration-rate in bed, 22 to 24, no pyrexia. Wassermann reaction, December 22, 1915, negative. The thyroid is moderately and symmetrically enlarged.

The PRESIDENT: Some authors say there is no sweating from a sclerodermic area, others say the sweating from those areas is excessive. This patient does still sweat in the axillæ where the patches are situated.

Section of Dermatology.

President—Dr. Arthur Whitfield.

Case for Diagnosis.[1]

By J. H. Sequeira, M.D., and W. H. Willcox, C.B., C.M.G., M.D.

Remarks by J. H. Sequeira, M.D.

This is a case of an army officer, 37 years of age, who, while on active service in Mesopotamia, contracted what was believed to be the "Baghdad sore." The condition appeared in November, 1918, and would not heal; remaining, in fact, an open sore for six months. Ultimately the sore was treated by operation and by antimony ointment, and under this treatment it finally healed up. I first saw the patient in February, 1920, when he had a number of discrete papules in the skin extending above and behind a very definite scar on the left malar eminence. They were of a reddish brown colour. The scar certainly resembled the scars I had seen in other cases of Baghdad sore, but I was not familiar with the grouping of these nodules, and therefore I asked Dr. Castellani to see him. Dr. Castellani thought it possible that the skin around the primary sore had been infected, and suggested that it would be a wise course to give intravenous injections of antimony. In spite of this treatment, however, the lesions have increased in number, and the trouble is now more widely spread than when I saw the patient first. I have had also the benefit of consultation with Dr. Willcox, by whose advice antimony was stopped and simple soothing treatment applied. The lesions, since I first saw the patient, have increased in number and the area involved has distinctly extended in a forward direction. I shall be glad to have the opinion of the members

[1] At a meeting of the Section, held July 15, 1920.

as to the nature of the condition and suggestions as to its further treatment.

REMARKS BY W. H. WILLCOX, C.B., C.M.G., M.D.

This patient was in Mesopotamia for a considerable time. Just before he left that country he was living in a mess where there were three other cases of Baghdad sore in a similar position. I saw two of the cases myself, and they were confirmed by bacteriological examination. There was no question as to the nature of the sores. The lesion on the left cheek did not develop while the patient was in Mesopotamia, but started while he was in India on his way home. He was seen by several medical officers, and the diagnosis was made, but apparently there was no bacteriological examination. The treatment adopted was that usually given for Baghdad sore and the lesion got completely well. Treatment was commenced in November, 1918, and the sore was completely healed by April, 1919. The condition remained all right until January of this year, when the edge of the scar began to thicken and the papules developed. My own view of the case is that the original sore, judging from the history and from the conditions under which the patient was living, and the very great prevalence of these lesions at that time, was an Oriental sore, but when I saw the lesion which developed later I formed the opinion that something quite different was being now met with. The subsequent lesion did not respond to treatment as Oriental sores did. I do not recollect a single case where there was a recurrence due to a proved Oriental sore lesion around an old sore which had healed for some months. It is well known that the Baghdad sore confers immunity, and the Arabs inoculate their children with Baghdad sore to prevent them from having disfiguring sores on the face. The heart and lungs of the patient are thoroughly sound, and there is no family history of tubercle so far as can be discovered.

DISCUSSION.

Dr. GRAHAM LITTLE: This patient was sent to me by my colleague, Dr. Willcox, in May last, when he showed much the same condition as now present. I gave my opinion then that the new efflorescence was a granulomatous infiltration almost certainly lupoid, and I am still convinced that this is the true nature of the disease now present, whatever may have been the original growth which was excised, concerning which we have no very authentic evidence. It was at my suggestion that the patient has kindly consented to come here to submit the diagnosis to the arbitrament of this Section.

Sir JAMES GALLOWAY: It is well known that arsenic is capable of producing curious eruptions, and perhaps the same effect may be produced by antimony. But in the present instance the lesion seems infiltrated and granulomatous in nature, and it is doubtful whether antimony would produce lesions of this kind. On the whole, if I were pressed to give an opinion, it would be that tuberculosis has been established by some means or other in the skin. I understand that a portion of the skin was removed in the early days when the patient had the Baghdad sore. It would be helpful if a piece of the skin were taken and examined by histological methods.

Dr. G. PERNET: The case appears to be one of lupus vulgaris. The affected area responds distinctly to the glass-pressure test; and the treatment he has had for the Baghdad sore, including curetting, may have modified the area before the lupus developed.

Dr. GRAY: I am inclined to agree with Dr. Little that this is a case of lupus vulgaris. It is certainly a granulomatous condition. I do not think the glass-pressure test is absolute in such cases. A case I brought before the Section some time ago, while almost certainly not tuberculous, gave the glass-pressure test to perfection.

The PRESIDENT: When I saw the patient from a couple of yards away I thought that it was undoubtedly a case of lupus, but on closer inspection I am not sure that it is tubercle of any kind. It is a granulomatous condition, and "if I were pressed"—to use Sir James Galloway's words, I would diagnose it as a curious case of lymphatic tracking of the original Baghdad boil. It would be extremely difficult to decide the diagnosis even if a portion of the skin were taken. I have had sections of Baghdad boil stained histologically which did not show the parasite, and they looked like lupus—not very characteristic lupus, it is true, but then, of course, lupus has a very variable histology. I suggest that two nodules be taken out, one inoculated into a guinea-pig, and the other cut into sections. I should be surprised if it turns out to be lupus. If it is a lupus it is not sclerotic at all. On the other hand, it is extraordinarily firm, and a lupus which is of the pure granulomatous type is of a soft and boggy consistency. I presume that the Wassermann test has been done and proved negative.—(Dr. WILLCOX: Yes.)—If it is neither tubercle nor syphilis it must be some weird, unknown condition.

Dr. WILLCOX: I took some scrapings from one of the papules, and these were examined microscopically by Captain S. R. Douglas. The result was quite negative as regards any Leishmania infection.

Dr. ADAMSON: I should have no hesitation in making the diagnosis of lupus vulgaris in this case at the present stage. I would suggest that a part of the lesion be scraped with a sharp spoon. If it is, as I believe, a case of lupus vulgaris, the lupus nodules would be found to be easily scraped away, which is not in my experience the case in "Delhi boil."

Case of Pemphigus Vegetans.

By E. G. Graham Little, M.D.

The patient is a clergyman of the Welsh Church, living in Wales all his life. He began to notice blisters in his mouth more than a twelvemonth ago, and the eruption seems to have been confined to that region for nearly a year, the whole mucosa of the cheeks and lips being finally affected, so that scabs covering a fetid discharge occupied the whole of the red surface of the lips. When I first saw him, in April, he had besides this condition of the mouth several thin-walled vesicles on the nape of the neck and on the forehead. As his means are small I admitted him into St. Mary's Hospital, where the condition rapidly developed, the vesicles being quickly transformed into ulcers, and numerous fresh lesions coming from time to time. He also began to show the presence of vegetations emitting a most offensive smell, in the groin, and a pustular discharge from the urethra which gave a negative result as regards gonococci. The temperature has remained very little affected notwithstanding the extremely septic nature of the surface affected. Examination of the unbroken vesicles showed them to be sterile. Cultures from the mouth gave mixed streptococci and staphylococci. My colleague, Mr. Ernest Lane, was kind enough to see the patient soon after admission, and was strongly of the opinion that the case was syphilitic, a view in which Sir Almroth Wright also concurred. Three Wassermann tests, one after a provocative injection of diarsenol, proved negative. There is no doubt in my own mind that this is an example of pemphigus vegetans of the grave type, and not syphilitic.

Case of Nævus Anæmicus (Vörner).

By E. G. Graham Little, M.D.

The patient is an unmarried lady, aged 33. Since birth she has had the condition for which she was brought to me by Dr. Findlater of Edgware. It has shown no change as she has grown older; it has never been different in colour, and there are no abnormalities of sensation or of the surface of the affected area, the only symptom being the ivory whiteness of the sites affected. These are distributed very much like

the fronds of maiden-hair fern, so that the cheek where the condition is most marked, in contrast with the rich fresh colour of her natural complexion, looks as if a spray of white maiden-hair fern were laid on its surface. The condition is also present on the side of the neck in the position shown in the diagram. Here the whiteness is much less obvious, and there is probably some lack of pigmentation as well as of the vascular redness.

The distribution while wholly unilateral and therefore suggesting a metameric segmental area, does not seem to comply strictly with any of Head's areas, the nearest approach being perhaps to his "hyoid" segment.



The disease must be of extreme rarity. No case seems to have been reported in this country. Lane, of New Haven, U.S.A., writing in 1913, had found no English record, his search of course including American literature. Vörner, who first described and named the condition, in 1906, returns to its consideration in a paper ten years later, in which he also notes the fact that while nævus anæmicus is a "nævus unius lateris," it does not follow Head's segmental distributions.[1]

[1] *Arch. f. Derm.*, cxxi, p. 376.

Case of Quinine Dermatitis.

By E. G. Graham Little, M.D.

The patient is a demobilized soldier, aged 37, who served in Salonika and Turkey and Russia, and left the army in January, 1919. I saw him first on July 1, when he was sent to me for an opinion by Dr. Davidson of Hanwell. He then had a vivid scarlatiniform erythema of three days' duration of the neck, chest, front of elbows, forearms, lower abdomen, groins, and buttocks. The rash was very itchy and burning, but he otherwise felt pretty well. The skin of the hands and feet (the palmar and plantar surfaces) was greatly thickened, and was partly exfoliating. The face was quite free. His history at that time was that he had had an attack of malaria three weeks previously, for which he had been given quinine in doses not ascertainable, and that the rash had appeared while he was having this drug. He also described a very similar sequence of events when he was given quinine in Russia in 1917, when he had had malaria. Dr. Davidson stopped his quinine, on my reporting my opinion that the rash was produced by this drug, but found no appreciable difference in the rash, which indeed seemed to spread, in extent, but was less congestive, and as the man had again a bout of malaria, he resumed quinine in ten-grain doses, *ter die*, without apparently making things worse. At the present time there is a nearly general exfoliation of the skin of the trunk, and the hyperkeratosis of the palms and soles is very marked. But the vivid redness has almost entirely subsided.

The case recalls a very striking record by Sir James Galloway of an erythematous and exfoliative dermatitis following on surprisingly small doses of quinine, but, in contrast with the frequency of quinine medication, dermatitis attributable to it seems to be remarkably uncommon.

Sir James Galloway: I am inclined to accept the diagnosis of Dr. Graham Little. A patient of mine whom I had shown for quinine dermatitis some years ago before this Section, came under my notice in another connexion within the last two or three weeks, and the man had vivid recollections of what he had suffered from his quinine erythema or dermatitis. The case may be a warning to others, inasmuch as this man got quinine dermatitis when he indulged in refreshing draughts of aërated water, which contained quinine in what were supposed to be tonic doses. This aërated water had a vogue in

London at that time. The patient had an extremely vivid erythema, and this was followed by a considerable amount of desquamation in area corresponding to the eruption on this patient. It was a very severe case, and the hyperkeratosis of the palms and soles was considerable in degree. I am not surprised to find that feature in this case also.

Case of Pemphigus.

By J. L. BUNCH, M.D., D.Sc.

PATIENT, a boy, aged 10, developed pemphigus when 3 days old, and has had recurrent crops of the disease ever since. The trunk and limbs are now covered with innumerable small bullæ, some ruptured, but mostly containing a clear fluid and rising from an entirely uninflamed base. There is no herpetiform grouping, no itching, and the boy's general health is entirely unaffected. He attends school regularly. The contents of the bullæ are sterile. There is no reason to suspect syphilis, and there is no similar disease in his family.

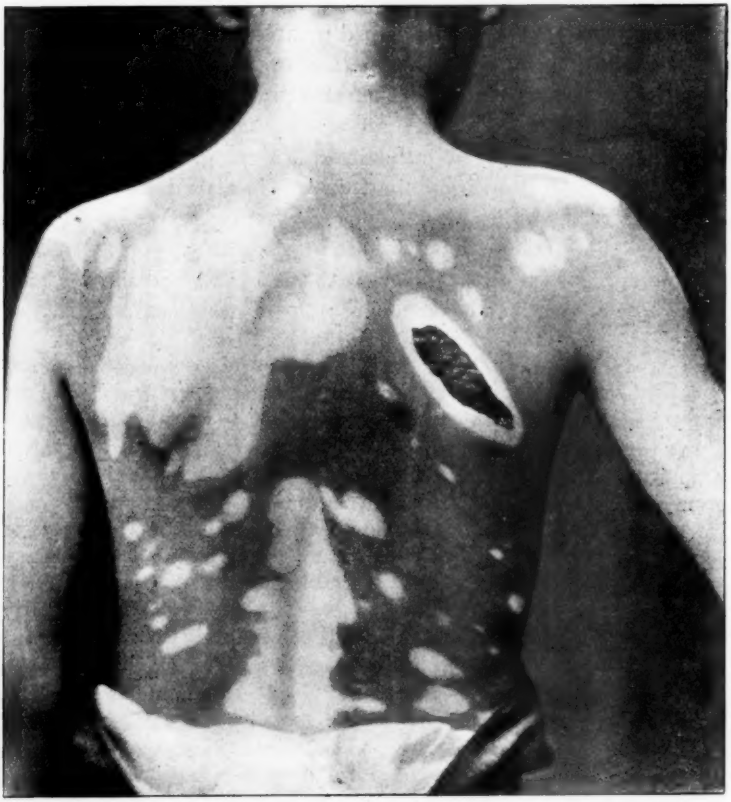
The pemphigus improves under local treatment, and the boy may be free for a day or two, but never longer: the disease recurs.

Sir JAMES GALLOWAY: I have had a few cases recently under my observation. I do not wish to raise the whole question of pemphigus here, but the striking thing about this case is that it is supposed to have started when the child was three days old. I should think that is almost a record for this type of pemphigus. I have recently had a severe case of "clinical" pemphigus under my care—a child of 8 or 9. The condition started within the first twelve months of life. So far as treatment is concerned, most cases yield to proper care, in which cleanliness plays an important part, but every now and then a case is found which resists everything, and the bullæ go on forming. My own experience is that the drier they are kept (so far as the patient will permit) the better. In the particular case to which I refer we kept the child dry with various powders, and finally used a powder of "flavine," which seemed to do good. The child now lives at Nottingham, and has consequently passed from my observation. If anyone should come across the child in any of the Nottingham hospitals, I hope he will report on it to this Section, so that we do not lose sight of it. Dr. Graham Little asks me what is the ætiological relationship of pemphigus. That, of course, is the crux of the whole question. My own impression is that nearly all these cases are locally infective.

Vitiligo with Central Pigmented Moles.

By J. L. Bunch, M.D., D.Sc.

The patient, a boy of 14, has a large number of vitiligo patches scattered over the trunk and limbs. These vary much in size, and are mostly irregular in shape. Some few are definitely circular. It



appears uncertain exactly how long the lesions have been present, but certainly a good many years. The boy is well nourished, active, and attends school regularly.

On the right side of the neck is a white patch, about the size of a sixpence, and in the centre of this is a small, pigmented, brown mole. It is elevated slightly above the surface, and can easily be felt. On the right side of the chest is a larger vitiligo patch, about the size of a crown, with a similar brown mole in the centre. But the mole is larger, more elevated, and rather darker in tint. Over the right scapula is another white patch, about 4 in. by 1½ in., oval and sharply marked off from the surrounding healthy skin. Running in its long axis is a large, brown, pigmented mole, about 3 in. long. It is raised about ⅛ in. above the surface, is papillomatous and covered with coarse, rough scales.

In January, 1918, I showed a case at the Royal Society of Medicine of a girl aged 12, with two patches of vitiligo, in each of which was a small central mole.[1] Sutton, in the *Journal of Cutaneous Diseases*, 1916, recorded two cases, and Sir Humphry Rolleston, in 1910, seems to have had a similar case, but it was complicated by jaundice. I have discussed these cases in a paper in the *British Journal of Dermatology*, 1918.

As regards treatment, the two small moles can easily be removed, and I propose to excise the largest mole. I have recently seen a case in which similar pigmented moles remained quiescent for many years, and then rapidly took on the characters of melanotic sarcoma. I had the tumour removed as soon as I saw it, and it remains to be seen whether this was done sufficiently early, or whether the patient will develop metastases.

Dr. F. PARKES WEBER: This is different from a case shown before the Clinical Section of the Society in 1910.[2] That case showed moles in the centre of congenital leucodermic spots. There was no vitiligo in that case, and I believe the spots were of the nature of what one might call "leucodermic nævus."

Urticaria Pigmentosa.

By J. L. BUNCH, M.D., B.Sc.

THE baby, aged 9 months, is covered with brown urticarial lesions, some macular, some definitely raised. The lesions vary in size. There appears to be little irritation and the baby only scratches at night.

[1] *Proceedings*, 1918, xi (Sect. Study Dis. Child.), p. 17.

[2] Cf. remarks by Dr. Parkes Weber in the discussion on Sir Humphry Rolleston's case of "Jaundice with Areas of Unpigmented Skin," *Proc. Roy. Soc. Med.* 1910, iii (Clin. Sect.), p. 197.

The case is shown chiefly because the scalp is extensively involved, as well as the face, and I do not remember having seen previously any such marked involvement of the scalp.

Carpoglyphus Passularum causing Dermatitis.[1]

By W. J. O'Donovan, M.D.

(ABSTRACT)

The man concerned in this case is cured, and does not appear, but I can show photographs as well as a sample of certain figs which were the cause of the trouble. The patient was a dock labourer, aged 24, who complained of an itching of the forearms which had worried him for some days. It appears that he had been shovelling discarded figs at the docks. Twelve men were working with him, and eight of his fellows were affected with itching arms. On the front of his forearms and the front and back of his hands and slightly on his face were numerous discrete red papules. He was asked to bring up some of the figs he was handling, and we tried them on the hospital monkeys, who enjoyed them, and suffered no ill effects. Under the microscope, however, one could see that the dust of these figs consisted of an enormous mass of dead and live acari. Dr. Bulloch put us in touch with Dr. Allcock, who named them the *Carpoglyphus*. I could find no other instance of the kind in this country, but Dr. Oliver tells me that in the last annual number of the *Proceedings of the Danish Dermatological Society* Professor Rasch had described two similar cases—a man and a woman—who were similarly affected after handling plums. Professor Robin, of Paris, in 1869, referred to this organism, but only to its anatomical characteristics, and I show through the epidiascope the drawings which accompanied his description and microphotographs of the present parasite.

Dr. Arthur Powell: I have an impression that I have seen reported—probably in German journals—cases of dermatitis attributed to *Carpoglyphus*. I think it probable the animal was an accidental intruder rather than a causative agent. I am sorry to see perpetrated in text-books of parasitology and of dermatology a statement that the so-called *Rhizoglyphus parasiticus*

[1] This case will be reported, *in extenso*, with illustrations, in the *British Journal of Dermatology* for August and September, 1920.

of Dalgetty is the cause of what planters call "water itch," "ground itch," or "coolie itch." This is now generally admitted to be due to the entry of the hookworm. I was present at the local medical society in India when Dr. Dalgetty first broached his theory and showed specimens of the *Rhizoglyphus*. I at once recognized the animal as a very common inhabitant of decomposing animal and vegetable matter. There is nothing to prevent the *Rhizoglyphus* straying on to a man's skin or his trousers, but if it should shelter in a ruptured vesicle of the skin there is no more reason to accuse the animal of causing the vesicles in the skin than there is of its producing the button-holes in the trousers. Dr. Dalgetty came and stopped the next day at my house and in that time was able to produce the identical "parasite," *Rhizoglyphus*, from an old pair of boots, an old saddle, and from the coating of some neglected pills. It would seem as if the animal was fond of keratin-containing matter as a food, and doubtless the peeling epidermis of a diseased foot would attract the animal as a tit-bit. Dr. Dalgetty was then satisfied of the identity of the animal and that its presence in the skin in cases of coolie itch was accidental. I thought we had then decently buried the animal as far as its parasitic or pathological character was concerned, but I regret to see its resurrection in even the latest editions of many text-books on parasitology and dermatology such as Braun, Fantham and Castellani. As a parasite *Rhizoglyphus* may safely be wiped out.

Case of Lupus treated by Brass Paste and Bro.

By Arthur Whitfield, M.D.

Most of us have been interested in the description of cases treated in this manner and have tried these preparations. Dr. Ellis, who introduced the method, kindly called on me and learning of my failure undertook to treat a case in one of my own beds. This woman is 48 years of age, and her lupus began at the age of eight. She has been treated at St. Thomas's Hospital, scraped a great many times, excised once, and has had Finsen light for five years. I think the dermatologist means by lupus always one distinct form of tuberculosis of the skin, while other people use the word perhaps rather more widely, to include what we should call scrofulodermia. One of the reasons for which I have picked out this case to show the Section is because, although it is of such long standing, it is evidently not a case in which the patient has any great tendency to succumb to a tuberculous condition. There is a large scar which is quite sound, with no nodules to be found in it. This patient was taken into King's College

Hospital, and Dr. Ellis kindly treated her with his brass paste method. The result of the treatment has been this: it began by ulcerating the lesions pretty severely. Curiously enough, it ulcerated the active edge less than it ulcerated the part of the scar which was not quite sound. The patient also had a temperature on one or two occasions, and suffered from some constitutional disturbance such as headache. I am bound to say that in my opinion in this case the treatment has done neither good nor harm. It has given rise to some discomfort, but I see no prospect at all of any permanent improvement as the result of the application of this method.

DISCUSSION.

Dr. ELLIS: In the matter of the treatment I may say frankly that I have been disappointed in this case. I recognized when it was presented to me that it was an extremely difficult case to treat. I have treated now something like 120 cases, so I speak from a good experience. This patient displayed one serious symptom which I have not yet seen to anything like the same extent in other patients who have been treated—namely, the extraordinary constitutional effects. I look on the whole thing as a question of balance between the germ and the tissues, and there is no doubt in my mind that there is a distinct increase of advantage to the tissues when these applications are made. No doubt in such cases as these in which there is a condition of hypersensitiveness—a cutaneous reaction to 1 : 1,000 tuberculin—a great deal more difficulty will be experienced in handling them than is found in a case which does not give so high a reaction. The main and very important fact, however, is that this treatment is potent in its action on tuberculous tissue, which is resolved under its influence. It is definitely proved that it does not kill the organism. It is no trouble now to grow this organism from the treated lupus case. But the fact is very important that the treatment has a specific action, without in ordinary cases any constitutional action. It can be given internally.

Dr. ADAMSON: Some eighteen months ago I began to use this treatment. Dr. Ellis very kindly supplied me with material and instructed me in its method of application, and I took some sixteen or eighteen cases of lupus and gave it a trial. At first I was very pleased with the results. The brass ointment when first used picked out the nodules very neatly—better than the pyrogallic acid paste which I had been using. But I found that as soon as the ulcers caused by the removal of the nodules healed up, the lupus nodules reappeared. I continued to treat these cases in this way for six or nine months. In no case did I succeed in curing any lupus patch and I have now discontinued using the brass ointment.

Dr. SEQUEIRA: My own experience is on all fours with that of Dr. Adamson. I had a number of cases treated by the brass paste, and there was certainly a very definite influence upon the lupus nodules of low resistance, but I do not think that the cleaning out of the lupus nodules thus obtained was better than that obtained with a very strong—33 per cent.—creosote and salicylic plaster. I have not seen a case of lupus cured by the brass paste, and in those cases which I myself call lupus vulgaris I have not seen any results which would incline me to continue the treatment after quite a fair trial.

Dr. GRAHAM LITTLE: I have tried this method largely in my department for the last two years, and with exactly the same result as has been described by the last two speakers. In fact, I would not go so far as Dr. Sequeira, for I do not think that even in lupus of low resistance it has had any appreciable effect. I have given up using it for any type of lupus. It was given a very enthusiastic trial because we happened to have a new department for tuberculosis started at the hospital at the time of the introduction of the method and the officer in charge was given a free hand with my cases, but in spite of that early enthusiasm I have ceased to use it.

Dr. WHITFIELD: This case of mine was a test case because it was treated by Dr. Ellis himself. I had used the brass ointment method in other cases with the same results. Where we have what we dermatologists call lupus—that is, a pure tuberculosis of the skin—it is almost if not quite the only kind of tuberculosis in which the effect of the remedy can be watched. This is not the case with tuberculous glands, for instance. If Finsen light be applied to a small nodule of lupus and its position be taken with atrophic skin, then one has a feeling that one has done something. But in the case of this paste method, the part has been made sore, and that is all that has been done. There are here four of us (Dr. Adamson, Dr. Sequeira, Dr. Graham Little, and myself), all of us experienced in the treatment of lupus, but we have failed in every single case. I regarded Dr. Ellis's proposal to treat this case as being in the nature of a friendly challenge. But the challenge has resulted in the patient being left *in statu quo*. There is no improvement in any particular that I can see. Dr. Ellis says that he recognized it at once as a very resistant case. But I recognized it at once as a case with a tendency to get well. It is a case in which the lupus has not rapidly extended, and there is a good deal of quite pure and healthy scar. If this treatment will not cure a case of lupus of this kind it must be put with the other failures in the treatment of tuberculosis. A specific treatment that will not cure the tuberculosis of lupus will not cure tuberculosis in any other manifestation. I wished very much to see the cultivation of the bacillus from the lupus in this case, but there again I met with disappointment. This test case has failed both as regards the treatment and as regards the demonstration of the possibility of cultivating the *Bacillus tuberculosis* from the lupus. This question of balance between the tissues and the parasite appears to me to be the purest

theory. There is only this evidence for a balance between the tissues and the parasite, that if the disease extends rapidly the tissues are getting the worst of it, and if it does not extend rapidly or tends to spontaneous cure, the bacillus is not making headway or is getting the worst of it. If one talks about balance, this case is one surely in which the balance is very equal. I should like in six or nine months' time to bring the case again before the Section after it has been treated by other methods to show whether these older and better known methods will not produce a more favourable result.

Arsenical Keratosis and Epithelioma.

By G. R. Hamilton, M.B., M.S. (Sydney).

This patient, aged 46, has had psoriasis ever since she was 10 years old. For thirty-six years she has been under various doctors, who always prescribed for her medicine containing arsenic. The psoriasis would at times appear to go away, but always very quickly returned. She took the medicine for nine out of each twelve months in each year.

Twenty years ago she noticed the skin on her palms and soles becoming thick. Fifteen years ago a few yellow spots appeared on her chest, and the areolæ around her nipples became darker. Four years ago she had all but her lower four incisor teeth removed because of their increasing bad condition. Eighteen months ago two spots appeared on her right arm, at a place where she had never had any psoriasis. Six months after their appearance she had them removed by CO_2 snow, but one month later they reappeared, coalesced, and became larger.

In March, 1920, she consulted Dr. Malcolm Simpson, who diagnosed the case and sent her to my chief, Dr. J. H. Sequeira, who admitted her to his ward. While in the ward the growth was removed, and Professor Turnbull reported it to be a "squamous prickle and horny-celled carcinoma." There is a section under the microscope. Her urine contained a slight trace of arsenic, and Dr. Marrack also found by Marsh's test slight traces of arsenic in her hair, skin and nails. Her blood only showed a slight leucocytosis. As can be seen, she has hyperkeratosis of the palms and soles, "rain-drop" pigmentation on the abdomen, and a few raised pigmented areas on the areolæ.

DISCUSSION.

Dr. WILLCOX: Arsenic has the character of depositing itself on the skin of the palms and on the nails and hair in the manner described. The nails in this case show that the recently grown portion is different from the distal portion, and I take it this corresponds to the growth which has occurred since the patient ceased taking the arsenic. An interesting feature is that sometimes in arsenical cases one may get, not the palmar thickening shown in this case, but a reddening and sweating. There was one such case shown before this Society some time ago, where I think you, Mr. President, suggested the possibility of arsenic, and in that case arsenic was found in the urine in appreciable quantities over a very long period.

Dr. W. J. O'DONOVAN: Dr. Willcox's interesting comments on this superficial pigmentation due to arsenic reminds me of the parallel conditions of pigmentation keratoses and carcinomata found in tar workers. In a paper by MM. Ad. Bayet et Aug. Slosse, in the *Comptes Rendus de l'Académie des Sciences*, 1919, No. 13, p. 704, the authors relate the constant finding of arsenic in the factory dust, pitch, hair, blood and urine of briquette workers. Briquettes are a patent fuel made by mixing and pressing coal dust and the residue of tar distillation, pitch.

The PRESIDENT: In the early stages of arsenical pigmentation it is not possible to diagnose it from pityriasis versicolor by simple scraping. I was under the impression when I first noticed that in some cases the pigmentation of arsenic lay in the horny layer and could be removed by scraping, that it was a new observation, but I found that Dr. H. G. Brooke had noted this fact in his Manchester cases. Of course one cannot remove the pigmentation in all cases in this way.

Case of Parapsoriasis en Gouttes.

By M. G. HANNAY, M.D.

THE patient, a man, is aged 22. There is nothing to remark in the personal history. The eruption began three and a half years ago (about Christmas, 1916), while in Egypt. Lesions appeared first on abdomen, groins, inner part of thighs, and then spread over trunk and limbs. He thinks that at first it was not scaly, but only became so later. There has been practically no itching. At first he had four days' treatment with sulphur ointment, which made it worse, but apart from this he is not aware that any treatment has had any effect.

At the present time the eruption is distributed over the lower part of the neck, trunk, arms, forearms, thighs and legs. There is a very slight degree of pityriasis of the scalp. The face, hands and feet are unaffected. The majority of the nails show one or more white spots, the size of a small pin's head.

The majority of the lesions are from ½ cm. to 1 cm. in diameter. They are scaly and of a faint rose or fawn colour. There is no induration. They are more or less circular in outline, showing only slight tendency to coalesce. A very few show a collar of scales at the circumference. On scratching with a blunt curette the scales appear whiter, but are not typical of psoriasis. On removing the last scales, a fine punctate purpura appears before hæmorrhage. Von Pirquet reaction negative.

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VOLUME THE THIRTEENTH

SESSION 1919-20

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Section of Electro-Therapeutics.

President—Dr. A. E. Barclay.

PRESIDENT'S ADDRESS.

Ideals in Radiology and Electrology.[1]

By A. E. Barclay, M.D.

The war has brought into prominence the wide fields that are open for the uses of electricity and other physical forces in medicine, particularly of X-rays, the result being that many men are prospecting with a view to taking up the work represented by this Section. For instance, in my own city five men are now established in X-ray work where formerly there were but two, and I understand that this is more or less the case in all the large towns. I thought therefore that it might be of value if I addressed myself particularly to those men who, knowing only the military side of our arts, should know what the future holds if they are to become successful practitioners.

Never were the responsibilities and possibilities of our Section greater than at this present time: a time which, in my opinion, will determine whether that Section is to "make good," or to lapse into mediocrity. You all know the past history of the medical application of electricity in medicine. The spectacular effects played into the crafty hands of the charlatan and filled his coffers. No claims were too extravagant when backed by electric sparks, and the credulity of the public was, and still is, quite incomprehensible. James Graham's celestial couch at £50 a night was, one would have said, an extravagance

[1] At a meeting of the Section, held October 17, 1919.

of impudence that even fashionable society would have ignored; yet it was a source of considerable revenue to the impressive blacksmith.

During the latter part of the eighteenth and throughout the nineteenth century, science pushed ahead, and at the present day the traditional art of medicine has been forced to yield to a science that becomes each day more exact, and more exacting from its practitioners. No medical man can do justice to his patients if he does not keep abreast of the enormous progress that is taking place in the application of scientific knowledge to diagnosis and healing; no experience in medical practice can make up for the absence of scientific knowledge. Even at the present time, however, the credulity of the public and its love of the occult and impressive, form a serious barrier to progress—so-called educated people are apt to appraise the work of the electro-therapeutist by the impressiveness of his apparatus. When the history of electricity in medicine comes to be written, one of the most striking facts will be that science eventually triumphed in spite of all the abuses of unprincipled practitioners, some of them unfortunately not outside the medical profession. Even at the beginning of this century some of our brethren looked askance at us if we possessed an electric battery; and the old British Electro-Therapeutic Society, the predecessor of this Section, had difficulty in obtaining recognition. The advent of X-rays, with their obvious utility in so many directions, was an enormous asset in the establishment of the value of the other uses of electricity, and a society that devoted a large portion of its energy to the study of X-rays had strong claims to come within the precincts of the Royal Society. To those who laboured and brought us through those difficult times, particularly to Dr. Lewis Jones and Sir. J. Mackenzie-Davidson, we owe a debt that can never be repaid; but they finished their work and passed on knowing that their efforts had done much to establish electricity in medicine on a sound basis.

A friend of mine, given to epigram, shocked a surgeon one day by saying that "there is a great and expanding future for physical treatment in medicine, of which surgery will eventually be one of its smaller branches." Of course this is a gross exaggeration of even our most sanguine pictures of the future, a future to which we look forward with growing confidence as the various societies and publications gain strength and influence. The name of our Section, however, is a clumsy one and no longer descriptive. We hope that it will shortly be renamed as the Section of Physical Medicine, and some of us look forward to a

time, not for distant, when it will be necessary to establish sub-sections in order to deal effectively with the somewhat diverse interests that are represented at our meetings. Radiology (in diagnosis), X-ray therapy in deep and superficial conditions, radium therapy and electro-therapy already claim their specialists, while such subjects as massage and remedial exercises are so closely associated with electro-therapy that they might well come in under the wing of a Section of Physical Medicine.

The Training of the Radiologist.

Whatever view we take of the future possibilities of our subject, the present is with us and I need hardly point out how vast are our opportunities—opening out in so many directions that no one worker can be a master of them all. What a change from twenty years ago, when in a few weeks a man could make himself as competent on the medical side of the subject as was possible! To-day it is a different story, and to take his place, he must be prepared to master a diversity of subjects such as are necessary in no other department. Of course it is essential that a man taking up this line of work should know his physics and electricity and all the technical work in radiography, radio-therapy, electro-therapy, &c., which are carried out for him by lay assistants. He will have to be responsible for the work of his department and no man is fit to do this who does not know the general principles, and the detail, of every job that is carried out for him by his assistants. This implies that he should also know the construction of his apparatus and the theory on which it is constructed, for it is for him to say what is, and what is not, possible with the apparatus at his disposal. In short, he must be the master and must be able to speak with authority to his radiographer, photographer, or any other assistant, and to correct faults in their technique. And these things are outside and introductory to the science and art of medicine, of diagnosis and healing by physical means. The man who takes up this branch must be prepared to master these subjects if he is to approach his speciality from a scientific point of view. The man who by turning on a switch, produces such and such a result is as empirical a physician as one of a hundred years ago who loaded a bottle with many different drugs, in the hope that some one of the constituents would be of value. He did not know how or why he obtained his result, but at that time the knowledge was not available. To-day the man who fails to know the A, B, C of the science in which he deals is nothing less than a quack, and presumably a lazy one, for

the knowledge is ready to hand to any man who cares to learn. I admit that, at the moment, the sources of knowledge are scattered and have to be sought out, but the establishment of the diploma at Cambridge with its very sound syllabus, ensures courses of study where all may obtain the requisite training.

The importance of specialized teaching for our subjects is patent, especially to those of us who had to find our own way into it, not knowing what lay before us, or what equipment of medical knowledge was necessary; before apparatus was standardized and each new "stunt" had to be tested. Nowadays, by comparison, the work is stereotyped and we know fairly well what apparatus is necessary and the lines on which a department will be developed. No department that I know has, however, attained the place it should reach, for, in my view, the X-ray and electrical department is bound to become one of the most important branches in any hospital. As an illustration of the growth, I will instance the fact that whereas ten years ago in the hospital with which I am connected, one in a hundred of all registered in- and out-patients found their way to my department for diagnosis or treatment, to-day more than one in eight passes through my hands, in spite of the fact that we do no skin work and that there is a special Institute in the hospital for radium which deals with about the same number of cases as the London Radium Institute. I have no doubts that departments in other hospitals show much the same growth, and in every one of them it is spontaneous and necessary to the efficiency of the hospital. To take over the management of such a department is a grave responsibility and no man can be too well equipped for his post. There is no appointment in the hospital which calls for better or more diverse training, for the head of this department must not only know his own work but must also be able to consult with the specialists in every other branch, and to do this he must be thoroughly conversant with their work and with the ways in which his branch can assist them. The business man puts his son to work as an office boy, and afterwards through all the clerical and other duties that comprise his business, and it would be the best possible training for the radiologist and electro-therapeutist if he could pass through a similar process. Presumably he has been a house-surgeon and house-physician and has been attached to some of the special departments, particularly the pathological. He should also pass through the X-ray, electro-therapeutic and radium departments, especially on their technical side, till he knows the detail of every process in each section; at the same time he must learn the

theory on which all this work is based. Fortunately the manipulations in the technical portion of his subject, usually carried out by lay assistants, do not require extensive practice. None the less, if it is possible, every serious student should know each detail of work that is performed in routine manner by the lay assistants. If he cannot master these things without doing them himself, let him take holiday duty in the dark room, in the radiographic room and elsewhere. Let it never be forgotten that if the medical man does not know his technical work, the results, when he takes charge of a department, will be dependent on his assistants, whom he will be incompetent to control. The head of the department must take the responsibility for the technical work of his assistants—if the work of his assistants is bad it is for him to set it right.

In your hands you have the outline of the proposed syllabus for the Cambridge diploma. You will note that it is comprehensive, and there are few of us who would care to tackle an examination based on this syllabus without a good six months' preparation. Many of us feel the great disadvantage under which we labour in not having had such a course of training before we started on our work. To have had our elementary knowledge of electricity expanded in the directions indicated by the syllabus would often have stood us in good stead, and to have thoroughly understood the physics of our subjects would have added greatly to our efficiency and, incidentally, to our interest in the work.

Systematic courses of lectures and demonstrations in radiology, radio-therapy, electro-therapy, and the other medical aspects of our branch should give the novice a groundwork that is the envy of those of us who had to grope our way. Some of you may be surprised, shocked and perhaps grieved to notice that in addition to all the various courses that are special to our branch, there are also courses in anatomy, physiology and pathology: subjects that some of us thought we had buried with our botany and zoology when we took our first hospital appointments and became "practical" physicians. Those responsible for this diploma had one thought in mind—i.e., that those who carried this hall-mark should be competent to be *consultants* in their art, as far as it is possible to make them so by teaching and examination. There is, however, one factor that no examination, and no board of examiners can ensure—the individuality of the man who presents himself for examination.

The Radiologist and his Apparatus.

It has been very truly said that the consultant is not the man who wishes to be consulted, but the man whom others would wish to consult —the ideal man for this work therefore is he who combines the keenness to master the whole of our arts with the personality which induces confidence in the opinions he expresses; one cannot labour the point too much, that it is the opinion and theory which will count in the long run, no matter how showy and expensive the apparatus, or how perfect the X-ray plates. On the other hand, nobody will trust a consultant who is content to turn out second-rate technical work, whose apparatus is slipshod and whose methods indicate carelessness and lack of thought.

In no other branch is there room for such thought and ingenuity in the tools with which the work is done, and this factor has attracted some of our ablest workers. Nearly all our apparatus has been designed by, or built to the instructions of, medical men, and there are few among our number who have not done something in this line, and who have not themselves actually made a certain amount. It is a delight to see the apparatus in some hospitals and private rooms—pure make-shift devices, often made for a few pence, out of some piece of scrap, and yet performing the work for which it was designed with perfect efficiency. The men who do such things must be a great worry to the instrument makers, for they save themselves and their hospitals hundreds of pounds. We have all seen many of these devices, and some of them have pleased me greatly; particularly an X-ray table, made out of packing cases, that did excellent work in a base hospital in France, and was as quick and easy to use as any other I have seen—its cost was certainly less than £1 in addition to the waste wood used, but it would not have impressed private patients! To demonstrate a new localizer for foreign bodies in the eye, a friend demanded a hairpin, a bit of looking glass and an empty half-plate box, and in a few minutes had erected a contrivance that looked like no scientific instrument on earth, and yet it would certainly have given results fully as accurate as, if not more accurate than, those obtained from any of the standard instruments.

In passing, when talking of the localization of foreign bodies in the eye and the marvellous precision attained, I would suggest as a most instructive experiment the re-localization of a few cases, taking pains not to know what previous results have been. It is not really that

these foreign bodies move, but that there is no way of fixing the eye, for it is not in the operator's control. Given an intelligent subject, the comparative results of two examinations, with a good method, should not be far astray, and I would guarantee that the cardboard box and hairpin arrangement would compare very favourably with any in this respect.

There are hundreds of pieces of apparatus of this type, not necessarily cheap and nasty, but all answering their purpose more or less efficiently, and, apart from economy, the only justification for such makeshift apparatus in a department or consulting room, is that it performs its function efficiently. If it fails in this respect and is still kept in use it shows that the man who uses it is content with inefficiency because of his pride in the device, a pitfall into which some of our ingenious workers are apt to fall. I sometimes think that those who deal in electricity are like collectors of curiosities and old furniture; either they make an impressive *nouveau riche* show with obviously expensive apparatus, or else they take a great pride in wonderful bargains they have picked up, or to be more correct, manufactured by their ingenuity. But all this is a digression from the point, that although a man may apparently have poor apparatus, his ingenuity may evolve perfect technical work out of it. It is, however, only a master craftsman who can do this and the average man will have to rely on the instrument maker. Some show in his department and consulting rooms is not only advisable but necessary, because it is only those who understand such things who realize the brain that achieves results with the impossible appliances one sometimes sees; his patients, or the subscribers to his hospital, cannot appreciate his ingenuity, and, unfortunately, it is still a necessity that patients and subscribers should be more or less impressed by the doctor and the means he employs. Human nature is a curiously complex thing, and it will appreciate a brightly polished, and perhaps useless, appliance and a clean cushion cover, more than the cleverest piece of efficient apparatus. Nevertheless, it is still, and always will be, the personality of the man behind the gun who will succeed, for he will achieve moral victories in addition to the mere physical results. A certain amount of showiness in his outfit will be a material help in this respect, but nothing, not even the most costly and well kept apparatus and self-assurance of manner, can make up for ignorance of medicine, slip-shod methods and lack of control over the forces employed.

The Hospital Work of the Radiologist.

What is the work of a radiologist in his hospital department, a department in which three-quarters of the work can, and should, be done by lay assistants? As I have already said, his post is essentially that of a consultant, the man who directs, and who knows every detail of the work that is done for him. Like the head of a business, his work consists in the direction of others, and he must not omit to give credit where credit is due—the radiographer who works under an unappreciative radiologist soon ceases to take an interest in his work. It should be the aim of the radiologist, therefore, to make the radiographer feel the responsibility of his position. In one department that I know well, when visitors are present, the radiographer is invariably given the credit of all good plates, and of course visitors never see bad plates in that department.

The radiologist's desk should be cleared of all routine work the first thing, and, so far as is possible, he should relegate to others those things which it is not essential that he himself should do, in order that he may be free at any time to consult with his colleagues, either in the department, at the bedside, or in the theatre. In spite of organization, however, it is not a "cushy" job, for he has many duties to perform; he sees and reports on all the X-ray plates, sees all the treatment cases periodically, and each day he should inspect the work that is being done by his assistants. Personally, I think it an essential and most important part of his work that he should himself do all the screen examinations of intestinal and chest cases, and, so far as is possible, he should keep himself free to be in the operating theatre and the post-mortem room to follow up all doubtful cases and see his failures.

The closest co-operation should exist between the radiologist and his medical and surgical colleagues, and his first aim should be that his personal services and those of his department are freely at their disposal. His section is quite unlike all other special departments, unless it be that of the pathologist, for it is not self-contained but established as the auxiliary of all other branches of the hospital, for the supply of the necessary information on which the surgeon and physician act. His opinion on an X-ray examination should be sought by his colleagues because of its value, but no radiologist should take up the position that his opinion, and his only, is of any value. A surgeon or physician has a perfect right to draw his own deductions

from work done in the X-ray department, and it is "up to" the radiologist to supply him with all the material for this purpose. If the surgeon or physician trusts his own opinion, he does so on his own responsibility if it runs contrary to that expressed by the radiologist. In most cases, however, one finds that more and more of the responsibility is thrown back on the radiologist, in some cases to a quite unwarrantable extent, for the radiologist seldom has time to examine a case clinically, and his opinion is usually one-sided, for he does not know the clinical side of the picture. It is only fair that all requisitions for X-ray examinations should be accompanied either by a clinical diagnosis or by a brief outline of the symptoms and physical signs, and I make it a rule that I will not examine abdominal cases without some indication in this direction; in other words, it is just as essential that the surgeon and physician should co-operate with the radiologist as that the radiologist should co-operate with them. For instance, the knowledge that a tumour has been detected at the clinical examination makes the radiologist take special pains in the palpation of this region. In some cases he so alters his routine opaque meal as to ensure that the bulk of it will be in such a position as to assist him in determining the relations of the tumour. Or in urinary work, if details are not given, my assistant is instructed to ask a set series of questions which will supply sufficient data on the clinical side to avoid pitfalls in giving an opinion on the X-ray examination.

The work of the radiologist, as regards diagnosis, should not be crowded into the minimum of time, for it is essential that he should hold himself at the disposal of his colleagues at *their* times if he is to work in close co-operation—i.e., if he is to keep in close touch with *their* work, and act in the consultant capacity. A great deal can be done by reports, but there is hardly a day on which one or more of his colleagues, or of the resident staff, do not wish to discuss cases with him. And this co-operative work would be enormously increased, with mutual advantage, if the X-ray department were suitably placed—almost all the departments in this country have just "happened" in any odd cellar or outlying spot that was available, quite irrespective of its proximity to the wards and operating theatres. It usually means a special journey, in an out-of-the-way direction, for a surgeon or physician to visit the X-ray department.

It has also become necessary that he should give routine simple demonstrations to medical students in X-ray diagnosis, and advanced teaching to those who are intending to take up this branch of work.

In the research work of his colleagues or those who are writing up theses, the X-ray department is, or should be, greatly used, and the head of the department should take a keen interest in seeing that the maximum amount of assistance is given; this may mean that he has to do a considerable portion of the work himself. Moreover, there are innumerable problems always turning up, and, if he is keen, he will always have some investigations of his own on hand. In fact, even a comparatively small hospital will keep a man usefully employed for one half of every day, even if he gets his assistants to do all the mechanical work.

Definite hours during which he is certain to be in his department are necessary, for his colleagues will quickly tire of making useless journeys to consult him or to get him to make a screen examination of a special case. In some instances physicians and surgeons make appointments with the radiologist in order that they may bring their ward classes down to see cases that have been demonstrated clinically—an arrangement which is obviously sound, but which often dislocates the work of the X-ray department for an hour at a busy period. A demonstration room in the X-ray department of every teaching hospital is absolutely necessary.

The underlying principle for a successful radiographic department is that it shall be organized and run as an auxiliary to the medical services of the hospital, helping in every possible way to promote efficiency. If the radiologist runs it half-heartedly, or to suit his own convenience, there is constant trouble and friction, especially where the staff of the hospital is young and progressive. There is a very strong case so far as the efficiency of the hospital is concerned, for the employment of a whole-time radiologist. There is certainly quite enough work for him to do, even if he does none of the technical work himself. The difficulty, however, in existing conditions, is that it would be impossible on financial grounds to get the type of man necessary for such a post. From the point of view of private practice such an arrangement would be a calamity, for the men with the experience would not be available for private work, and, as usual, we shall probably continue with the compromise that has already become established.

The Abuse of the X-ray Department.

Much has been written on the use of X-rays and on their dangers. There is no call for me to enlarge on these subjects, but there is one result of X-ray diagnosis which is not fully appreciated and which I

class as a definite danger—i.e., the growing tendency to employ X-rays as a means of rapid diagnosis in preference to diagnosis by clinical methods. If this tendency increases, clinical diagnosis by the older methods will become almost a lost art.

How are X-rays to be used in diagnosis? Every case of injury or pain in the back comes down to the X-ray department, and, in most cases, no previous attempt whatever is made to arrive at an accurate diagnosis. The art of diagnosis by intelligent deduction, not far removed from guess-work perhaps, is no longer used in a very large number of cases. There is more than a tendency to laziness in the use of the clinical methods, with the result that the student is not taught to think things out for himself. In a large number of cases X-ray examination is quite unnecessary, and a careful clinical investigation would have revealed this, but it is a great deal easier to write a request for an X-ray examination than to go into the case clinically. From the teaching point of view this is an abuse of the X-ray department that must lead to most inefficient work when the students go out into practice and have not an X-ray service ready to hand. From the patient's point of view it is not satisfactory, except in the fracture cases where a great deal of painful examination is obviated. In the indefinite backache cases for instance, a negative renal examination accompanies the patient on his second visit to the surgical out-patient department, where he probably falls into the hands of an assistant who follows up the case in a desultory manner and with little interest—as it is not a first attendance—with the result that the patient is never systematically examined. The X-ray department should not be used in this way—its resources should only be called upon when the patient has been examined and a clinical diagnosis arrived at. If it were used in this manner, for the supply of confirmatory evidence, it would result in an increased accuracy of clinical diagnosis and the students would not scamp their clinical work. The way in which patients are referred to the X-ray department at present, indicates clearly either that the staffs are too busy to diagnose the cases by clinical means or that they do not care to risk a diagnosis that may be upset by the X-ray findings. Whatever the cause, however, one notices an ever-increasing tendency on the part of the junior staff to use the X-ray department as a short cut to a diagnosis, and they will not trouble themselves with clinical methods if they can get what they want from the X-ray department.

The Therapeutic Side and the Responsibility.

Turning now to the therapeutic side of the subject, the department conforms much more closely to that of the other special branches. Patients are referred from surgeons, physicians and special departments, and come, or should come, under the care of the electro-therapeutist. In many hospitals the position of these patients is ill-defined—they do not know whether they are under the electro-therapeutist or under the physician by whom they were referred. Personally, I have had to make a hard-and-fast rule both in private and in hospital practice, that I will only take responsibility for the patient in so far as he is under me for X-ray or electrical treatment—I will not prescribe a placebo, a purgative, a narcotic, or any other drug, nor will I even give instructions as to the ordering of the patient's life if I can evade doing so—i.e., I will not trespass upon the responsibilities of the physician in charge of the case. On the other hand, full responsibility for the work done in my department is accepted and insisted on. I will not take instructions from anyone as to what treatment I give or how I give it. The electro-therapeutist must himself see every case, and he must take the responsibility for the work done under his supervision—it is for him and him only to say whether the case is suitable for the treatment he controls and knows better than any one else, and it is for him and for him only to prescribe that treatment. It is also for him to see that what he has prescribed is carried out.

There are few radiologists who have not, in days gone by, fumed, both in hospital and private practice, at minute directions as to what is to be done with certain cases, instructions such as the radiologist should give to his assistant. Sometimes these instructions are quite intelligent, at others they are the most absurd and dangerous balderdash, obviously written with the intention of impressing the patient, and, as obviously, written by physicians who have picked up a few technical terms and do not know anything about the subject. I grieve to state that far and away the worst examples of this type of fooling with which I have met have come from well-known consultants. More often, however, the general practitioner hands on instructions that: "So-and-so says that the patient is to be treated in such a manner, and so often." (In all probability "So-and-so" never said anything of the kind, but, knowing the usual routine practice adopted, has stated an indication of this as the probable number of treatments that will be required in order to guide the patient as to the cost.) These experiences have become less

and less frequent in recent years, but curiously enough I have two mild samples this week, both from eminent physicians. The first, from a general practitioner reads:—

"Dear Sir,—One of my patients has recently consulted Dr. —— about Graves's disease. He has advised him to have a course of X-ray treatment of the thyroid gland 'one Sabouraud unit, well filtered, twice a week for a month. After that once a week for a month should suffice.'"

The next from the consultant himself:—

"I have asked Miss —— to see you with a view to having some electrical treatment as I think a sinusoidal current would do good. She suffers from pains in many parts of the body which, from their shifting character, may be described as 'neuralgic.' I can find no cause for them."

In the early days, it was a frequent occurrence to receive instructions such as "please give X-rays three times a week," which, after a little mild chaff as to the absurdity of such an instruction, was converted to "please treat with X-rays," and later on to "to see Dr. ——, with a view to treatment," leaving it to the man in charge of the department to choose such of the methods at his disposal as he knows will be most suitable. Obviously this is the proper course.

The result of instructions such as I have suggested is that either the electro-therapeutist has to hold his end up or else virtually acknowledge that "So-and-so" knows as much about electro-therapeutics as he does himself. I admit quite frankly that many a time I have diverged widely from my own routine, at some inconvenience, simply because "So-and-so" had prescribed the routine treatment that I would have given in the ordinary course of events. By so doing I have usually gained the patient's, and the general practitioner's confidence, which had been seriously prejudiced at the start. If we, as electro-therapeutists, turn on a switch at the bidding of some one who has read up the subject in a book, and have not the courage of our convictions, which are based on experience, we might as well retire gracefully and allow our profession to fall back into the hands of the quacks from whom we hoped we had rescued it.

The Status of the Radiologist and Electro-therapeutist.

The position is often almost absurd—a patient comes with a note from a doctor asking for ionization, of which he has just read for the first time, to be applied in a case in which we know perfectly well that

this form of treatment is useless, whereas we know that we can benefit the patient by some other form of treatment. Is the electro-therapeutist to let the doctor down by telling the patient that what he has recommended is useless, or is he to stand by his own experience? In my opinion there is no question as to what he should do; the only question is, how to do it without damaging the prestige of the doctor with his patient. In nearly every case it can be done by saying that we have found another form of treatment that is more successful in this class of case, and that, if the patient and the doctor are willing, we should like to try this in preference to the treatment prescribed; if we are not successful we will then try the form of treatment suggested. Of course, this attitude, if we are successful, at once puts us on a right footing with this particular man, but in the early days it was often a very difficult problem what to do, for the relations of the consultant and practitioner to the electro-therapeutist had been completely prejudiced by the absence of medical electro-therapeutists and particularly of medical men who took this work seriously. It has been a long, uphill fight, breaking down these prejudices, and even at the present time I do not think that there are more than half a dozen medical officers in charge of X-ray and electrical departments of the large hospitals who are on the full staff; in many cases they are paid perhaps £100 a year, and are treated by their hospital committees and by the consultant staffs on this basis. Needless to say, the work in these hospitals is sometimes hopelessly inadequate and is also on the basis of £100 a year. Little wonder, then, if the average doctor treats the electro-therapeutist as if he were still doing the same work as his predecessor, the layman, who operated switches, and doubtless did his best for the patient.

An X-ray and electrical department is the most vital of all the special departments, and on its efficiency the medical services are dependent. If those in authority are content with a department on the basis of £100 a year, it follows that the surgical and medical practice of that hospital will be on the same footing. To be efficient and progressive, the working costs must be fairly high, and the man who controls this expenditure, and who does work of such vital importance, must sooner or later take his place on the consultant staff—if he is worth it. And that is another point. In the past there has been little competition for these posts, because their importance was not realized, and little or no status accrued to the man who took them, or status was granted so grudgingly that competent men would not take the posts. Those who could not become surgeons or

physicians, and yet were unwilling to take up general practice, went to the X-ray or other special departments. Now the position is altered; in the war many men who formerly knew nothing of these things have learned the rudiments of X-ray and electrical work. They see the possibilities of our branch, and are keen to start in practice. Very shortly there will not be a town of any size that has not one or more specialists—and they must be specialists, for, if they are in general practice, their brother practitioners will not send them cases. In other words, there is now healthy competition which will ensure suitable candidates for any post that falls vacant. We have suffered in the past from men who have either taken their posts as a side-line or because nothing else was available to keep them out of general practice.

There is another factor also that has been a very strong deterrent to men taking up this work. I have said that their position, if granted at all, was granted grudgingly, and it is the consultant surgeons and physicians who have been to blame for this. They did not realize the importance of placing a first-rate man in the post and *making the post worth holding*. Personally I have never suffered in this way, so that my remarks are based entirely on what I have seen and heard of in other towns, particularly in London, where the consultant staffs have often ignored the man whom they nominated, and who spent a full half of every day in working for them at hospital. All their private cases (except doctors' wives) they have sent to other men, or more often to laymen, leaving the man who, even if paid £100 a year, is their colleague, to pick up a living as best he could. The gross unfairness of this is manifest, for, having only a half of the day left for private work, the radiologist cannot, unless he is loyally backed up by the men for whom he works in his hospital, make a sufficient income to warrant the purchase of expensive apparatus, which must be kept up to date. I have often heard the other side of the question from surgeons who have told me they could not send their cases to their colleague, because his technical work was so bad, or that they were bored by lengthy reports when they asked for radiographs. Even if this happened to be the case, as one has to admit it sometimes was, the fault lay in large measure, if not entirely, with the surgeons and physicians themselves, on a two-fold charge—i.e., that they did not select a first-rate man for a post that must inevitably rise to first-rate importance, and that, having appointed him, they did not back up their nominee and make his position assured both in private and hospital practice.

The X-ray Department.

A few words as to the department itself, and then I have done. In almost every hospital in this country the X-ray department has been housed in any place that happened to be available, often in a cellar, on the theory that a dark room was more easily made light-tight in a cellar than elsewhere, and that anything would do. In spite of this, these X-ray departments have grown as best they could, and many of them, including my own, are quite past praying for. To attempt to alter them is sheer waste of money—most of them cannot be altered to meet the necessities of present demands, to say nothing of the expansion of the work that must take place.

What are the requirements in a large teaching hospital? In the first place the radiographic department should be either close to the wards and operating theatres or else on the way to them, so that surgeons and physicians may see things for themselves. We must get out of our hole-and-corner departments, out of our cellars or attics, for the importance of our work, to say nothing of our health, demands ample space and air in which to do our work—which does not merely consist of four hours' operating a week, but requires half of every day. At least three rooms of ample size are needed: (1) For surgical X-ray work; (2) for routine medical X-ray work; (3) for demonstration purposes. In addition, there must be an office with a thoroughly sound index and cataloguing system, a store room, which will become an X-ray museum, and ample dark rooms and photographic rooms.

The Demonstration Room.

The demonstration room is the only one of which I wish to speak in detail because I do not know of such a room in any of our hospitals. The demonstration room, as I picture it, should be the place where the radiologist spends most of his time. In it all the X-ray plates are examined and reported on. The walls are covered with viewing boxes and stereoscopes, filled with interesting cases for demonstration, and a card index catalogue gives the location in lantern slide cabinets of a series of pictures to illustrate any subject on which the radiologist, or any of his colleagues, wishes to lecture, either with the lantern that is installed or in one of the lecture theatres. The office where the clerk works and the store-room with its wealth of material, open into this room, so that everything is readily available for demonstrations. A powerful

X-ray plant is provided with an upright screening stand and a couch to be used for demonstration, and also for any special work that may be going on.

Such a room would be a great attraction to his colleagues and to students, and would make available the enormous wealth of material that is at present often wasted. His colleagues could bring their students to such a place without feeling that they were interfering with the routine work of the department. The radiologist, although maintaining his supervision, would depute more and more of his routine work to his medical assistant in the medical X-ray room, and his radiographer and photographer would carry on their routine work in their rooms, unhampered by the intelligent but time-absorbing student.

There is another purpose that such a demonstration room might well serve—i.e., as a social centre for the staff where they could meet and perhaps have a cup of coffee while they discussed cases, football or politics. The absence of social life which has resulted from the growth of hospitals, especially on the unit system, has been disastrous, and members of the staff may go from year's end to year's end without meeting their colleagues. I suggest that the radiologist has a great opportunity if he builds a room of this type and introduces such a tone into it that he can draw together the scattered forces and interests.

In the smaller hospitals less elaboration is of course necessary—in the cottage hospital a single room may fulfil all purposes adequately, but in a hospital where there is any bulk of work two X-ray rooms are necessary and at least two separate and independent installations, apart from apparatus used for therapeutic purposes. Even at the present time we frequently find hospitals of two or three hundred beds and large outpatient practice that are "carrying on" with a single installation, and we hear of the surgical practice being held up for days because of a breakdown in the X-ray department. It is quite time such things were altered and the vital importance of the X-ray service recognized by the provision of adequate and suitable accommodation for the work, together with the universal recognition of the status of the radiologist.

It is not essential that the radio-therapeutic and electrical departments should be housed with the radiographic department, but that they should be separated is a disadvantage second only to that of the separation of the radiographic department from the theatres and wards. A divided department inevitably means that the therapeutic side of the

work does not get the attention that is necessary unless it is possible to place a medical assistant in charge of it.

In conclusion, the future, with its vast possibilities, is in our hands, and it is for us to rise to the great occasion which the war has afforded us in bringing our arts into the prominence that is their just due. Let us be worthy of it, and show that the confidence that is being timidly extended to us is not misplaced.

Section of Electro-Therapeutics.

President—Dr. A. E. Barclay.

Introduction to a Demonstration of a Simple Means of obtaining "Morton Wave Current" and "Static Modalities" from a Coil.[1]

By G. B. Batten, M.D.

Although static electricity has no doubt been used with therapeutic intent for centuries, I think it may be safely said that it had very little therapeutic effect except by suggestion until Dr. W. J. Morton, of New York, early in 1881 introduced the static induced current. From that time, however, the static modalities found a very useful place in electro-therapeutics. In 1899 the same worker gave us what is now known as the static wave current, or more usually the Morton wave current, which I believe holds an almost unique position in the treatment and cure of several painful affections, such as sciatica and other forms of neuritis. Static electricity has been very largely used in America, but to a much less degree in Great Britain, probably because in this damper climate it is more difficult to keep large static machines in a constant state of efficiency. In August, 1917, after reading the paper by Dr. J. A. Shorten and Dr. T. W. Barnard, of Bombay, on the suppression of inverse current from the secondary of coils by earthing the negative, I was led to think again of the problem of obtaining static currents from a coil. Soon after this, when treating a patient by X-rays from a very high tube, I noticed a distinct "breeze" coming from the end of a stick which I had placed under one of the high-tension wires to keep it further away from her hair.

"That, surely, is a static breeze," I thought, and so I began experimenting again, and very soon found the simple means of cutting down the ampèrage, or rather the milliampèrage, of the current from a coil without much impairing the voltage, which I hope shortly to demonstrate to you to-night.

[1] At a meeting of the Section, held November 21, 1919.

My device, shown in fig. 1, is simply a series condenser or condensers, a condenser in fact where there are several pieces of tinfoil or conducting material separated by glass plates as the dielectrics, and in which none of the pieces of tinfoil are connected with each other, as is shown in this diagram. The sizes I happened to select as efficient were four pieces of tinfoil, each 8 in. by 5 in., separated by plates of cleaned photographic glass 12 in. by 10 in., and all insulated by hard wax. The first piece of foil is attached to the wire from the coil and the last piece of the electrode to the patient.

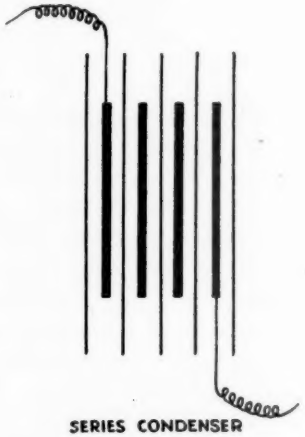


FIG. 1.[1]

Fig. 2 shows the whole device. It consists of

(1) An induction coil with a long fine secondary winding; any of the old coils which have been discarded of recent years, because they did not give enough milliampèrage for modern X-ray tubes, will do. A coil with a thicker secondary winding will not do nearly so well.

(2) An adjustable alternate spark gap with ball terminals, and preferably enclosed, to drown the noise of sparking, not in the usual glass vessels but in a simple, rather large, wooden box or cabinet. With glass, moisture soon collects and no proper spark passes.

(3) The special condenser resistance already described.

(4) A good insulating stool.

[1] Permission to use this block and that on the next page has kindly been given by the Editor of the *Journal of the Roentgen Society*.

The method of using is to cut down the current in the primary to 75 or 50 or even 35 volts and to make the time of contact in the turbine or other mercury interrupter very short.

To make a good earth contact from one of the secondary terminals, and to take the current from the other terminal through the special series condenser resistance, and then to the patient on the insulating stool. Of course, all this side, including the condensers, must be well insulated from earth.

Now a static current has been defined as a constant unidirectional current of high potential and very little ampèrage. It is therefore quite impossible to obtain a true constant static current from a coil

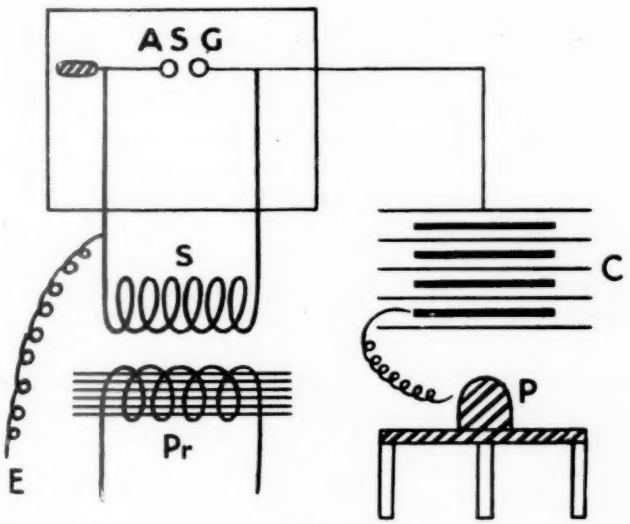


Fig. 2.

or a step-up high-tension transformer, from which the current is in the first case interrupted and in the second alternating. I do not therefore claim to be able to obtain true static current from a coil, but I do claim to be able to obtain all or nearly all, the effects of the "static modalities," or methods used for treatment by a current from a static machine, by using the unidirectional interrupted current of high potential and very little ampèrage supplied by means of this device from an induction coil suitably wound. There are six chief modalities used in treatment by the static current :—

(i) The static bath.
(ii) The static brush discharge.
(iii) Static sparks.
(iv) The high potential glass vacuum tube current.
(v) The static induced current.
(vi) The static wave current.

In the sixth and most useful, the *static wave current*, the negative is earthed, the alternate gap adjusted to spark and the positive led to the patient on the insulating stool. The strength of the current and its effect on the patient may be modified in several ways when my device with a coil is being used.

(1) By varying the current to the primary of the coil.

(2) By varying the length of sparks between the balls of the alternate spark gap—the longer the sparks the stronger the effect on the patient.

(3) By varying the size and number of the special series condenser resistances.

(4) By varying the size of the electrode in contact with the patient. The larger the electrode the greater the current, the less the patient feels and the less the muscular contractions, other things being equal.

With an ordinary static machine the rate of sparking is varied in one or two ways; with a coil, of course, it is chiefly, but not entirely, influenced by the rate of interruptions in the interrupter, but it can also be varied by the adjustment of the spark gap and by the size of the discharging balls and by the size of the series condensers.

I have noticed, of course, what is well known, that small muscles, such as those of the hand and forearm, will respond to much more rapid stimulations than will the larger muscles, such as those of the thigh or the trunk; it is therefore necessary to run the interrupter and set everything else so as to make the rate of stimulations quite slow when treating the larger muscles. This with a direct current is most easily done by employing a dipper interrupter and running it slowly. When working with the alternating current, however, the rate of interruptions is governed by the periodicity of the alternating current in the main, and this is much too fast for all but the small muscles.

The best method both with the direct and with the alternating current is to interrupt the secondary current between the series condenser and the patient by means of a "Kater" pendulum, and swinging this so as to give from one to two interruptions per second, and with this I found the large muscles contract nicely. The strength of these contractions can

also be increased by partially earthing another part of the patient by a well known device, preferably under the patient's own control. With a static machine proper one can vary the current causing contractions by placing the main conductors close together so that sparks pass rapidly, and cause rapid contractions, or farther apart, to produce slow contractions. Also by using some surging device, such as the ingenious small one of Dr. Turrell's, one can obtain slow and rhythmic contractions of the muscles. With my device I can very easily get the same double effect. By running the interrupter fast, say up to 3,000 a minute, I get a torrent of sparks, and the Morton wave current then causes contractions at the rate of 50 a second, I believe, in the fibres and cells, of not only the muscles, but of the nerves and all the tissues within the area under treatment. Then by means of the "Kater" pendulum I get slow contractions of the whole muscle at whatever rate I desire, from four times to once a second or slower still. I find twice a second the most useful for any but the largest muscles. I am inclined to believe that the combination of fast contractions of the fibres and cells of all the tissues, and the slow contractions of the whole muscles is better, and has a better therapeutic effect in many cases, when the Morton wave current is obtained from a coil in the way I have just described, even than when obtained from a good static machine.

As to results of treatment, I will not trouble you with a recital of many cases, but will select two of my early cases as examples.

Case I.—A young married lady who, besides managing her household had worked very hard in a Government office where her work necessitated turning over papers by the hundred thousand. She contracted occupation neuritis, in the left arm and shoulder, with great pain in shoulder and upper arm and some pain and decided numbness in the radial side of forearm and hand. The pain caused by turning over even the pages of a book was so intense that despite various treatments by radiant heat and drugs, &c., she had to give up her work, but even then obtained no relief. In October I gave her four combined treatments, i.e., with heat from a 3,000 candle-power "half-watt" lamp which I had tried alone for her before, and now with the Morton wave current applied to her shoulder by a metallic gauze electrode and by a large metal electrode held in the hands. After the fourth treatment, which was rather a long and severe one, she had worse pain the next day, but the day after that it quite disappeared and has not since returned in her shoulder or upper arm, and she could turn over papers with comfort. She did not get rid of the numbness in her forearm and after two months she had a slight return of pain in her forearm and elbow. I therefore applied the Morton wave current again to hand and elbow, with a large metal handle, by an electrode on the forearm and

by a vacuum electrode and she has been completely relieved and now the muscles in her left forearm, which had become wasted, have grown, and are as large as in her right or good forearm.

Case II is that of a well-known lawn-tennis player, who was suffering from "tennis elbow" and had so much pain at the external epicondyle of his right humerus that he feared he would be unable to continue his play. Two treatments with radiant heat and Morton wave current, for twenty minutes each, entirely relieved his pain and enabled him to go through the tournament successfully.

I have found this current especially useful in these, so-called, occupation neuroses. It is very useful also in lumbago and sciatica, infantile paralysis, synovitis and sprains, myasthenia, all forms of muscular weakness and want of development, in neurasthenia, in simple prostatitis, and of course in the after-treatment of many war injuries.

I do not claim that this device of obtaining static modalities from a coil is as good as the true static current from a really large and efficient static machine—except the Morton wave current, in which I believe it is at least quite as good, but I believe it will prove to be more effective than the current from the under-powered static machines often used. Anyhow, I feel sure it will bring these static modalities within the reach of many medical men and therefore of patients who would otherwise have to do without this valuable aid. I foresee the following advantages of this device over ordinary static machines:—

(1) Economy.

(2) Ease of control.

(3) Portability.

There is no reason why quite a small light, specially wound, coil should not be made sufficiently powerful and yet workable from any lamp socket in a private house or hospital ward.

(4) Reliability.

This is most important, for a coil will work well under any weather conditions.

The apparatus that I myself use, and show you to-night, is one made by myself, and chiefly consists of pieces of apparatus that I already possessed. The spark gap, however, is especially made by Mr. A. E. Dean, as is also a convenient condenser resistance which I have here. The vacuum electrode which I shall use was lent by the Medical Supply Association.

Section of Electro-Therapeutics.

President—Dr. A. E. Barclay.

The Leucocytic Blood-Content of those Handling Radium for Therapeutic Purposes.[1]

By J. C. Mottram, M.B., and J. R. Clarke.

(From the Research Department, Radium Institute, London.)

These radium workers are subject to widely different amounts of irradiation according as to whether their employment necessitates their being near to or distant from the radium, and whether for long or shorts periods of time. Among the workers under observation two classes were especially subject to exposure: (A) Laboratory workers who prepare and measure applicators containing emanation and radium; and (B) clinical workers who attach various screens to the radium applicators and subsequently apply them to patients. The remaining workers receive less exposure, down to the servants of the institution, who are only subject to very small quantities of the γ-radiation which pervades the whole building.

The leucocytic blood content of all these individuals (twenty in number) is shown diagrammatically in fig. 1, in which a comparison is made with thirty-eight normal individuals. (The normal figures were obtained from a paper on the effect of solar dermatitis on the leucocytes of the blood [1].) The diagram shows that the polymorphonuclear leucocytic and the lymphocytic blood content of the radium workers is decidedly lower than that of the normals. In the case of the polymorphonuclear leucocytes, there are eleven individuals below the lowest normal, and in the case of the lymphocytes three below the lowest

[1] At a meeting of the Section, held December 19, 1919.

normal. The lowest figures are: For polymorphs, 522 and 1,035 per cubic millimetre, and for lymphocytes, 706 and 855 per cubic millimetre. The classes A and B are indicated in the diagram, and are seen to form the large majority of the more affected workers. The exceptionally high A in the polymorph. chart showed subsequently much lower readings of 2,048 and 2,343. Similar, but less marked, blood changes have been described in X-ray workers [2], and a leucopænia has been produced experimentally in animals with X-rays by a number of observers [3], [4].

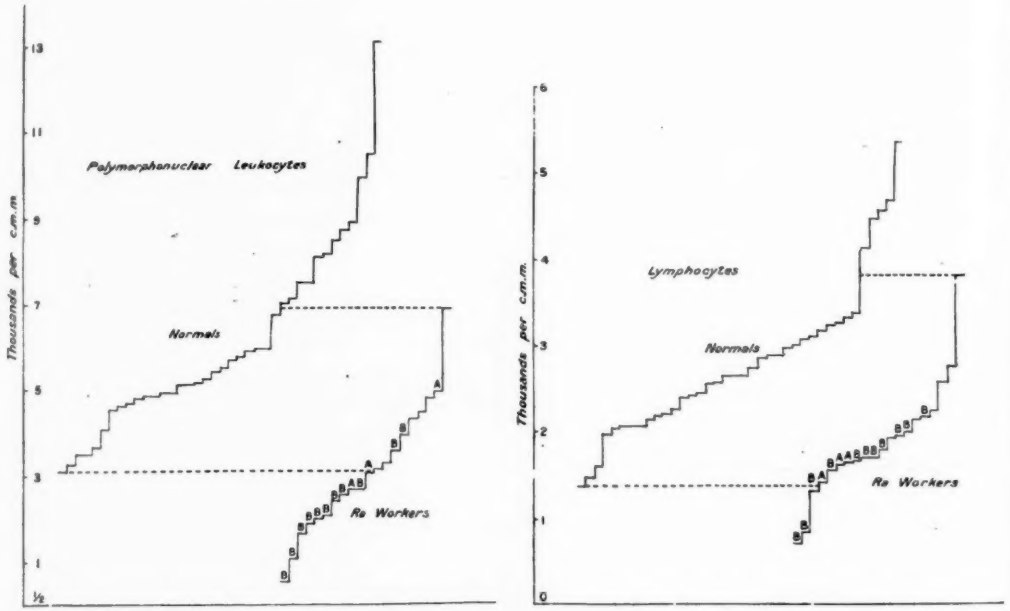


FIG. 1.

Each step in the diagrams represents a single individual. *A*, laboratory worker; *B*, clinical worker.

Attention is drawn to the absence of any evidence of a leucocytosis even among the less irradiated workers, the highest counts falling near the middle of the normal series. This indicates that a daily dose of irradiation, even when very small, will not produce a leucocytosis: whereas it is known that in small animals alternate periods of X-radiation and freedom from irradiation will, under certain con-

ditions, produce a profound leucocytosis [5], and that the leucopænia following a single dose of X-rays is often followed by a leucocytosis [6].

The leucopænia of radium workers, classes A and B, manifests itself after a few weeks' exposure—for instance, a class A individual showed a fall from 7,283 to 3,077 polymorphs, and from 4,544 to 1,612 lymphocytes after one month : the corresponding figures for a class B worker were 4,448 to 2,398 and 1,824 to 1,690 after two weeks. The effect of a holiday of two months is well seen in the following case :—

	Before holiday	After holiday	25 days later
Polymorphonuclears	522 ...	1,909 ...	1,063
Lymphocytes	885 ...	2,158 ...	857

On the other hand in several cases a holiday of two months was followed by a fall instead of a rise. It is thus clear that the onset of the fall is rapid, and the recovery by comparison slow.

No association between this leucopænia and any condition of ill-health has been definitely recorded. Whitlows of the fingers and other infective conditions of the hands are of common occurrence in radium workers ; this is, however, probably due to local changes.

An attempt has been made to estimate the daily amount of irradiation which workers of class B receive. Electroscopic measurements were found to be impracticable, as the leak was so great that the instrument could not be charged in the room where the attachment of screens to the radium applicators was carried out. A photographic plate was, therefore, made to occupy the place of a worker. It was screened with varying thicknesses of aluminium from 0·2 mm. to 2 mm. The shadows which resulted on development were of nearly equal density, showing that γ-radiation was chiefly affecting the plate. A control plate was exposed to the γ-radiation (3 mm. lead and 0·2 mm. aluminium screens) from an applicator having 9 mgr. of $RaBr_2 2H_2O$ per square centimetre for varying lengths of time. The two plates were developed together, and it was found that one minute exposure to the applicator produced a shadow similar to that on the experimental plate. This may be conveniently expressed in biological X-ray units, Rads [7].

This unit is an exposure to the β- and γ-rays from 2·75 mgr. of $RaBr_2 2H_2O$ per square centimetre for one hour; this dose is just sufficient to prevent the growth of rat sarcoma and to produce an

erythema when applied to the human skin [8]. Assuming that the radiant energy of β- plus γ-rays is to γ-rays as 50 to 1, then class B workers receive $\frac{1}{3000}$ of a rad daily per square centimetre. This small amount is, however, incident over the entire front surface of the body, and, as a generalized condition is under consideration, an attempt is made to estimate the total amount of incident radiation and to compare this with the amount received by a patient undergoing treatment for cancer of the breast.

The intensity of the γ-radiation from a radium applicator at a distance r therefrom is proportional to $\frac{e^{-\lambda r}}{r^2}$ where λ is the coefficient

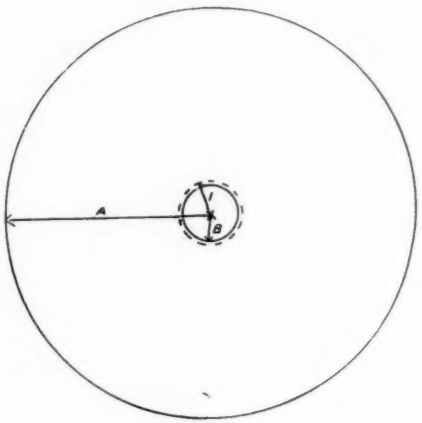


FIG. 2.

of absorption of the medium traversed by the rays. In the case of a patient undergoing treatment the radium is concentrated over a small area (of the order of 2 sq. cm.) situated about 2 mm. from the skin. The radiation received by a small area δA, distance r from the centre of the applicator or group of applicators, is thus equal to $I_0 \frac{e^{-\lambda r}}{r^2} \delta A$; I_0 being the intensity of the radiation at a point on a circle unit distance from the centre, and λ the coefficient for absorption of the γ-rays by air.

The integration of this quantity over the irregular surface of the body of the patient is very difficult. It was found, however, that a full

strength applicator, 2 sq. cm. in area, prepared as for a carcinoma case, when laid at one end of a photographic film for twelve hours, had no effect 1 ft. away. A case of this type usually receives about thirty hours' treatment, and hence the radiation received by the remote parts of the body can be neglected. The integration, therefore, was simplified by considering the body to be a cylinder with its axis horizontal, the front elevation being a circle.

It is assumed that the applicator is placed at the centre of this circle, which is of radius a, fig. 2, and that it is itself circular, of radius b, fig. 2; b being small compared with a. Measure a and b in such units that $a > b$, but b is very nearly equal to unity. Then the intensity of radiation, received by the body at unit distance from the centre, is equal to the strength of the applicator, to a degree of approximation sufficient for our purposes. Let this be I_o.

The radiation received by an annulus, thickness δr at a distance r from the centre, is—

$$I_o, \frac{e^{-\lambda r}}{r^2}, 2\pi r, \delta r.$$

Thus the total radiation received is—

$$R = \int_1^a I_o, \frac{e^{-\lambda r}}{r^2}, 2\pi r, \delta r,$$

+ the radiation received by the circle of unit radius.

But, we have supposed the intensity of the radiation over a circle of unit radius to be constant and equal to I_o.

$$\therefore R = 2\pi I_o \int_1^a \frac{e^{-\lambda r}}{r} \delta r + \pi I_o.$$

$$\text{Let } P = \int_1^a \frac{e^{-\lambda r}}{r} \delta r = \int_1^a \frac{1}{r} \left[1 - \lambda r + \frac{\lambda^2 r^2}{2!} - \frac{\lambda^3 r^3}{3!} + \dots \right] \delta r$$

$$= \int_1^a \left[\frac{1}{r} - \lambda + \frac{\lambda^2}{2!} r - \frac{\lambda^3}{3!} r^2 + \dots \right] \delta r$$

$$= \left[\log a - \lambda r + \frac{\lambda^2 r^2}{2, 2!} - \frac{\lambda^3}{3!} \frac{r^3}{3} + \dots \right]_1^a$$

$$= \log a - \lambda(a-1) + \frac{\lambda^2}{2, 2!} (a^2-1) - \dots$$

Now $\lambda = 6 \times 10^{-5}$ (cm.$^{-1}$ (9); so λr is of the order 10^{-3}, and powers higher than the first can be neglected.

$\therefore P = \log a - \lambda(a-1)$,

Hence $R = 2\pi I_o [\log a - \lambda(a-1)] + \pi I_o$.

Taking now, as the case of a typical radium treatment for cancer of the breast, a set of applicators grouped over a circle of 3 cm. radius, totalling 200 mgr., and left on for thirty hours, screened by 2 mm. of lead. In this case the "dose" is 6,000 milligram-hours.

Measure a and b in units of 3 cm.

then $b = 1$, $a = 10$,

λ in this case = about $1{\cdot}8 \times 10^{-4}$ (cm.)$^{-1}$

$I_o = 200$,

$$\therefore \quad R = 2\pi 200 (\log_e 10 - 9 \times 1{\cdot}8 \times 10^{-4}) + 200\pi,$$

$$= \pi 200 (4{\cdot}6050 - 0{\cdot}0032 + 1).$$

$$= 3413{\cdot}5.$$

$\therefore$ Total radiation is measurable as 102400.

The area exposed by a class B worker, assuming the body cylindrical as before, of the same radius, and 20 cm. thick, is 3,000 sq. cm. From the results given above a total radiation measurable as 1,413·72 would be received per day. In other words class B workers receive daily about 1·4 per cent. of the total radiation received by a patient during a course of treatment for carcinoma. It follows, therefore, that every ten weeks a class B worker receives the same quantity of radiation as a patient undergoing treatment for cancer of the breast. It is clear, however, that whereas in the case of the patient the radiation is concentrated over a small area, in the case of the class B worker the radiation is spread over the whole body surface.

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DISCUSSION.

The PRESIDENT: We have had a most interesting-paper, one which must make those of us who work in radiations think seriously of the dangers to which we are exposed and which we are very apt to overlook in the interest of the work we are doing. We shall probably go away and have our own blood examined.

Dr. RUSS: I think it a great pity that the lateness of the hour will prevent many remarks on Dr. Mottram's paper, the brevity of which is out of proportion to its importance. Some of us were adjudged to be alarmists when we raised our voice as to the necessity of protecting people whose business it is to deal with radiations, from their possible damaging effects, which are not visible to the naked eye. Dr. Mottram remarked on the experiments which have been made on rodents with the view to the production of leucocytosis, and he remarked that that did not occur in human beings under daily radiation. But there is one experimental point which Dr. Mottram does not remember at the moment, and that is that to get the leucocytosis in the rodents they must not be irradiated every day; only after an interval between irradiations is there an increase in the number of leucocytes, and that may be a point of importance. These observations are, it seems to me, rounded off with a completeness which is very satisfying to the most critical mind, and I think we ought to congratulate Mr. Clarke for having so tackled the mathematical part of the work; because it is one thing to expose a photographic plate and compare tints with a definite quantity of radiation, and quite another to begin integrating that quantity over the surface of the body. And when the result of that is found and compared with the total dose administered, there will be only one opinion as to the value of the mathematical treatment in such a case.

Dr. FINZI: This paper is of a very startling nature, and gives radium workers very seriously to think. I would like to ask Dr. Mottram one question: whether his workers whom he examined are exposed to the emanations at all; it might make a difference in the results. And I ask him whether he has examined the leucocyte count in a patient who has been exposed to one dose of radium over a small area, such as in treating rodent ulcer, and if so, whether he found an effect on the leucocyte count. What has struck me about these workers is the extraordinarily large dose of radiations they seem to receive over the whole body; for an amount of radiation which would be applied as one dose to a breast carcinoma to be received by them in ten weeks seems to be rather large. Possibly they are working with very large quantities. I can understand laboratory workers being exposed to these large amounts. But I cannot understand how clinical workers are.

Dr. MOTTRAM (in reply): The clinical workers receive the majority of their exposure during "making up," which consists in applying screens to the various applicators. About $2\frac{1}{2}$ grm. of radium is distributed in the applicators, so that these workers come in contact with large quantities of radium. As regards the effect on the blood of a single application of radium, such as for rodent ulcer, I have not examined cases of rodent ulcer, but with carcinoma of the breast there is a definite fall in the leucocyte count. I am not in a position to say positively how long the fall persists, and I do not know that any work has been done on this; it is a matter for investigation. The clinical workers do not come in contact with emanation to the same extent as the laboratory workers who make the applicators, and who are less affected than the clinical workers, possibly because they are skilled and know how to protect themselves, and also because they use radium in a less active state.

Section of Electro-Therapeutics.

President—Dr. A. E. Barclay.

The Treatment of Muscles by Artificial Stimulation.[1]

By G. Cooper, B.A., M.D., Major R.A.M.C. (T.C.).

The subject chosen for the title of this paper is one which I feel is of general interest to all medical men who have had under their care the treatment of patients injured in the war. Practically every wound has resulted in an injury of some degree to one or more muscle groups, and long after inflammation has subsided and healing has taken place there remain a disability and loss of function in the affected limb which are to a large extent due to pathological changes in the muscles. The treatment of such cases is generally handed over to the department of physio-therapy for treatment by massage and electricity, and too often such treatment is carried out in a mechanical manner without proper knowledge of the rationale of the methods to be adopted for the restoration of the muscle to its normal condition and function.

The injuries sustained by such muscles may be classified according to their ætiology under two heads :—

(A) (1) Injury of the muscle due to direct trauma. (2) Injury due to the action of toxins on the muscle fibre.

(B) Injury due to suspension of the function of the muscle.

Trauma may result in the loss of muscle substance and its replacement by fibrous tissue, or in an injury to its nerve supply. Such loss generally results in a state of fibrous contracture or in a loss of contractile power if the nerve-supply is involved. Toxins destroy the protoplasm of the cells, fibroblasts develop, and fibrous tissue replaces the muscle fibres. The injury caused to muscles by these two factors,

[1] At a meeting of the Section, held November 21, 1919.

trauma and toxæmia, has been very considerable, but it is insignificant when compared with the widespread injury that has been brought about by interference with the normal functions of muscles and their consequent atrophy. A single muscle may be directly affected by trauma, but this injury may interfere with the normal action of all the other muscles of the limb for a considerable period and thus give rise to serious pathological changes in the musculature of the limb.

As we have seen, the type of muscular atrophy known as disuse atrophy is of widespread distribution, and if we exclude the dystrophies and also injuries due to toxins and to direct trauma, it is reasonable to classify all atrophied muscles under one of the following three heads, according to the factors that have brought about their loss of function. (1) Muscles that have lost the power of voluntary contraction through lesions of the lower motor neurone (these have been largely caused by injuries of the peripheral nerves). (2) Muscles the normal contraction of which has been suspended by immobilization of the neighbouring joints—a condition commonly seen in limbs to which splints have been applied. (3) Muscles, the contraction of which has been reflexly inhibited because their action causes pain in the joints whose movement they control, as, for example, wasting of the quadriceps in arthritis of the knee.

The dictum laid down by Abernethy that diseased parts should be kept at rest is in accordance with the principles of nature, and it is obviously necessary to immobilize joints to secure proper fixation of the fragments of a fractured bone or to limit the spread of acute inflammation; but the muscles and the fibrous structures of the limb concerned have frequently been involved in the most disastrous results from the application of this principle of rest. Muscles have wasted or have undergone fibrous degeneration, and the movements of the joints have been restricted by contraction of the adjacent fibrous tissues.

It is an interesting observation to note that the degree of atrophy of the tissues that follows suspension of function in the locomotor apparatus has no parallel in any of the other systems of the body, where temporary suspension of function to the same extent is not possible.

The Cause of Muscular Atrophy and its Prevention and Cure.

Some reference to the physiology of muscle is necessary in order to attempt to explain the cause of atrophy and to justify the methods adopted both for prevention and cure of muscular degeneration. A muscle

fibre contracts either in response to a nerve impulse along the normal channels, or it may respond to direct stimulation in virtue of its irritability. Muscles will contract feebly when pinched, and certain chemicals will provoke a response when directly applied to the exposed muscles. So far as can be ascertained there is no difference in the nature of contraction or in the chemical changes produced by a normal nerve impulse and in that brought about by electrical or other stimulation. The irritability of a muscle fibre would appear to be somewhat akin to the irritability of the amœba, which possesses to a feeble extent the power of response to direct stimulation, and the muscle cell might be regarded as a highly specialized cell of the amœba type, specially developed for contractile purposes and peculiarly responsive to stimuli of a nervous type. Curari, as is well known, places a block at the nerve ending, and stimuli applied to a motor nerve so affected are powerless to produce contraction of a muscle. Stimuli applied directly to the muscle can, however, bring about a contraction. Muscle contraction is therefore a response to an external stimulus, and the questions of loss of contractile power and of change in the muscle substance, are closely associated with one another and with the causes operating to bring about loss of function in the muscle fibre. The health and condition of any cell is known to be closely associated with its function; this would appear to be especially true with regard to muscle tissue, the atrophy that takes place being a direct result of its suspension of function. The nutrition of a muscle fibre would appear to be determined by the exchange that takes place between the muscle plasma and the surrounding lymph during the acts of contraction and relaxation, the interchange thus being dependent on its exercise of function.

In response to an external stimulus certain very complex changes take place in a muscle fibre. During contraction lactic acid is formed, but there is no consumption of O_2 or evolution of CO_2 at this stage. The process of *relaxation* is, however, accompanied by an oxidation of certain carbohydrates; oxygen is consumed and CO_2 produced. The lactic acid disappears but not by oxidation; it appears to play a very important part in the contractile process, but the nature of its action is not definitely understood. The actual contraction and subsequent relaxation are very difficult to explain. As a metal rod lengthens when heated and shortens again on cooling, because different degrees of temperature alter the equilibrium of its atoms and produce a reaction of its elastic forces, so the molecular arrangement differs in the muscle according as it is at rest or excited, and its external form differs accord-

ingly. The active muscle is short and thick, the inactive long and thin, and in suddenly passing from one state to the other the muscle contracts or expands, not against an inherent elasticity, but by an elastic action, in order to assume the natural form of *equilibrium* which corresponds to its active or inactive state (Weber).

The carbohydrates demanded by the process of oxidation are undoubtedly drawn from the surrounding lymph, through the limiting membrane of the muscle fibrils, and the expulsion of CO_2 and other metabolites must take place through the same channel.

Certain functional states of the cells are known to be accompanied by increased permeability of the cell membrane, and there is conclusive evidence that the cell membrane is rendered more permeable during excitation of the cell. McClendon found in excited muscle an increase of electrical conductivity—an index of increased permeability to ions. This means that the cell membrane becomes permeable to ions to which it was impermeable while unexcited—in other words, it ceases to be polarized. Siebeck's discovery that potassium chloride enters more rapidly into excited muscles than into resting ones also indicates increased permeability during functional activity. Conversely, Lillie has shown that all agents which cause increased permeability of the cell membrane act in an exciting manner on the cell.

The essential characteristic of life is incessant change, and the maintenance of health and condition of the muscle fibre appears to be closely associated with the acts of contraction and relaxation, interchange with the surrounding lymph taking place during these acts. It is therefore a reasonable line of treatment for muscles which have undergone atrophy to seek to bring about a restoration of their condition by *provoking contractions in the affected fibres.* These may, therefore, be either in response to normal nervous stimuli or be evoked by artificial stimulation.

The restoration of muscles by active contraction can only be referred to briefly in this paper. The development of muscles by exercise would appear to be due to at least two factors: (1) Increase of size and tone of muscle fibres taking part. (2) The "calling up" of a large number of muscle fibres of an embryonic or of a dormant type, of which there is a considerable proportion in every muscle. The observation has been made that muscles are overwired (Souttar)—that is, that the nerve trunk contains many fibres which do not appear to function—support for which view is obtained from the fact that we have so often on exploration of a nerve been surprised to find that a very ex-

tensive lesion of the nerve has frequently not been attended with any marked interference with the function of the muscles supplied.

This theory of overwiring is probably true, the explanation possibly being that such nerve fibres and their corresponding muscle fibres are embryonic or dormant. That such dormant fibres can be "called up" and B 3's drilled into A 1's is a matter of ancient observation, and systematic and periodic stimulation of the neuro-muscular apparatus, whether by voluntary effort as in gymnastic training, or by the artificial means employed in physio-therapy, is definitely known to result in an increase in size and tone of the muscles so treated.

Treatment—Preventive and Restorative.

Treatment may be regarded as preventive or restorative, according to the stage of the injury. The preventive treatment of atrophy of the tissues of an injured limb is necessarily a difficult problem, and must be largely circumscribed by the nature of the injury: but even when the exigencies of surgical treatment have been fully considered, it is deplorable to find so many patients sent at a late stage to the department of physio-therapy; the wholesale atrophy of their tissues bears eloquent testimony to the fact that the suspension of the functional activity of every tissue of the limb was considered a *sine quâ non* of surgical treatment. Fortunately, there were many surgeons who treated the injured limb as well as the injury itself, and even under the most difficult circumstances managed to secure sufficient functional activity to prevent the atrophy that attends absolute disuse.

There is no doubt that too much attention has been given to restorative treatment as compared with preventive treatment, and the surgeon who has conquered sepsis and secured a perfect alignment has but little reason to be proud of the result if the joints of the limb are immovable, and the muscles hopelessly atrophied. For the atrophy of such muscles is frequently hopeless, and in many cases has proceeded so far that a large proportion of the muscle cells have perished and have been replaced by fibrous tissue. No amount of subsequent stimulation can restore such a muscle, and "permanent disability" too often marks the measure of the surgeon's skill.

For the treatment of muscle by artificial stimulation two methods are available: stimulation by massage and stimulation by electricity.

Massage.

Bearing in mind that our object is to produce contraction in muscle fibres with a view to determining exchange between the fibre and the surrounding lymph it does not appear that in massage we have an agency of any considerable value in restoring the size and tone of a wasted muscle the response of which is limited to direct stimulation, as is the case in muscles suffering from reaction of degeneration. Where the nerve-supply is intact contractions in response to manipulations are largely the result of stimulation of the muscle fibre through the muscle plate, but where the nerve-supply is interrupted contractile response to manipulation must correspond to the irritability of the muscle fibres to direct stimulation, and in most cases this response to direct stimulation is very feeble. I am inclined to regard massage as effective to a large extent in such cases on account of its action on the vaso-motor system, and this result will be largely brought about by mechanical action, as the vaso-motor reflexes are also affected in peripheral nerve lesions.

Contraction produced by Means of Electrical Stimulation.

In electricity we have an agency which is peculiarly effective in producing contractions of muscle fibres, and, as is well known, by utilizing this form of energy we can obtain the most complete contractions of individual muscles or muscle groups.

For the production of contraction in muscles, we can use either the faradic or the interrupted galvanic current. The stimulation caused by the electrical current is due to the displacement of the ions in the nerve or muscle, and the more sudden the displacement the more stimulating is the effect. A constant current is therefore incapable of producing contractions in muscles, and it is only at the make and break of the current that stimulation takes place. It has been suggested that muscles probably contain two kinds of contractile material, the striated portion and the sarcoplasm. The striated portion responds to brief stimuli and contracts rapidly, the sarcoplasm responds to longer stimuli only and contracts slowly. A sluggish contraction would represent a contraction of the sarcoplasm. Muscles suffering from reaction of degeneration, as is well known, will not react to the currents of brief duration set up in the ordinary induction coil or faradic battery. They will, however, react to the interrupted galvanic current, and muscles suffering from reaction of degeneration would there-

fore appear to have sustained a loss of striated elements with survival of the sarcoplasm alone. This appears to have been borne out by the fact that when reaction of degeneration is fully established, the character of the contraction becomes sluggish. For the treatment of muscles suffering from reaction of degeneration the galvanic current alone is effective. The faradic current is ineffective unless its voltage is enormously increased, in which case the intensity would be great, and the excitation very painful. It is thus ineffective as a practical method of producing contractions. It is necessary to remember that several factors are concerned in the production of contractions in muscle by electrical stimulation; the current must have a minimum of intensity, and this minimum current, in order to be effective, must last for a definite time, this representing the velocity of excitability of the muscle. In normal muscles this is about one-thousandth part of a second; in paralysed muscles velocity of excitability is much slower, and a current may have to act for one-twenty-fifth of a second to produce contraction in a degenerate muscle. Hence the ordinary faradic current is ineffective for producing contractions in paralysed muscles. The practical problem in the treatment of muscles by electrical stimulation would appear to consist in the selection of that particular type of current that will give the best contractile response.

Considerations in the Selection of Appropriate Currents.

The selection of appropriate current is governed by two considerations: (A) The degree of the contractions produced. (B) The degree of pain caused by the current.

(A) Under the head of "degree of contraction" we are limited to the use of currents of a definite duration in the case of degenerate muscles, but in all other muscles we can stimulate normal contractions best by the use of rapidly interrupted currents. The normal voluntary contraction is a tetanus—the effect of rapidly succeeding stimulations, and this continues as long as the stimuli act upon the muscle. Various attempts have been made to estimate the rate of stimulation in normal voluntary contractions. Helmholtz, by analysing the sound of contracting masseter muscles, came to the conclusion that the rate of stimulation was 35 per second. Contradictory results were obtained by other workers, but the present opinion of physiologists is that nervous stimuli pass into a contracting muscle at a rate that varies between 20 stimuli per second as a minimum, and 35 stimuli per second as a maximum.

The production of tetanus by artificial stimuli, however, gives a very much wider range of stimulation. A rate of 20 shocks per second has been found to be the minimum required to give tetanus. The upper limit that will produce frequency tetanus has not yet been ascertained, but tetanus has been obtained with up to 20,000 induction shocks per second (Kronecker). It is a point of considerable interest whether we are right in assuming that the normal rate at which nerve impulses pass into a contracting muscle is really about 40 per second. The sound that is heard in contracting muscle is a low composite note of approximately 40 per second, but this note does not necessarily represent the rate at which nervous impulses are passing into the muscle, and it is but slender evidence on which to base an estimate of the normal impulse rate. If 40 is approximately the rate of normal stimulation we should not expect to find muscle responding to an artificial stimulation of a much higher frequency. It is doubtful if Helmholtz's estimation is correct, and the fact that with improved apparatus much more complete and effective contractions can be obtained at a high rate of stimulation would favour the view that the normal rate of stimulation is much greater than has been supposed.

(B) Sensation appears to be influenced by two factors: (1) the the length of the waves employed; (2) the uniformity of the interruptions. The effect of a current on sensation is very much influenced by the length of the waves, the stimulation of the sensory nerves being due to the movement of ions. Such movement is relatively small with short waves, but with longer waves there is a greater excursion of ions, and consequently the production of a considerable degree of pain. Sensation is also to a very large extent influenced by the uniformity of the interruptions, both as regards their rate and their character. The interrupter in the old type of faradic battery was very irregular in its action, and it gave rise to currents of uneven length and varying voltage. In 1915 Mr. Dean placed a new type of faradic coil on the market—the Wilson coil. In this coil the type of interrupter employed was an adjustable steel spring which gave an interruption of 200 to 300 per second in the primary. These interruptions were much more uniform in type than any that have hitherto been used in faradic coils, and there was consequently much less variation in the induced current, the waves were shorter, and the type of current was much less painful in its action than any that had hitherto been obtained with any other coils. Experimenting on the same lines, I designed with Mr. Dean a new type of telephone interrupter for induction

apparatus, which as at present constructed will give an interruption in the primary coil of 800 to 900 per second, and the current obtained from this coil will be found to be the least painful and the most effective yet produced. Interrupted galvanic currents, where the rate of interruption exceeds 20 per second, can also be used for the stimulation of non-degenerate muscles, and of late this interruption has been done principally by the method introduced by Frimandeau.

The Frimandeau coil is best described as a primary coil, fitted at both ends of the iron core with vibrating hammers of similar type. The ordinary circuit may be described as the exciting current, and its passage causes the hammer to vibrate in the ordinary way. Frimandeau makes use of the alternate magnetization and demagnetization of the core to cause a synchronous movement of a similar hammer affixed at the opposite end of the core. A galvanic current is made to pass across the gap thus formed, and so we have a means of interrupting a constant current in a circuit, which may be described as the stimulating circuit. By employing various types of interrupters the rate of interruption can be varied, and I have succeeded in obtaining from two to 900 interruptions per second in the exciting circuit. The galvanic current so interrupted can be employed for the stimulation of muscles. When the rate of interruption falls below 20 per second single twitches are obtained in response to each stimulation. Above this rate tetanus is obtained from non-degenerate muscles, and the Frimandeau current has proved very useful in evoking effective contractions from such muscles.

For the production of tetanus with this type of current I find that an interruption of from 200 to 300 per second is the most effective. Above this rate the current appears to have the character of the ordinary constant current, and at very high speed of interruption no contraction whatever can be obtained. The nature of the interruptions at speeds of over 50 per second is most probably not a complete make and break, and the current is most probably of an undulating type, the resistance across the gap never being sufficient to interrupt completely the passage of the current. Various methods of applying the stimulating current are in use: metronomes for single make and break, and various types of apparatus which give rise to a current of a surging type.

The method most in favour, however, is the method of graduated contraction, the maximum number of fibres being engaged only at the crest of the stimulus; such a contraction approximates most closely to the normal type.

The Question of Fatigue.

The interesting question of fatigue remains to be considered. According to the work of Maggiora and Mosso, and of Treves, who have studied the question by means of ergograph tracings, it would appear that there are three different types of fatigue: (*a*) Fatigue of a personal type for each individual; (*b*) fatigue of the nerve itself; (*c*) fatigue of the muscle. Each individual has a personal type for his ergograph fatigue curve; it is constant even with wide variation of the external work. The onset of fatigue of a personal type is the most easily induced. Nerve, unlike the nerve centres, is, comparatively, inexhaustible, and shows no visible signs of fatigue even when thrown into a state of activity for several hours; this is due to the extremely slow metabolism of nerve tissue. When a motor nerve is excited the muscle apparently becomes fatigued long before the nerve.

Fatigue as regards muscles is purely a *toxic* phenomenon, and is due to the non-removal of lactic acid, which is produced during contraction and for the removal of which oxygen is absolutely essential (Fletcher and Hopkins, 1907). The experiments of Ranke showed that on circulating a saline solution containing no nutrient material through a fatigued muscle of a frog the fatigue disappeared; by further experiment he also demonstrated that the aqueous extract of fatigued frog's muscle circulated through the fresh muscle of another frog produced fatigue phenomena. In the first experiment oxygen was conveyed to a muscle which was charged with lactic acid, and in the second the toxic solution conveyed lactic acid to a resting muscle.

The rhythmic activity and comparative non-fatigability of heart and respiratory muscles is undoubtedly due to (1) the rich blood-supply, which provides a plentiful supply of oxygen and nutrient materials; (2) to the definite resting phase between each contraction. In experimental work it has been found that the slower the rhythm of contraction the longer the onset of fatigue is delayed; if a muscle is contracting slowly enough to allow of complete recovery between each contraction, it is comparatively inexhaustible. This recovery depends on a proper supply of nutrient to repair waste, and of oxygen to allow of the removal of lactic acid.

In the treatment of muscles by artificial stimulation there should be no danger of fatigue if the following precautions are observed: (1) Some preparation of the limb is necessery to ensure a good flow of blood, such as follows treatment by the whirlpool bath. (2) The limb should be kept warm during treatment, and in a position in which there is no

interference with the circulation. (3) Stimulation should be rhythmic in type, and there should be a definite interval to allow of a complete recovery between the contractions; this interval should be at least five seconds.

That artificial stimulation will bring about an improvement in the size and tone of a muscle group has been conclusively proved in practice by the actual measurement of degenerate muscles which, treated by artificial stimulation for some months after the injury, were found to have increased in size and improved in tone, although operation subsequently confirmed the diagnosis of complete interruption of their nerve supply.

SUMMARY.

(1) Excluding the muscular dystrophies and injury due to trauma and toxins, all muscular atrophy is of the disuse type—that is, it is caused by suspension of the normal function of contraction and relaxation.

(2) The condition and tone of a muscle are largely dependent on their function—interchange between the cell and surrounding lymph taking place during contraction and relaxation.

(3) Prevention of atrophy and restoration of atrophied muscles by rhythmic contractions constitute therefore a reasonable line of treatment.

(4) Artificial stimulation of muscle conserves the nervous energy of the patient, and in most cases of injury is the only method that can be employed.

(5) Of methods of artificial stimulation, electrical is the most valuable.

(6) Treatment by electrical stimulation is governed by the consideration of two factors: (*a*) The degree of contraction produced; (*b*) degree of pain caused by the stimulation.

(7) Pain is largely a matter of the length of waves employed and the uniformity of the interruptions.

(8) Apparatus devised to give a uniform type of interruption yields the best results.

(9) Fatigue is a toxic phenomenon due to accumulation of lactic acid. There is no risk of fatigue if a proper blood-supply is ensured and a short interval allowed between the contractions.

(10) Practical experience has demonstrated the value of artificial stimulation in restoring the condition of wasted muscles and in preventing atrophy.

(The Discussion was adjourned to the next meeting, December 19.)

Section of Electro-Therapeutics.

President—Dr. A. E. Barclay.

Adjourned Discussion on Major Cooper's paper "The Treatment of Muscles by Artificial Stimulation."[1]

Dr. E. P. Cumberbatch: I think we must all feel indebted to Major Cooper for having brought this subject forward, and it will be very interesting to hear the views of members on the treatment of paralyses, for the treatment of cases of paralysis following nerve injury is likely, for a long time to come, to form a large part of the work of electrical departments of hospitals. Hence a knowledge of the methods used and the results which are being obtained by those who work there will be of definite value. Major Cooper's paper would have been still more complete if he had presented an oscillographic record of the output of his coil. And since he makes definite claims for the coil we ought to be told how the current is applied, the frequency of its application, and other details. I have tried his coil, and I agree with him that the contraction it produces is painless. There must, of course, be a certain amount of discomfort experienced when muscles are strongly tetanized but there is not the stinging pain that some medical coils cause. Owing to Major Cooper's absence to-night I am unable to ask him one or two questions which I had intended to. I think some of us have tried the effect of stimulating muscles by means of frequent discharges of condensers of small capacity, by the method introduced by the late Dr. Lewis Jones. This is an even more agreeable form of stimulus and I wonder whether Major Cooper has tried it, in addition to the other methods he has experimented with. Major Cooper also makes the claim that his is the most effective form of treatment. We need to know how he applies his current, what electrodes he uses, and in what situation he applies them; also whether he uses them in water, or out of it, and also how he prevents the contraction of antagonizing muscles. Major Cooper also makes the statement, at the end of his paper, that "artificial stimulation will bring about an improvement in the size and tone of a muscle group has been conclusively proved in practice by the actual measurement of degenerated muscles which, treated by artificial stimulation for some months

[1] At a meeting of the Section, held December 19, 1919.

after the injury, were found to have increased in size and improved in tone, although operation subsequently confirmed the diagnosis of complete interruption of their nerve supply." Anybody who is able to do that is certainly to be congratulated on the result. I understand that the stimulus which Major Cooper uses is that from the secondary current of his coil. It is therefore of very brief duration; but how does he make a stimulus of such brief duration produce contraction of a muscle with an interrupted nerve supply and showing, presumably, R.D.? There are certain points which Major Cooper mentions with regard to the preliminary treatment of muscles before applying the electrical stimulation, and I agree with him that this preliminary treatment and preparation is necessary. The first thing is to give such preliminary treatment as will increase the blood supply; for example, by the whirlpool bath. The second necessity is to so place the limb that there is no strain on the muscle. It is also imperative to produce *rhythmic* contractions of muscles, alternating with relaxations, to prevent or reduce fatigue. With regard to the treatment of paralysed muscles, we have to remember that there are other parts requiring treatment besides the muscles; yet in most of the accounts we hear and read, the impression is gained that the treatment consists in nothing more than the production of contraction and relaxation of the muscle. But there is, in many cases, the chilblain circulation of the skin, and there are trophic sores which develop on the affected limb, and the inefficient blood supply to the affected member. All these matters should receive electrical treatment if we are to carry out a complete treatment of the paralysis. There are several methods which I have utilized from time to time for treatment, and I hope others will give their experiences on them too. One is by means of condenser discharges. Another is one on which Dr. Turrell may give his experience, viz., Lapicque's condenser method. I have, up to the present, utilized, most largely, the sinusoidal alternating current, which I apply to the limbs immersed in hot water. The limb is thus kept warm. The contraction of antagonizing muscles can be mechanically prevented. The sinusoidal is not an absolutely painless current, but that is not altogether a disadvantage in treating paralysis. In the treatment of paralysis it is my opinion that the muscles should also be stimulated *reflexly* by exciting the afferent nerve-endings in the skin, as well as by stimulating the muscles *directly*. I think the good results we have had at St. Bartholomew's Hospital during the last few years by the sinusoidal alternating current have been due to some extent to the stimulation of the peripheral nerve-endings in the skin. It is possible to make the muscles contract by stimulating the sensory nerves. I have recently demonstrated that by stimulating the posterior roots of the spinal cord it is possible to get contractions of muscles. The contraction, however, occurred on the opposite side. I would like to refer to a case in which voluntary power reappeared after a long period although no muscles were made to contract during electrical treatment. It was not a case of gunshot injury to nerve, but the paralysis was due to anterior poliomyelitis. This patient developed the condition when $1\frac{1}{2}$ years of

age, and the treatment prescribed was to keep the limb in a straight position by a splint, and do nothing further. There was no recovery in the limb for five years. She came to London, and was at first under the care of Dr. MacGregor who was acting for me at the time. He found that the region below the knee was completely paralysed. In the thigh there was very feeble voluntary power in one muscle. After the sinusoidal current had been applied below the knee, from leg to leg, for three months, she developed some voluntary power in the peronei. Then some voluntary power returned to the big toe, and then to the peroneus tertius. The current produced no contraction of the muscles. The explanation I have suggested is, that by stimulating the afferent side of the reflex arc the anterior horn cells and their axons were also stimulated. Whatever may be the explanation the fact remains, and in other cases, too, we have noted return of voluntary power where it had been long lost although the electrical treatment produced no contraction. When, however, muscles have not lost their normal reactions then the graduated exercise by faradic currents and their active contraction is the best form of treatment. Then there is the question of "disuse atrophy." Major Cooper mentions that the wasting of a muscle after injury or after suffering the interruption of its nerve, is entirely due to disuse. But there are certain experiments which seem to throw doubt upon that view. Professor Langley, in a communication to the *Lancet*,[1] refers to experimental division of nerves in animals and subsequent stimulation of the paralysed muscles for long or shorter periods, and yet atrophy was not prevented. Another point is, that if a muscle is thrown out of use by injury or disease of the upper motor neuron, the muscle does not waste until a long time has elapsed. If, on the other hand, the lesion is one of the lower neuron, wasting sets in early and is rapidly progressive. Major Cooper seems to place little value on massage for the treatment of paralysis. I hope Dr. Justina Wilson will give us her views on the utility of massage for this purpose. I hope I shall not be deemed too censorious in my remarks on Major Cooper's paper. I regard it as a very useful contribution, and it contains an instructive résumé on the physiology of the subject, which is too often neglected. I am anxious to hear what other workers in our Section have to say in regard to methods of treatment of limbs, which are paralysed as a result of nerve injury, and what results they have achieved.

Mr. W. R. BRISTOW: I did not have the advantage of hearing either the opening paper or Dr. Cumberbatch's remarks upon it, but the subject is one which has always particularly interested me. The treatment of muscle wasting by faradic stimulation is a subject which has formed, in our orthopædic centres during the war, a very large proportion of our electrical work, and one is glad it is so, because it seems to me to be a part of electro-therapy, which is absolutely rational. I would not like to tilt against one of the honorary secretaries by saying some other parts of the subject require more defence. It is difficult for

[1] *Lancet*, 1916, ii, p. 6.

some of us to understand that the static machine can do all the things which are claimed for it, and that the high-frequency current is quite such a panacea as some would have us believe. But in the faradic stimulation of muscles we have a method which is proved by its results, and for anyone not specially electrically inclined, has only to be seen to be believed. It is a method which, I think, cannot be replaced by massage, by remedial exercises, or by anything else in the early treatment of muscle-wasting, whether the muscle is the subject of atrophy from disuse or reflexly as a result of joint disease. It is almost hackneyed to refer to the case of the vastus internus in relationship to the knee-joint, but I think there is no method so good for improving the tone and promoting the recovery of the vastus when it wastes following injury of the knee. Those who are particularly interested in massage and exercises sometimes go out of their way to show us the way in which the vastus can be exercised while all the rest of the quadriceps is kept at rest. It is interesting but is not always easy to follow. Artificial stimulation of muscle, the electricity being used entirely as a stimulus, makes that a perfectly simple and easy procedure. Again, as I think Major Cooper points out, it is important to regard the electrical stimulus purely as a stimulus, to claim no benefits from the mere passage of a faradic current through a muscle. It is the same with regenerating nerve. In the early stages when you use an interrupted galvanic current after peripheral nerve injury, I do not think anyone seriously believes that the passage of the current up or down, as the case may be, has any particular effect either on the completeness or the rapidity of the regeneration of the axis cylinders. But the method described is that, *par excellence*, by which you can pick out and stimulate individual muscles and individual members of a muscle-group. In that lies its extreme importance. I quite agree with what Major Cooper says about massage. From this point of view—dealing with the regeneration of muscle—I do not think massage is the best means to adopt. When œdema is present, I agree that nothing can equal or replace massage; but when dealing with muscle wasting, either because of joint disease, or because of nerve injury, or because of trauma, then stimulation of muscle, with its attendant circulatory and chemical changes, can most effectively be produced by electrical stimulation, though massage and exercises can be used with effect later.

Dr. LEVICK: I endorse very strongly what Mr. Bristow has just said about the importance of making a muscle contract in treating wasted conditions caused by nerve injury and wasting from disuse. In these cases, I agree, nothing can replace electrical contraction of muscle. It is a pity it is so difficult to get this form of treatment carried out on a large scale. At present, most of the work which is being done in this direction is being carried out on pensioners who were injured in the war, and for them clinics are being established in out-of-the-way places as well as in London districts, and there are not enough specialists to supervise the work in all these centres, hence the carrying out of the work has to be largely entrusted to masseurs and masseuses.

These are given certificates of proficiency by the Incorporated Society of Trained Masseuses, whose certificates are relied on for efficient work in assigning the people concerned to their particular clinics. I have gone round to a considerable number of them now, and one finds that practically none of these masseuses have been instructed in this method. Some of them do not even know the names of the muscles in the limbs they have to work on, and do not know how to concentrate on, say, a median nerve injury. And even if the muscles are pointed out to them, they do not know how to procure the electrical contraction in them in a proper manner. I think it should be recognized that this method of treatment ought to be taught everywhere. At present many of these women hurry through an examination with a very inadequate syllabus, so they go out only half-taught: there are now hundreds of them performing this work which they cannot do properly. Major Cooper's paper has a valuable collection of information from various authorities. One matter to which he calls attention is the interchange of waste products and nourishment in the muscle fibre by means of the lymph, and he speaks of the products of metabolism in muscle being removed by the lymphatic stream. I do not think the lymphatics remove these products in any appreciable quantity, although it is generally so stated. The capillaries are in close contact with the sarcolemma, they ramify over the muscle fibres themselves; but the lymphatics do not pierce the muscle sheath. Consequently the blood stream is in close contact with the muscle fibre, and it is impossible that the sarcolactic acid could be removed fast enough by oozing from the muscle fibre in the lymph channels. And if you put septic matter in the middle of muscle, you do not get enlargement of lymphatic glands. And sarcoma does not spread by the lymphatic channels, but via the blood-vessels. I think this a very important matter: it comes largely into the question of the treatment of muscle. Major Cooper mentions that there are two forms of contractile substance in muscle. I know this view was advanced some time ago, but in normal muscle the response to the stimulus is always of the same length: there is the same distance between the elevations in the myographic tracing, though the height of the peaks will increase with the strength of the stimulus. The only exception to this is when the current is so strong that it takes some time for the surrounding tissues to return to their normal resting position.

Dr. JUSTINA WILSON: I have only just had this paragraph placed before me, and I have not had time to think out any sort of reply. Major Cooper's paper is most interesting and instructive, but I think he has "damned with faint praise" a branch of the profession which has done very useful work in the restoration and stimulation of muscle, and I must say a word about that. Massage, of which I claim to have had a greater experience than anyone else here, has a great place in the artificial stimulation of muscle, and I am always sorry to notice the scant courtesy it receives from many electro-therapeutists. Personally, I think electricity and massage are interdependent: you will never get the best results from electrical treatment until you bring massage to bear

upon it, just as you will not get the best results from massage and movements unless they are associated with electrical treatment. Many people do not seem to realize what massage is. It has three different branches, all of which have different effects, i.e.: (1) The actual manipulations; (2) passive movements; (3) active or voluntary movements. All of these play an important part in the restoration of muscle function. As regards the first, Major Cooper is evidently only acquainted with one of these manipulations when he states that massage is effective only by the amount of its action on the vasomotor system. His remarks apply only to light stroking, which acts reflexly. He entirely ignores one of the most important branches of massage, i.e., muscle kneading, which by its rhythmic and powerful squeezing effects, exercises a very important influence in removing waste products, and in promoting exchange between the muscle fibre and its lymphatic and blood-vessels. These passive manipulations are destined to play the same rôle as stimulation by the galvanic current, i.e., they bring about an increase in the general metabolism of the whole limb, and I agree with Dr. Cumberbatch when he speaks of the necessity for stimulation of all the various tissues of a limb. I think that is what massage does: it increases the blood supply, the warmth, the nutrition, the circulation of all tissues, not only of muscle. It does away with acrocyanosis and Raynaud conditions, which are so hampering to electrical treatment. Then comes the question of passive movements. In treating a limb we need to have actual movement of the joints themselves as well as treatment for the muscles, and nothing in electro-therapeutics can take the place of passive movements for the prevention of deformity, and for restoring the circulation of the joint. Active or voluntary movements directly exercise the affected muscles and not only promote active contraction, but if done with the intelligent co-operation of the patient, they also have an effect on the nervous system and on co-ordination. With regard to Dr. Levick's scathing remarks; in the absence of any of its members, I must stand up for the Incorporated Society of Trained Masseuses, and I would remind Dr. Levick that there are schools and schools, and that all training is not bad nor superficial. I am afraid he must have been singularly unfortunate in his experience of masseuses, for I cannot say that I have had a similar experience. These girls are taught and examined by medical men and women. And I ask Dr. Cumberbatch, who is one of the examiners, to support me in that as regards the higher examinations at least, they are thoroughly well taught. They are not ignorant of the use of electro-therapeutic methods nor of the names and actions of the muscles or their nerve supplies. The Incorporated Society has done its best to raise the massage profession from the neglected state to which the apathy of the medical profession had relegated it. The Society has been greatly handicapped by the war, and by the necessity for rapidly turning out vast numbers of workers with short trainings of only six months. They have now set themselves to insist on a thoroughly good training, and all candidates have, from January, 1920, to give evidence of a full year's training in anatomy, physiology, massage, Swedish movements, and so on.

Dr. ALASTAIR MACGREGOR: Major Cooper's paper is an excellent résumé of the physiology of normal muscle and the pathology and treatment of degenerated muscle. With certain reservations, most of us will agree with the author on his various points. In former days fractures, especially those near joints, were kept up too long. This fact was pointed out in 1882—thirteen years before the discovery of X-rays—by Mr. (afterwards Professor) John Chiene, of Edinburgh, who said that cases of Colles's fracture, for instance, were kept immobile far too long, and that the wrist-joint should be moved sooner than was usually done. Sir William Bennett and Professor Lucas-Championnière afterwards preached the same gospel. Major Cooper omitted to mention that during the Great War there was an army order issued in France that no compound fracture near a joint was to be moved until a certain time after all suppuration had ceased. The cases of open wounds and fractures "over there" differed widely from similar cases seen in civil practice. In France and Flanders, when they came under treatment, the cases were mostly already septic; owing to the barrage the wounded could not be got in as soon as was surgically desirable. Hence the first consideration was to save the patient's life, the second was to save his limb from amputation, and the third was to try to get the best result from treatment of the disabled limb. I believe that some disastrous results followed early movement in such septic cases, and on account of the prime necessity of saving life in very difficult circumstances. I do not think that the Army surgeons on the whole deserve to be blamed for all the bad results to which Major Cooper alludes. I do not know whether his telephonic interrupter would have come under the ban of the military authorities "over there." Major Cooper says that electrically produced active contractions are necessary for the recovery of degenerated muscle. Once upon a time I thought so too, but do not feel so sure about it now. In reference to this point I may be permitted to mention two cases. One of them has been already mentioned by Dr. Cumberbatch. I saw the case first, as Dr. Cumberbatch was away from work at the time. The patient, a little girl, had suffered from infantile paralysis for five years, and her left leg was flail-like. All attempts to test the electrical reactions of the muscles of the affected limb were futile because the child cried so. I put each leg into a Schnee bath and applied rhythmically varied sinusoidal current, the child strongly objecting to having the treatment localized to the affected limb. She would only bear a very mild current, never sufficient to cause contraction of any of the muscles, yet, as Dr. Cumberbatch has told you, voluntary power gradually returned in several of the muscles. The second case was that of a woman who attended at St. Bartholomew's Hospital. She suffered from bilateral musculo-spiral paralysis due to lead poisoning. On testing with faradism, galvanism, and condensers, the electrical reactions were found to be practically the same on both sides. We decided to try the effect of labile faradism on one arm and labile galvanism on the other, three times a week. After about two months of this treatment voluntary power returned in the arm treated with faradism, but not until a fortnight afterwards did it

return in the arm treated with galvanism. At this stage the patient ceased to attend, apparently having left the district, because no letters succeeded in bringing her back. Major Cooper damns massage with very faint praise. I agree with Dr. Wilson that massage is a most useful help in such cases as those under consideration, and that a combination of massage and electricity is better than either remedy used by itself. It is essential that massage be properly performed. Massage either does good or it does harm; there is no halfway house about it, it is not a placebo. I have been under treatment at some spas where the masseurs resembled prize-fighters more than anything else. After each treatment I felt like a schoolboy who had been severely reprimanded. The discussion this evening has been very interesting. My one regret is that Major Cooper could not be present so as to answer certain points which have been raised.

Dr. LEVICK (in further remark): I assure Dr. Wilson I was not intending to criticize any of the schools of massage at all. What I criticized was the I.S.T.M. examination and its examiners, of whom I am one. We have not done our duty as a Board of Examiners. I have spoken to several members of the Association, and they say, "We are in the hands of the examiners," and I agree. There is a motion to talk the matter over. I am not against the schools. I have examined at three examinations at the I.S.T.M., and of all the candidates I have examined only two have shown even moderate knowledge of the way to exercise muscles by electricity, and of the muscular anatomy of the limbs. At Shepherd's Bush I have had a lot of masseuses sent with the view of getting employment; four were sent down by the Ministry of Pensions in the last two months, the Ministry stating that they held the full I.S.T.M. certificate in electricity and massage. One had been head masseuse at a military hospital, and had been entrusted with the testing of muscles, doubtless on the strength of her certificate. All those four I was obliged to turn down because they were too ignorant of anatomy to be admitted into my department. The one who had been head masseuse could only tell me the names of one or two muscles in the forearm. I asked her what muscles were involved in a low median injury, and she said "the interossei"! The fact is, the syllabus of the I.S.T.M. has not kept pace with the times, as it does not include the efficient application of electricity to orthopædic treatment.

Dr. SABERTON: I have not had the pleasure of reading Major Cooper's contribution, therefore I would like to know whether he lays stress on the value of preliminary warming of the limb in order to get better electrical reactions and more efficient treatment. To my mind the preliminary warming gives a different reaction in the electrical testing and the value of the treatment following. And I think diathermy is a valuable preliminary to electrical stimulation or massage.

Dr. NOEL BURKE: This contribution we are discussing is so extraordinarily interesting that it is necessary to put a curb on oneself when speaking on it.

There is one point in the paper which I wish to criticize, as Dr. Cumberbatch has already done, and that is the statement that all the results of nerve injury are purely those of disuse. I do not see how that can be accepted, in the light of the work of Professor Langley, which ought to be better known among doctors than it seems to be. Major Bristow says he does not think people will believe the galvanic current will do good in cases of nerve injury. I must believe what I have seen and I have seen cases in which one could not get a contraction owing to the condition of the muscle, and yet the passage of the current during treatment in two or three weeks has so improved the condition of the limb, its circulation and general nutrition, that it has been possible to elicit the contraction known as the complete reaction of degeneration. I have used preliminary heat, but not always, and then the galvanic current, and also massage, and between them, in a very short time, it was possible to produce contraction of those muscles. That seems to prove that benefit has resulted. I do not think one is entitled to say there has been nothing but disuse atrophy going on. With regard to methods of treatment, I think there is no doubt contraction is essential, both for its massage effect, the removal of waste products, and for the fact that it preserves the physiological action of the muscle. But if one does nothing but produce contractions by electricity, one is missing a great deal that is possible. Last month I had a treatment by the coil of Major Cooper, and it was extraordinarily pleasant, but one in which the sensation is no more painless than that from the surging galvanic as applied by a water resistance machine such as was devised by the Electrical Supply people a couple of years ago. This has the advantage that in the case of reaction of degeneration you can select the muscles you want to treat, and I do not gather from Major Cooper that you can select the muscles with reaction of degeneration by his machine, nor do I understand how he gets contractions from them. I would like to say a word in support of the I.S.T.M. and their examination. I have examined for it for five years, and I have never been at one without demanding some information on the names of the muscles and their nerve supply, and I have got a fair knowledge from the candidates. If not, I reject them.

Dr. G. B. BATTEN: I think we shall all agree we must get warmth and general nutrition to the affected limb, and therefore I believe massage, diathermy, and radiant heat all do good. Still we shall also agree that the stimulation we get from electricity is the most important agent. As an electrician, I want to say that all these forms of current are variations of the same thing, whether galvanic, faradic or static. They all mean a movement of electrons, and it is only a question of voltage and rapidity of the interruptions. Whether you use a rapidly interrupted galvanic, or a more rapidly interrupted faradic, or a much higher voltage static wave current, it is the same thing, and the sinusoidal is the same with rapid reversals. The discussion has wandered rather away from the machine mentioned in Major Cooper's paper, which interrupts at something like 500 to 800 times a second. Dr. Levick has told us—and I agree with him

—that the time of response of a muscle remains a constant, and it is a question of getting the optimum rate of contraction of a particular muscle fibre, and the optimum rate for a muscle as a whole. You cannot expect the quadriceps extensor or the latissimus dorsi to contract as fast as the interossei, therefore you want a different rate of contraction. I think Major Cooper has exceeded the optimal limit. I think the rate for fibres of a muscle should be from 30 to 100 per second, and for a whole muscle not more than ten times a second. With regard to the I.S.T.M., nearly all the candidates I have examined know their work up to 90 per cent. of the schedule, but they do not know anything outside the schedule, and the fault lies in the schedule, not in the examiners.

Dr. TURRELL: I have been asked to reply, as far as I can, for Dr. Cooper. It is difficult for me to do so, especially as I differ from him on one or two minor points. Still, I will try to submerge the differences. I have one advantage in that I am able to say it is an excellent paper, for that could hardly have been said by Major Cooper himself. In such a paper as this, three things are required: extensive clinical experience, an intimate knowledge of physiology, and some mechanical ingenuity. But the chief requirement is clinical experience. Major Cooper had a very distinguished physiological career before he started electro-therapy—another thing he could not have said for himself—and at Netley he has had charge of one of the biggest electro-therapeutic establishments in this country, one of the best-run and one of the most complete, and so is eminently fitted for the task he has undertaken. Except for an efficient static machine, which owing to the war he could not obtain, he was exceedingly well equipped there. I know, from my experience, that Major Cooper made a great impression on the surgeons with whom he worked at Netley. One leading London surgeon told me that, from his experience of Dr. Cooper's work, he was convinced of the great value of muscular contraction excited by electrical means. Dealing, first of all, with Dr. Cumberbatch's remarks, I think we ought to ask ourselves, in treating paralysed muscles, what we are out to do. It is simply to maintain the limb, when the nerve is severed, in as good a condition, trophically, as possible until the nerve regenerates. We do not claim—certainly Dr. Cooper does not—the regeneration of a nerve by electrical stimulation. And I think Mr. Bristow will agree that a nerve will recover more quickly if the leg is well nourished and the blood supply is kept up in the limb than if the limb is allowed to remain ill-nourished, with trophic sores and ulcerations upon it. With regard to the method of stimulation, when a nerve is completely paralysed and there is reaction of degeneration in it, you will require a very large stimulus to produce contraction. And I have concluded, as a result of my experience in the war—and I think Major Cooper is of the same opinion—that where a large stimulus is required to cause a contraction, better results will be got from the sinusoidal or by the continuous galvanic current. The nutrition is maintained in the limb by ionic interchange, whether the ions pass backwards and forwards or in a unidirectional direction. In treating sciatica by ionization,

with a large pad on the buttock, and passing 100 m.a. through that leg, if you compare the heat of the two legs you will at once notice the treated leg is distinctly the hotter, and this increase of heat is necessarily attended by an increased blood supply. During the war we have known that trophic sores have disappeared when the nutrition of the limb has been improved by the galvanic or the sinusoidal current. When the nerve is on the point of regenerating, the need is to produce contraction, and that is best done by a selective form of application, such as the Lapicque condensers, which pick out the paralysed muscles and produce contractions in them without contractions occurring in the healthy muscles. And this has a valuable re-educative effect on the patient, for you can get the patient himself to help with the movements. I agree with Dr. Cumberbatch's remarks in regard to the use of the sinusoidal current for the cases with advanced degeneration.

Mr. Bristow was very refreshing, because he carried us back to 1840, when the faradic was the only current used. And he was in very good company, the company of Duchenne of Boulogne, who relied on the faradic current for everything in those days. He is also in the company of a very eminent anatomist, Professor Keith, who says that "as to the exact nature of that influence (electricity) we know little more than Hunter did in 1793." I would like to deal with what Dr. Cumberbatch said in reference to Professor Langley's experiments. I have read carefully Professor Langley's paper, and I do not think, in view of our experience, that it is of the slightest value from the practical standpoint. He took rabbits and divided the sciatic nerve on one side; then treated the paralysed limb with condenser stimuli. But the stimuli he used were from condensers 0·016 mfd. and 0·025 mfd. capacity. And you know that 0·016 mfd. is barely sufficient to stimulate contraction of the sensitive muscles of the face, and 0·025 mfd. is only of sufficient duration to cause contraction of an ordinary healthy muscle in a limb. If any nerve degeneration sets in, those stimuli used by Professor Langley are absolutely insufficient to excite any contraction. The stimuli he used were only administered for four to eight minutes at a time, and were repeated for only four to fourteen days. What electro-therapeutist would expect to get improvement in a paralysed limb by so few stimuli administered for so short a period? But even then there was a slight improvement in the treated over the untreated muscles, though how it resulted I do not know. I have not time to refer to the other methods which Mr. Bristow runs atilt at, but I would point out that in the matter of coils we have advanced very little since 1865, when Tripier showed, at the French Academy of Medicine, coils with a variable rate of interruption, and variable secondaries, and different primaries. Except for the condenser introduced by Fizeau there has been but little advance in the design of coils since Tripier's time. It is yet to be proved that the coil Major Cooper has shown us is an improvement. I find it is a very fatiguing coil: there is a very rapid rate of stimulation, and the muscles get tired in about one minute. The massage question has already been dealt with. I do not examine for the Massage Institute, as nearly everybody else in the room seems

to do, but I have been very pleasantly surprised at the knowledge of their work shown by the masseuses I have come across. And what has impressed me even more is their evident keenness to learn. If they do not know their work, it is the fault of the medical men, and it is the fault of the medical men that the masseuses do not know more. Many of the men in charge of these Electrical Departments leave the whole of the muscle-testing to the masseuses: they do not tell them anything—for the reason that they know nothing about it themselves.

In reply to Dr. Saberton, Major Cooper does warm the limbs before treatment, and in his well-organized department he has every facility for doing so, and he is, I believe, in favour of the whirlpool baths for this purpose. One or two of the speakers this evening appear to be under the impression that Dr. Cooper utilizes the rapidly interrupted coil for the stimulation of paralysed muscles: I am sure that they must have misinterpreted his remarks or misread his paper. It is only in the treatment of muscles with intact nerve supply that Dr. Cooper advocates the use of any form of faradic current.

Exhibit.

Dr. HEYERDAHL exhibited Slides of Cases of Actinomycosis of the Face and Neck cured by Radium.

DISCUSSION.

Dr. ROBERT KNOX: I congratulate Dr. Heyerdahl on his results, which are excellent. I have seen several cases of actinomycosis, some twenty years ago. Earlier cases I treated with iodide of potassium, and I have had two cases practically identical with the second case shown by Dr. Heyerdahl, and they both responded at once to iodide of potassium, each receiving prolonged dosage. I have frequently seen cases which I suspected to be actinomycosis among cancer patients, but as I could not isolate the organism I have had to assume they were not such. Recently I treated a soldier who had been sent back from France with the diagnosis of actinomycosis. This involved the side of the cheek and the neck. I treated the condition with X-rays and iodide of potassium combined, and he got well. I am certain there are more cases of actinomycosis in this country than we are aware of. We are very much indebted to Dr. Heyerdahl for coming here and showing us his excellent series of cases, and I would also like to congratulate him on his excellent photography.

Dr. HEYERDAHL (in reply): I have treated actinomycosis in other regions too—in the abdomen, for instance, and I have tried to treat it when it has been in the breast. In the abdomen the development has been prolonged, perhaps two or three years, but except in the cases in the face and neck I have had no results.

Simple Foot Switch for use in Electro-diagnosis.

By W. J. Turrell, M.D.

The idea of this very simple instrument, which I show you this evening, was obtained from the *American Journal of Electrotherapy and Radiology*. It is nothing more than the ordinary foot switch which is often used for ringing the dining room bell, it is known as the Daisy Foot-tread, and is obtainable from most ironmongers. It has very distinct advantages over the ordinary metronome as an interrupter of the current when testing the reaction of nerves and muscles. It is simpler, cheaper, has nothing to get of order, and the duration of the stimulus is directly under the control of the investigator; which is a point of considerable importance when testing muscles with a feeble response. It should be specially suitable for testing reactions during operations.

Section of Electro-Therapeutics.

President—Dr. A. E. Barclay.

Radiology: Lessons of the War.[1]

By C. Thurstan Holland.

The subject of the paper I propose to read to you this evening is "Radiology: What the war has taught us." In its main purpose it is written with a view of putting on record certain facts, to point out in what respects we failed and in what directions we succeeded, and to invoke some discussion from the point of view of ensuring that in any future emergency the general lines on which the X-ray work should be established and carried on shall be laid down. It is quite impossible to deal with a subject of this kind without making certain adverse criticisms with regard to Army methods and War Office administration: but in making these criticisms I trust that it will be understood that it is rather the system with which I am dealing, and not with individuals.

When war broke out so suddenly in 1914 practically no one recognized what was going to happen. I mean by this the lines upon which the war would develop, the strain it was going to be on every branch of the service, the rapidity with which one event was to follow another and to upset all calculations. It became almost impossible to look far enough ahead: measures which were taken to deal with events as they appeared to demand it rapidly became obsolete, and others had to be improvised and pushed on to take their place.

These remarks apply to almost everything which was either done, or not done; and X-ray work—a small, though important, branch

[1] At a meeting of the Section, held January 16, 1920.

of the whole machine—could not be expected to differ materially from the rest. We experts, if I may be allowed to apply this term to ourselves, are apt to consider our own little show as perhaps of more importance than it actually is; and when we see things in our line not going on in quite the manner we consider proper, some of us at any rate are inclined to forget that there are perhaps things of equal, it not greater, importance, the demands from which call for priority in treatment.

Let us recall for a moment the position of Army X-ray work in July, 1914. The schedule of Army X-ray equipment was practically that which had been in use during the Boer War. To all intents and purposes there were no Army medical officers expert in X-ray work. No extra pay was granted to officers qualifying and practising this branch of medical work. No special organization existed in the Medical Branch of the War Office to deal with X-ray work. It is a very easy thing to look back and adversely criticize our administration for such a position of affairs, but it must be remembered that there had been nothing in previous wars, nothing in the Army work during peace, *and nothing done by any of the radiologists of this country,* to suggest to the authorities that an elaborate department should exist. It must, I consider, be taken as a fact that none of us up to that date had realized in any way what would be the necessities of X-ray work in a great European war.

This, then, being the position when war began it is necessary to recall what followed: and again I take it that it was not realized by any of us as to what was going to happen. The talk in the early days seemed to imply that the war would be over by Christmas, 1914—perhaps indeed it might have been, but if it had it would not have been the ending all of us desired. It appears to me, looking back, that it was only as the months before Christmas, 1914, slowly passed away that the realization of a long war, in which the whole nation would be involved, gradually began to force itself upon us.

Many of us have criticized the War Office for not, immediately on the outbreak of war, having organized an X-ray branch of the Service, managed and controlled by X-ray experts. I myself have done so, and I have drawn attention to what the Americans did when they came into the war, and contrasted it with what we did not do. Perhaps this criticism was not quite a fair one, but it was provoked not so much by what had not been done in 1914, but by what had not been done from 1914 up to 1917—up to the date when America

did come in—and by the position during those years of our work and of ourselves: and from the point of what was not done during those three years I think criticism is justified. Long before 1917 it had become very evident that X-ray work was a prop on which much of the successful war surgery was being built up. Surgeons—and very especially the younger ones—were everywhere realizing that efficient X-ray work was essential to them—I would emphasize the word "efficient": and yet up to this time, 1917, and indeed even later than this, nothing had been done officially to ensure that the X-ray work, equipment, and organization, should be controlled by an expert board or committee, and that the whole control and management of this branch of medical work should be in expert hands. It is in this direction that the permanent medical staff of the War Office failed, and failed badly.

I recognize of course, that my—indeed our—friend, Sir Archibald Reid (Captain Reid then) had been attached to the War Office in an advisory capacity as early as November, 1914, and that later on certain physicists were called in to help him and a small committee formed. I recognize also very fully the splendid work he did for so many years, and rejoice in the honours which have been conferred upon him; but this does not alter what I consider to be the fact, that the expert radiologists of this country as far as giving advice and helping to organize and control X-ray work were entirely ignored. Why was this? I have already indicated some of the reasons, but there were others. Why was the offer made by this Section of the Royal Society of Medicine soon after war began—the offer to place the services of its expert members individually and collectively at the disposal of the War Office—practically "turned down"?

It is possible that the War Office chiefs were not alone to blame for the short-sighted policy adopted. At that time many of the older surgeons were almost the only members of the profession who were consulted as advisers. Many of us are well acquainted with the attitude of many eminent surgeons towards radiology—it exists, in London especially, even to-day—the attitude which may be summed up as follows: "All we want is a good radiograph; this can be taken by a layman quite as well as by a medical expert, even better perhaps; give us this and we can read the plates ourselves, make our own diagnoses, draw our own conclusions."

To my certain knowledge the Committee of eminent surgeons which met at the War Office never had associated even one expert radiologist,

and Reid did not sit upon this Committee. In other words advice on X-ray matters was a question for surgeons, and for surgeons alone.

It is well known to all of us that, at a certain period, one of our members made some very pointed criticisms as to the X-ray work which was being done throughout the country in war hospitals, &c., and that these criticisms were directed especially towards apparatus and the localization of foreign bodies, and towards the assistants working in X-ray departments. It is equally an open secret that the consulting surgeons reported to the War Office that "all was well with the X-ray work throughout the home commands." You will notice that it was the "surgeons" who made these favourable reports, and not the X-ray experts. To my mind it was a most unfortunate thing that it was only the consulting surgeons to the home commands who were asked to report on this subject, and I have no doubt whatever that if, at this time, the expert X-ray men working in Army hospitals at home had been asked to report, certain grave defects in the X-ray work, then existing, would have been pointed out. Most certainly if I had been asked—to make it a personal matter—my own experiences of the early years would have given me ample grounds for proving that things were anything but satisfactory.

I propose to quote to you some of these experiences in support of this: and I have no doubt that all those of us who worked at home could quote many more. Those I give are only examples.

(1) During the first year of the war an Army Order came to the hospital to which I was attached instructing me to receive two young students of physics, to give them *one week's* X-ray training, and then to report upon them as to their suitability for X-ray work. I would emphasize the fact that they were physics, and not medical, students. They spent a week with us—wrecked several tubes, and incidentally the temper of the sister-in-charge. I then reported that I found them pleasant and intelligent students who had put in a week's work in the department. I did not commit myself further than this. Very shortly afterwards I received two letters of grateful thanks from two young gentlemen who had been appointed to take charge of two hospital X-ray departments at a salary of £250 per annum each.

(2) A similar instance illustrating the official attitude towards X-ray work was that in which an elderly medical man came to me with a request for intensive instruction as he had been ordered to take charge of the X-ray work on a very large hospital ship. For all practical purposes he knew nothing about X-ray work. There was no time to give

him any instruction of value before his ship sailed; the day before it sailed the X-ray equipment arrived and was dumped on board. I saw this equipment at his urgent request—it was as beautiful as elaborately polished wood and metal work could make it—on the other hand I do not know to this day what some of it was meant to be used for, and I am equally certain neither does the gentleman placed in charge. He had also been supplied with about £20 worth of 15 by 12 plates and no dishes in which they could be developed.

Appointments of these kinds were common and their direct results can be illustrated by the following:—

(i) I saw a man who had a large, healed, operation wound, 6 in. in length, to the left of his sternum and directly over his heart. He told me that he had been X-rayed and that a bullet had been seen and located and the surgeon had attempted its removal. As a matter of fact the said bullet was exactly 1 in. deep in the muscles of the back.

(ii) I saw another man who had been trephined after an X-ray examination had shown a small foreign body inside the head. The man told me that he had had no symptoms whatever except a slight headache, and that, at the time of the operation, the original wound was quite healed.

Now it seems to me fairly certain that no surgeon would have attempted such an operation if the X-ray report had been to the effect that the piece of metal, very small, was 2 in. deep in the frontal lobe, which was where it was.

(iii) An officer I once saw had a fairly large foreign body behind the upper part of the left lung; he had no symptoms calling for its removal; the wound was healed; we were not certain whether it was just inside or outside the pleura. Later on I saw this officer again; and this after an eminent surgeon, who had made his own screen examination, had decided upon the anterior route by means of a large incision in front just below the collar-bone. I need hardly say that he did not find and remove the foreign body.

These are merely examples of what we were all of us more or less constantly seeing. They were inevitable under the circumstances, but they show quite clearly that all was not well with the X-ray work at the time when so favourable a report was furnished to headquarters.

Later on in the war, in 1918, X-ray experts were appointed to the various Commands as advisers to the D.D.M.S.'s; a sufficient acknowledgement of error inasmuch as it showed that at this period the War Office had become alive to the fact that X-ray work should be organized

and officered by experts, and that they, and not eminent specialists in other branches of the profession, should be responsible for the X-ray work to the heads of departments.

I look upon this change of attitude, tardy though it was, as a very distinct recognition of the claims, which had all along been made, as to the status of radiology in the Army.

This recognition was due to two causes: (i) the sympathy extended towards X-ray work by Sir John Goodwin; (ii) the support which the three surgical members of the Advisory Board, namely, Sir Berkeley Moynihan, Sir Harold Stiles and Sir Robert Jones, gave him in this matter. We radiologists owe a very great deal to these four gentlemen, more perhaps than many of you may be aware of.

These expert appointments, the addition of several experts to the War Office X-ray Committee, the grading of all R.A.M.C. officers as regards their X-ray knowledge and abilities, all came too late to have any great effect on what had been done before. It was well on in 1918 before any of them became effectual, and events moved so rapidly from this time onwards that no great amount of reconstruction or reorganization was possible; but the recognition of important facts is there, and will undoubtedly have a distinct bearing on the future.

Some recollections of what happened to ourselves in the first few years owing to the attitude of the authorities are not devoid of interest. Most of us older radiologists were, when the war broke out, *à la suite* Territorial officers, others joined up in a similar capacity as it became evident there was a great deal of X-ray work to be done. Many of us were too old to go abroad or had already been damaged by X-rays to such an extent as to make this impossible. What was our position? How were we treated? I joined in 1908 when the territorial hospital staffs were first appointed. I took the junior rank of Captain, looking upon it as merely honorary, and on the distinct understanding that I should, if ever required, perform such duties in a military hospital as I performed in civilian hospitals. Mr. Macpherson, then Under-Secretary of State for War, in November, 1917, in the House of Commons, to all intents and purposes admitted that this assurance had been given; qualifying it only by saying that the "clinical" duties should not necessarily be identical. He further stated that he was not aware of any case in which *à la suite* officers were compelled to duties other than clinical, and later on that he was not aware that any of these officers were called upon to inspect hospitals administratively. As he seemed to be so grossly ignorant of what we all knew was going on in every

territorial hospital in the country I wrote him the following letter. This was in December, 1917.

Sir,—I beg to inform you that officers of the *á la suite* staff have been ordered to perform duties other than either clinical or medical. Ever since the war began many officers on the staff of the First Western General Hospital have been ordered to do (i) orderly duty, (ii) inspect auxiliary hospitals. Consulting hospital physicians, specialists, such as physicians attached to general hospitals in the capacity of throat specialists, skin specialists, anæsthetists, X-ray specialists, and even dentists, have been detailed for these duties. Most of these men may be described without prejudice as well known leaders of the profession in their various lines. Most of them, men of about 50 years of age. These men for the most part took their Commissions with a view to being able to offer their country skilled professional services of the most valuable kind in the event of national emergency. They were willing, and anxious, to render these services. They accepted the junior rank of Captain to enable them to perform these services.

(i) Orderly duty consists in, amongst other things, re-classifying wounded and sick discharged from the attached hospitals and already classified by the medical officers attached to these hospitals. In admitting cases sent to the base hospital by other hospitals. In signing railway warrants and the letters of the officer commanding and the registrar of the hospital. In doing odd duties of all kinds.

(ii) Inspecting auxiliary hospitals they are instructed to report on the keeping of books, the condition of ashpits, the condition of kitchens, and many things entirely apart from clinical work.

I would most respectfully point out that these officers were, and are, entirely ignorant of, and quite incapable of, reporting on and doing much of the work required, and that in these reports much of the work they have done is absurd. Additionally the waste of time valuable to the Army and to the general community is obvious—and so on.

I suppose I was fortunate to get any reply at all, but the fact remains that I did get one—from "the great man's" secretary. It read as follows:—

Dear Sir,—I am requested by Mr. Macpherson to acknowledge your letter of the 18th instant.

And now, having indicated in some measure the general attitude of the War Office towards X-ray work and X-ray workers, and its very general effects, it is time for me to pass on to other questions; and, in the first place, let us for a few moments consider the following:—

(II) What has been the Effect of the War on the Production of X-ray Apparatus, on the Invention of New Instruments, and so on?

Perhaps I may take the latter in the first place. The only outstanding advance in apparatus has been the American production of the standard mobile transformer unit with its radiator type of Coolidge tube. This may be said to be the direct result of the war, and it marks a distinct advance in apparatus. An X-ray generating outfit of this kind, working from a petrol engine or a lighting circuit, constructed to work at practically a fixed milliamperage through the tube, almost fool proof—and this nothwithstanding the recent lamentable accident to our French colleague—capable of doing all the work required at casualty clearing stations, advanced hospitals, and indeed most base hospitals, camp hospitals, and so on, would have been invaluable had it been available at the beginning of the war as a standard apparatus for general use. An outstanding fault of our equipment, and a fault it appeared to be impossible to apply any remedy for, was that no standardization seemed to be possible. At the beginning the shortage was such that it was necessary to take almost any apparatus which the instrument makers could supply. Many attempts were made later on to get some of the things standardized; but, amongst other things, the difficulties in manufacturing and the difficulties of labour, which got worse and worse as more and more men were called up, made such ideals impossible.

The outbreak of war found out the weak points in our home X-ray industry, but on the whole our manufacturers met the situation splendidly; and as far as coils, interrupters, couches of various kinds, and most, if not all, of the minor accessories, the supply was reasonable and the quality very good.

Under the direction of the War Office Committee the petrol engine sets, chiefly for use abroad and in places where an electric current supply was not available, reached a high level of excellence and efficiency. Later the mobile car units were turned out in good numbers, and were remarkably useful and efficient. Their design and equipment left little to be desired, and they accomplished a great deal of most useful work, especially at casualty clearing stations.

Plates and printing paper suffered somewhat in the earlier years chiefly on account of the glass and paper difficulties, together with the

joining up of many of the skilled hands. In the latter part of the war both were excellent.

Tubes threatened at first to be a serious question. We all know to to what an extent we had relied upon Germany for our tube supply. The difficulty, however, was overcome, and, whilst importations from America helped to a considerable extent, our own manufacturers rose to the occasion most successfully. Taking into consideration the position of the home industry in 1914, the glass difficulty, the almost entire absence of any skilled tube makers, the results obtained were tremendous, and great credit is due to all those who took this matter in hand.

It is up against us all to see that we give all possible support to our own manufacturers in the future, and not to buy foreign-made tubes unless we are compelled to do so. At the same time, my personal experience is that, certainly with some makes of home-made tubes, they have not been so good or reliable during the past six or eight months as they were during the latter years of the war. The faults I have had to complain of are: (1) An undue tendency to puncture; (2) a liability of the cathodal terminal inside the tube to loosen, and in some cases to break off; (3) and the very serious one of the regulating device being quite inefficient. And a word of warning to the manufacturers may be uttered that such faults must be overcome if we users are not to be driven by bitter experience, at a cost to both our pockets and our tempers, to buy in other markets.

I have said that the plates and printing paper rose to a high degree of excellence during the war, but here again, and especially during the past six or eight months, X-ray plates, in my opinion, most certainly have undergone a serious deterioration in quality. I have been finding it necessary to very considerably increase times of exposure, and even then have not been able to obtain the same quality of negative as I did before the war. It is not only slowness, but loss of quality of which I personally complain, and I should be interested to hear the opinion of others on this point. In regard to this plate question it may be of interest to read to you some correspondence which I have recently had with a well-known firm of X-ray plate makers. It began by a letter of mine, in which I asked what was wrong with the plates, as I found them wanting in both speed and quality. The usual plate-maker's letter came in reply stating that they were at a loss to understand my complaint, as their tests—mark this—had shown their plates to be up to their usual average. They asked for samples. In reply I sent them some of the same batch of plates plus another letter explaining

the reasons for my complaint in full. The answer to this was "that my complaint was justified." (This, mark you, notwithstanding that previously they had quoted their tests against me.) The reason given is of some interest: "Owing to the unevenness of the glass supplied to us during the last few months we were compelled to coat our X-ray plates rather thinner, and this we find accounts for the falling off in quality to which you refer." I acknowledged the receipt of this explanation, expressed my extreme interest in the same, and called their attention to the fact that their X-ray tooth films had deteriorated in the same manner as their plates, and I presumed that I was to understand that the celluloid suffered from the same complaint as the glass. A further explanation followed as regards the films, "that owing to commercial reasons they had been compelled to alter these, which now required four times the exposure to those previously supplied." I closed the correspondence by drawing their attention to the fact that to supply—without any warning—for a satisfactory article something which was quite unsatisfactory did not appeal to me as being either rational or business-like.

This correspondence is interesting to me from two points of view: (1) That the said plate-makers evidently considered my mental capacity to be such that "any old reason" would do for me; (2) that the deterioration of the plates and films occurred at a time when the price of silver was higher than it had ever been before. However it is of some interest to note that the result has been that the plates have considerably improved, and that I am informed the tooth films are now at their old standard of speed.

I now must turn to my third, and last, proposition :—

(III) What has been the Effect of the War on X-ray Work and on X-ray Workers?

As far as we older men are concerned, I am afraid that—speaking very generally—war work has not taught us very much. We have of course had a large experience of the localization of foreign bodies, experience which up to a point was very interesting; but as the war went on and on most of this work had of necessity to be reduced to a routine more or less, except for individual cases it ceased to be of any very definite interest. It is probable, also, that after a few years, most of this kind of work, except for the ubiquitous piece of needle in the hand, will come almost to an end.

Next in importance were the vast numbers of bone injuries. From the purely X-ray point of view I do not think we learnt very much which added to our X-ray knowledge from these. Of course I do not mean to imply that they exhibited no points of X-ray interest, or that both individually and collectively many did not exhibit some new features, but their X-ray examination cannot be said to have influenced X-ray work to any extent.

These cases formed the bulk of the X-ray work abroad, and all the work in any except base hospitals. Thus, whilst those of us who worked at home still had to deal with the numerous other conditions which require X-ray examination—soldiers suffering from them in the same way as civilians—those doing the X-ray work abroad for the most part had experience only of wounds and their immediate consequences.

The following classes of cases, without claiming that this is by any means a complete list, seem to me to be those in which radiology proved of very great advantage, and in which the science and art of radiography was advanced.

(1) In diaphragmatic hernia, a condition recognized occasionally before the war as within the scope of X-ray diagnosis, the value of radiographic examination was certainly brought into more prominence owing to the comparative frequency in which this condition has been met with following wounds of the diaphragm. Our X-ray knowledge of the appearances, and of the methods of examination, and the diagnostic points has been advanced.

(2) In "gas gangrene" an entirely new X-ray advance was made. The possibilities of X-ray diagnosis in this condition were first of all established by my friend, Dr. Morrison, when serving with the Liverpool Merchants' Hospital: his beautiful radiographs and the description of them appeared in the *Archives of Radiology and Electrology* for December, 1915. I believe this to be the first published X-ray description of this condition. Later radiology became of great importance in the diagnosis and prognosis of gas gangrene, as is well shown in a masterly paper by Miss Ivens which was read before the Surgical Section of this Society in the latter part of 1916.[1]

The opportunities of examining cases of tropical abscess of the liver by X-rays have increased enormously. Men returning from the East who have had dysentery, and who have apparently completely recovered, are later on developing these abscesses of the liver, a condition in which an X-ray examination is often very definite.

[1] *Proceedings*, 1916-17, x (Sect. Surg.), pp. 29-110.

In therapeutic work the treatment of keloid condition by both radium and X-rays has been much advanced, and the depilating power of X-rays has proved of the utmost value after plastic operations on the face following war injuries. I would especially mention the work of our colleagues, Robert Knox and Stevenson of Dublin.

I do not intend to say much about localization and the many and various instruments elaborated in its cause. Practically every X-ray man—with the notable exception of my friend, Dr. Morrison, I may say that he claims notoriety on this account—I say every X-ray man, together with a good many who were not such, invented or designed some little bit of apparatus with a view to facilitating either actual localization, or the operations for the removal of foreign bodies. As I assisted myself, I can include myself in any criticisms or remarks on this part of my subject. But when we come down to the bed rock of things, and really ask ourselves in cold blood what new ideas we gave birth to, it is as well to pause and consider the position. Let me remind all the newer workers—at the same time perhaps I may also include many of the older workers—of a paper which appeared in the *Archives of the Röntgen-ray* in August, 1900, by Dr. Rémy, of Paris, and this paper had been published still earlier in France, whilst Dr. Rémy demonstrated his methods at St. Bartholomew's Hospital in June, 1900. This paper is illustrated by means of photographs. It is unfortunate that, in his extensive bibliography of localization papers in the March number of the *American Journal of Röntgenography*, Case does not give this reference, although he refers to an earlier paper by the same author published in 1897. Now in this paper of Rémy's and in his photograph illustrations is clearly shown the method of screen localization from below up by means of a tube shift, i.e., the triangulation method we have nearly all of us used as the basis of our methods. Rémy also used metal directors from above to indicate the paths of the X-rays, and to point to the spot where the foreign body actually was, the forerunner of the Hirtz compass and all such similar devices. I advise all of you to read this paper, and especially would I advise those who have learnt all they know about X-ray work from war radiography to read it. It was written at a time when X-ray apparatus was very different to that which we now have, when technical difficulties were very great; and yet it lays down all the principles of the triangulation method of localization from below up, and suggests all its future developments. It makes a lot of our pet "discoveries" look very small indeed. Localization work has been interesting: it has brought out very much

ingenuity. I do not consider, however, that it has brought about any real great advance in the science and art of radiography, and when we get right back to ordinary civilian life, and those foreign bodies, still unremoved, have finally settled down, we shall revert to the occasional localization of the pre-war days.

The great effects of the War on X-ray work must be looked for in other directions, and it is in these directions that our work has been, and will be in the future, influenced. The war brought radiology into very great prominence, and this notwithstanding the attitude of the authorities during the early years. The enormous number of laymen and women who became interested in, and who took part in the work of, our war hospitals both abroad and all over this country, altogether apart from the enormous number of wounded who were radiographed, means that the eyes of the general public have been very much opened to the importance of X-ray work in medicine and surgery. Especially was this the case in our large home hospitals, as in these the cases were by no means limited to war injuries. The effect of this will tell in many ways. It will mean a large increase in X-ray work, as members of the lay public either themselves, or on account of the advice of friends, will increasingly insist upon having X-ray examinations of themselves made, and this alone will, should it be necessary, force the hands of their medical attendants. However, it is hardly probable that this will be necessary. The whole of the medical profession of this country has had the same thing impressed upon them also. The war has opened the eyes of the general practitioners of medicine throughout the country to the vast possibilities of X-ray work.

Remember that it is really only within the last twelve years or so that the X-ray position as regards the kidneys, the stomach and the thorax has gradually become firmly established. To the majority of the general practitioners scattered all over the country this work was not well known. All these men have now served in war hospitals at home or abroad, or have been intimately associated with the work in these hospitals, and they have been able to see, and to realize, the advantages to themselves and to their patients of an X-ray examination conducted by a skilled expert. Thus again, the direct result of the war will mean a great increase in X-ray work.

Surgeons and physicians, especially perhaps the former, have had forcibly impressed upon them, by what they have seen day after day—"the importance and scope of radiology." This again means increased work. We may therefore in the future—indeed I know from my own

work, both hospital and private, that this is the case now—look forward to a greatly increased use of radiology for diagnostic purposes; and also to a demand that this work shall be of the very best quality—the lay public and the profession generally have been educated up to this. From this point of view the "blessings" of the war have been great, and they will be greater in the future.

There is, however, an important fact which should not be overlooked: whilst medical men generally, who do not intend to practise radiology, have seen for themselves the value of X-ray work, from two other classes of workers there is a possibility of harm resulting. In the first place a very large number of lay assistants, male and female, have learnt a smattering of X-ray work: many of these have become really very efficient, but for the most part this is very far from having been the case. However, many of them, efficient or otherwise, have a very exalted idea as to their capabilities, and there is a danger that a certain number of these people may set up as unqualified practitioners to carry on the "art" of "X-ray photography." It is up against the medical profession generally to see that, if this happens, success shall not be the result. In the second place, and to my mind this is even more serious, a large number of qualified medical men have learnt certain parts of X-ray work very well. They have become very efficient at looking after coil outfits — in taking and interpreting radiographs of injuries to bones — in the localization of foreign bodies: but, as to their general knowledge of radiography and its interpretation as regards the kidneys, the thorax, the abdomen and its contents, bone diseases and X-ray treatment, their knowledge is so small that they may rightly be described as knowing nothing whatever. It is also quite evident, from some of the communications one has seen of late in the medical and X-ray journals, that many of them know nothing of the past literature of the subject, and a great many re-discoveries of old established facts have apparently been made and published. Some of these men have already, I believe, started work as X-ray specialists; others, from my own knowledge, are contemplating such a step. I would utter a word of warning and of advice to these men.

Cambridge University has published the Syllabus of a course in Radiology and Electro-therapeutics—this course is very comprehensive and commences on January 20. At the end of six months' teaching an examination will be held and a Diploma issued to those who are successful in passing it. My strong advice to all those who have learnt their X-ray work during the war is to take up this course of

instruction, and to obtain the Diploma, before starting on their X-ray careers. The six months sacrificed in learning will be as nothing, the advantages to themselves and to their future clients will be enormous. They will not then learn their X-ray work at the expense of much anxiety to themselves and at much harm to their patients, whether hospital or private. This Cambridge Diploma has come at a very opportune time, and I cannot help thinking that the war has had some effect in bringing it about. It should materially assist to raise the status of X-ray work and of X-ray workers.

These, then, are the few remarks I wish to make as regards "Lessons of the War": and in conclusion let me briefly sum up the chief points:—

The first I would make is that the war has taught us the tremendous value of radiographic work in war—that war surgery depends very largely for its success on efficient X-ray work — that it is emphatically proved that X-ray Army work should be directly controlled by expert medical radiographers—that such work should be recognized by the Army as expert work—that it has shown most conclusively that there is a necessity for a permanent staff at the War Office to organize: (1) The teaching of X-ray work to officers: (2) the teaching of trained nurses and orderlies who should be available as assistants; (3) the standardization of Army X-ray equipments; and with this goes the further point, that such equipment should be kept well abreast of the times.

The second is that it is our duty to see that in the future the teaching of radiology is put on a proper basis in all our medical schools, that not only should post-graduate work be firmly established, and that all who wish to practise as experts should of necessity be forced to undergo special training and take a special diploma; but that in addition a certain knowledge of X-ray work should be part of the ordinary curriculum of the medical students, and that teaching should be organized in view of this, and that in the examinations qualifying for practice X-ray work should not be ignored.

Section of Electro-Therapeutics.

President—Dr. A. E. Barclay.

(With the Röntgen Society and the Institution of Electrical Engineers.)[1]

Chairmen—Dr. A. E. Barclay (President, Section of Electro-Therapeutics), Afternoon Session; Mr. Roger T. Smith (President, Institution of Electrical Engineers), Evening Session.

AFTERNOON SESSION.

The Chairman.

Ladies and Gentlemen,—By your attendance you are showing the great interest you feel in the subject which has been chosen for the discussion at this meeting. Judging by the papers I have seen, which are to be submitted to you, I think we shall have a very profitable session.

Since the Electro-Therapeutical Section last met, the President of last year, has, I very much regret to say, passed away from us. I refer, of course, to Dr. C. R. C. Lyster. Dr. Lyster was one of those who thought nothing of himself, but went straight ahead with his work, and, at the end, he succumbed to its sad effects. I ask you to be good enough to stand in memory of him, and in recognition of the work he did.

The audience rose in respectful homage.

Dr. Russ was to have taken the chair during the first half of this discussion, as President of the Röntgen Society; in his absence I am taking the chair, and I call upon Dr. Reginald Morton to read the first paper.

[1] At a meeting held at the Royal Society of Medicine, February 26, 1920.

The Efficiency of High-tension Transformers as used for X-ray Purposes.[1]

By Reginald Morton, M.D. (Trin. Tor.), F.R.C.S.Edin.

For several years past it has been evident to many radiologists that, in the absence of some fundamental change in our methods of generating X-rays, further development in the science of radiology would be limited to matters of detail. Great improvements in apparatus and in methods have been brought about, especially during the war, but so far as the real essentials are concerned we are no further on than we were a dozen or more years ago when, perhaps without realizing it, we arrived at a sort of blind end out of which we have been trying to find a way.

The various steps that have marked the development of radiography are familiar to most of us. The demand for shorter exposures led to the production of more powerful coils and tubes capable of receiving these heavy currents—at one time the race between the coil maker and the maker of X-ray tubes was not without some elements of excitement. Almost it seemed that, given a sufficiently large coil and suitable tube, instantaneous radiography would follow as a matter of course; but we were not long in finding out that the induction coil was subject to limitations of an inherent nature—the self-induction and impedance of the secondary winding—that most effectually barred any real progress beyond a certain more or less well-defined limit. The monster coil was a disappointment that increased with its size, and it is doubtful if we have anything that can compare with a good 12 in. coil and mercury break, so far as it goes, as an efficient means of exciting an X-ray tube. The use of the electrolytic interrupter satisfied the demand for more current through the tube, but the large amount of inverse current was a serious drawback, which even the valve tube (itself a very unsatisfactory instrument) failed to remove entirely. Still the cry of the radiographer was for more milliamperes, free from inverse current, feeling quite sure that once obtained all would be well. At this juncture an electrical engineer—my friend Mr. (now Dr.) H. Clyde Snook—

[1] Throughout the body of this paper, as distinct from the title, the word "transformer" refers to apparatus having a closed magnetic circuit as distinguished from the "coil" with its open straight core.

brought forward his high-tension transformer, with a closed magnetic circuit and mechanical rectifier, giving a perfectly unidirectional current in larger volume than anything of which we knew. The promised land seemed well in the foreground and we hailed him as our deliverer. We have had this instrument now for close on a dozen years, and there is no doubt that it has been of enormous service to the whole science of radiography, but can anyone of us who has had a large experience in its use say that it has realized our full expectations? Do we get a shortening of the exposure in proportion to the greater current as compared with that from a coil? Or again, given the same screen effect, can we get the same result on a plate for a given exposure as in the case of a coil? With all the good points of these machines, the fact remains that to these questions only a negative answer can be given; and machines of the Snook type are very inefficient as producers of useful X-rays. In the early days of our disappointment we hid our chagrin by blaming the X-ray tube, and possibly many radiologists continue to do so. During the years of the war it is true we had much to put up with, but not entirely due to faults in the X-ray tube as such, or on the part of those who made them for us in the face of great difficulties. My own belief is that the best quality of X-ray tube such as we had at the outbreak of war is a wonderfully efficient and satisfactory implement, more sinned against than sinning, but quite unable to show its possibilities under existing conditions of use.

The chief object of this combined meeting is to explain our difficulty to the members of the Institution of Electrical Engineers and the Röntgen Society in the hope that they may be able to suggest a more efficient means of exciting our X-ray tubes. As some of them are not practical radiologists I think it would be as well to explain one or two fundamental matters, so that they may the better appreciate our requirements. The essential need of the radiographer is a supply of X-rays having a high degree of actinism and with sufficient penetration to pass through the thickest parts of the body without serious loss of this actinism. A "soft" or low-resistance tube gives off very actinic rays which have only a low power of penetration; we can increase this penetration by raising the voltage of the current applied to the tube, but only to a limited extent under present conditions, as I shall explain presently. We can have more highly penetrating rays by using a "hard" or higher resistance tube, but this is accompanied by an equal, or even more rapid, falling off in the essential property of actinism, and plates made with hard tubes are of inferior quality and little diagnostic

value except in the case of fractures and other of the simpler applications of radiography. How then are the prime requirements of the radiographer to be met?

Before attempting to answer this question we should first critically examine existing methods of operating X-ray tubes, and the only two we need consider are the induction coil and the high-tension rectifying machine of the Snook type. The large static machine is out of the question, in this climate at least, and the high-tension transformer with valve tubes to suppress the waves in one direction was early discarded.

With the aid of an oscilloscope and a rotating mirror some very interesting phenomena are presented for our study. If both terminals be connected in turn to the coil and transformer and a weak current be passed, we get more or less characteristic current curves of each, and the volume of current being very small the curves are not distorted by self-induction or impedance to any appreciable degree. Only the



FIG. 1.

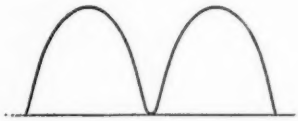


FIG. 2.

negative pole glows and its digression from the zero line is in direc proportion to the impressed voltage. In the mirror the coil shows us a curve having the general characteristics of fig. 1, while that of the transformer is shown in fig. 2, a rectified double-sine curve. In the one we have an alternating current quite unsymmetrical in both form and duration; in the other a series of perfectly regular sine curves, identical in form, duration and sequence. The periodicity remaining constant the curves are narrower and more numerous the more slowly the mirror is rotated; rapid rotation widens them out without affecting the height, all this being due to retinal persistence.

Here we have two voltage curves with widely different characteristics, and practical experience has taught us that the first is the more efficient for our purpose, up to the limit of its capacity. We get more penetration and more actinism for a given current, greater steadiness, less heating, and a longer useful life for the X-ray tube. A further point in favour of the coil current is that it will keep a tube working smoothly

on a lower reading of the milliammeter. Every tube has a certain amount of inertia and requires a higher voltage to start it than to maintain it in action; the sudden rise at the moment of "break" readily overcomes this inertia.

The explanation of this superiority of the coil over that of the transformer is a problem that has puzzled many radiologists—few more than myself. The mean value of the current through the tube is of no help to us in estimating efficiency, and we must have more detailed information as to what actually takes place.

I think it is generally agreed that penetration is dependent on electron velocity within the tube, which in turn is dependent on the impressed voltage. Thus, it would appear that the impression of a sustained high voltage on a tube in the best condition to ensure the greatest actinism should meet our every requirement. By "sustained" I do not mean "continuous" in the ordinary acceptance of the term; for reasons which will appear in the course of this paper my view of the ideal current is one consisting of extremely high-voltage impulses, of very brief duration as judged by ordinary standards, yet sufficiently sustained to have an appreciable time factor. Such current as would pass through the tube by virtue of those lower voltages represented in the lower part of the above curves should be suppressed or diverted. If the voltage of the initial impulse be sufficiently high and sustained for even a small fraction of a second, one will be enough for any ordinary purpose in radiography.

This is the problem to be solved, and, as others may be disposed to disagree with me, I will now briefly relate those observations and experiments that have led me to these conclusions.

In the days of comparatively feeble apparatus we were troubled with the inverse wave of the coil current as soon as we began to use as our source of supply the public supply mains with their higher voltage. To suppress this undesirable element the simple "point and plate" spark gap in series with the tube gave excellent results so long as the current was insufficient to establish an arc. This simple device gave us some very valuable indications that were for the most part ignored, and at the best did not receive the attention they deserved. Its action was of course that of a resistance gradually increased until all evidence of inverse current disappeared from the tube. Incidentally it also suppressed a corresponding amount of the current in the proper direction; the two being equal and opposite, the milliammeter reading was unaffected. Notwithstanding this elimination of a portion of its proper current

the X-ray output of the tube was also unaffected—at times I thought it was actually improved, but as I could not reconcile this with prevailing ideas I hesitated to commit myself to any definite statement. For all that, I carried my observations a step further and, instead of leaving the spark gap just sufficiently wide to eliminate the inverse current, I opened it still further until the milliammeter reading began to fall to a quite definite amount. Still the X-ray output remained appreciably unaffected as judged by plate and pastille. Opening it still further, the X-ray output began to fall off, but not to the same extent as would be expected from the diminution of current. Here, again, I thought I detected a distinct gain in efficiency, but ascribed it to some vagary of the tube itself—and they had many in those days. At this stage the spark gap ceases to be a satisfactory instrument—unpleasantly noisy in the open, and troublesome from the rapid formation of nitric fumes if enclosed. The obvious lesson from these observations is that it is through the influence of the higher voltages alone that we get useful X-rays from our tubes. Below a certain point the resultant rays have little or no penetration and for our purposes are worse than useless.

This being the case, the question naturally arises as to why the coil is unable to meet our requirements, considering the enormous voltage available at the moment of "break." If we examine an oscilloscope in series with an X-ray tube strongly energized by a coil, with the mirror rotating at moderate speed, the explanation is at once available. If we consider the enormous potential set up by the coil when arranged for a rapid exposure—capable of bridging 12 in. or more of air—and the relatively low resistance of an X-ray tube in the best condition for radiography, the initial rush of current must have a very high magnitude. The oscilloscope proves this to be the case, but it also shows that its duration is extremely short, so short indeed that rapid rotation of the mirror has little if any effect in opening it out laterally, and also it is extremely difficult to be sure of its upper limit where it tails off to nothing, so gradually that we cannot be sure where the end is. This is partly due to the self-induction of the secondary, cutting off this rush of current almost simultaneously with its birth. It is also due in no small degree to something inherent in the X-ray tube itself. Notwithstanding its brief duration—its correct representation in a diagram would be as near Euclid's definition of a line as anything I know of in nature—it is this impulse that makes the coil current so efficient up to the limit of its capacity. It is the absence of any such high-voltage impulse

that keeps the efficiency of the transformer current so low. For the same current through the same tube the top of the transformer curve extends little more than half-way to the highest point of that of the coil.

Of the total radiation from a tube energized by a coil, probably no more than 20 per cent. consists of useful rays; energized by a transformer the proportion is probably less than 10 per cent. This is of course a deplorable state of affairs, which is not made any more palatable by the knowledge that the efficiency of both coils and transformers is lower than it need be, for which radiologists have only themselves to thank. I am fully conscious of the seriousness of this charge, but so long as the radiologist takes milliamperes as his sole criterion, quite regardless of how they are made up, just so long will the further improvement of existing apparatus be delayed. There is no inducement for the manufacturer to improve efficiency—he must gather up and use every available fragment of current if he wishes to sell his apparatus—because radiologists as a whole have learnt nothing from the disappointing efficiency of the high-tension transformer. Yet improvement in efficiency is not only possible but easy, and manufacturers will readily effect this for us if we only make reasonable demands, backed up by knowledge of what we really do want. As an instance of the present state of things, take the case of a certain mercury break to which is attached a mechanical rectifying device that has achieved some success, and of which some members may have had experience. It arose out of a desire to apply the principle of the Snook machine to the coil, and of course my first impulse was to rectify the inverse wave and pass it through the tube. Fortunately, before I got very far with this idea I recalled the lessons learned from the spark gap, and decided to eliminate it altogether; but still somewhat obsessed as I was at that time with a desire for more milliamperes, I arranged the rotating wires to pass fairly close to the collectors, giving the latter the form of a sector. Even then I was criticized for not providing tinsel brushes on the ends of the rotating wires to ensure good contact. In vain did I plead that I was already picking up current of little or no value, but the Electro-Therapeutic Section would have none of it. Feeling sure of my ground, and still following the lesson of the spark gap, I shortened the sector to a point and finally altered it to a ball. Then I began to increase the clearance between the ends of the wires and the balls, and every alteration was followed by a gain in efficiency, steadier action, less heating, and longer life of the X-ray tube. I am not prepared to say just how

far we might go in this direction, but it gradually becomes noisy and insulation difficulties begin to arise. At present I know I am sacrificing a certain amount of efficiency for the sake of silence, but on the whole I am well content with my coil set.

When first brought out, this device was the subject of a certain amount of unfavourable criticism by certain manufacturers, who later on did not hesitate to copy it, and with an unwonted unanimity not one has remembered to acknowledge where they first got the idea. However, I readily forgave them when I saw how they had carried it out. Here again we see the baneful influence of the unreasoning and unqualified demand for milliamperes. In a recent number of the *Archives* there is an advertisement in which "actual contact on the high-tension collectors" is put forward as a special feature, whereas the fact is that this firm has gone to no little trouble and expense to lower the efficiency of the outfit with which it may be worked. This is, however, not so bad as the case of another firm who, in the effort to "go one better," rectify the inverse wave and pass it through the tube. So far from being an improvement it is no more than a complicated and costly means of spoiling an otherwise good machine.

This is not the way to improve efficiency; instead of demanding more milliamperes, let us measure the efficiency of our apparatus by the radiological result obtained from a given number of milliampere-seconds on both plate and pastille. That is the only thing that matters to the practical radiologist, and to increase this efficiency the manufacturer will be obliged to cut out the lower half of the coil curve and probably not less than 80 per cent. of the transformer curve. That this radical departure from usual practice will be followed by a distinct gain in efficiency is not a mere expression of opinion; I have tried it and proved it to be true beyond all question of doubt.

With regard to machines of the Snook type, I had one of these installed at the West London Hospital some years ago, and when delivered it was like most others—disappointing. It had the original cross-arms of wood in the rectifying portion. After a few months' use I began to shorten the collecting blades, and after each alteration there was a gain in efficiency. For a given adjustment of the primary circuit the reading of the milliammeter steadily declined with each shortening of the blades until the latter were represented by only a point. The efficiency at this stage was at least equivalent to that of a coil. It should be mentioned that though the collector was but a point the current began to pass before the rotating conductor reached it, and continued to flow

for a corresponding time after it had passed, so that a not inconsiderable portion of the top of the curve was made use of. I could not carry on with this as it brought insulation difficulties not provided for in the design, and I had to be content with a sector about one-quarter of the original length. Even then the efficiency of the machine was practically doubled, the consumption of X-ray tubes was reduced in a most satisfactory manner, and they gave better service while they lasted. Latterly, after several years of hard work and unfavourable conditions arising out of the war, it had to be reconstructed, as to its moving parts at least, and I am sorry to confess I allowed myself to be persuaded into having a disc rectifier instead of the original wooden cross-arms. The efficiency is now back to what it was, or nearly so. The disc may be sound for mechanical reasons: radiologically it is bad, as at present constructed. Personally I prefer the original arrangement.

It may be somewhat of a surprise to some that practically all the views I have expressed up to this point, and all the experiments I have tried in order to improve the efficiency of X-ray apparatus have sprung from observations with the oscilloscope and rotating mirror made as far back as 1904. I believe I was the first private individual in this country to acquire an oscilloscope, and shortly afterwards I gave a demonstration at a meeting of our parent society, the British Electro-Therapeutic Society. It was my good fortune to have the co-operation of the late Dr. Lewis Jones, and well do I remember the first evening we spent with this fascinating instrument. We were frankly mystified, yet both felt that what we had observed was of great importance, though at that time we could not appreciate its full meaning. It was often referred to in our conversations during the all too few years he had to live. On that occasion we made a note to the effect that "the important and probably only essential part of the impulse through an X-ray tube is of extremely brief duration." Probably few men made more numerous intelligent anticipations than Dr. Lewis Jones and, fortunately, he lived to see the realization of many. Though we made the above observation together at the same time and place, I want him to have full credit for it; I believe that our methods of exciting X-ray tubes will be profoundly altered in the immediate future on lines that are the direct outcome of this observation. I prefer to picture to myself the intense pleasure it would have given him to have seen this through to its consummation, in addition to the privilege I now have of paying a tribute to the memory of one of the most excellent of men.

It will be remembered that those were the days of small coils and

small tubes; the mammoth coils and the Snook machine were yet to come. When these and a host of other new things did come, together with new methods and new applications to be worked at, my oscilloscope was forgotten. It was not until I was approached on the subject of this meeting that I dug it out again and resumed investigations that should have been done years ago. It was not without some trepidation that I set it up for the first time to examine it in series with an X-ray tube and Snook transformer. In view of the wide difference in the voltage curves of this and the coil, and that the high-voltage impulse of the latter at the moment of "break" might quite well have been the determining factor in what we had observed, I had a great fear that what I was about to see might be at least difficult to reconcile with ideas firmly held for many years, if not refuting them altogether. There was no need for anxiety as it happened, and figs. 3 and 4 show the general character of the curves, if such they may be called.

The width of the initial impulse (*a*) is the image of the electrode, and does not indicate the proportion it bears in time to the whole. Rapid rotation of the mirror does not appreciably increase its width, while the rest (*b*) of the cycle, consisting of useless low-voltage current, widens out in proportion to the speed. I think we now have a complete explanation of the relative inefficiency of the transformer as compared with the coil. The initial impulse (*a*) does not reach so high as in the case of the coil, for the same mean current through the tube, nor is it any more sustained. The high reading of the milliammeter is due almost entirely to part (*b*), which also overheats our tubes and generally behaves as a nuisance. The explanation of this unique phenomenon of the passage of a high-voltage current through an X-ray tube I must leave to the physicist, who I am sure will find it an interesting if not a difficult problem.

It is now easy to understand how efficiency was improved: (1) By the use of a spark gap; (2) by increasing the resistance in the secondary circuit of my mechanical rectifier; and (3) by shortening the collecting blades in the Snook machine; all three being no more than the provision of one or more air gaps in the secondary circuit.

It should be remembered that with the relatively feeble apparatus in general use at the time these original observations were made with the oscilloscope, we were obliged to use tubes in a comparatively "hard" condition to get the necessary penetration. The gradual improvement in the construction of coils and transformers since then has enabled us

to use tubes of considerably lower resistance, with a steady improvement in results. These "soft" tubes have a lower initial inertia as well and, as might be expected, the oscilloscopic appearances are not the same. The wave does not rise so abruptly; it shows unmistakable tendency to prolongation, and its fall is less rapid. Figs. 3 and 4 were obtained with a tube rather harder than is usual under the best conditions, but in the case of this transformer the main supply is a low-voltage alternating one, the least suitable of all for X-ray purposes. These altered appearances under modern conditions of working in no way conflict with my general principles; the oscilloscope merely confirms what we already know—viz., that we are "getting on."

It being my firm opinion that the initial impulse is the only part of the cycle that matters for our purposes, this is where we should concentrate our attention with a view to increasing its height—quite feasible, as the success already attained with the "single flash" coil proves—or by prolonging its duration, which may be more difficult. So far we have only partly succeeded in giving it an appreciable time factor. If we

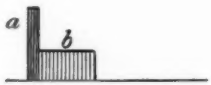


FIG. 3.



FIG. 4.

could do this, even to the extent of a very small fraction of a second, the problem would be solved so far as the requirements of medical diagnosis are concerned. Exposures would be similar to those common to daylight-photography under the most favourable conditions, and the flow of current through the tube would be such as to be more conveniently expressed in amperes, or at least in decimals thereof. Special provision will have to be made for screen examination, but radiography will be "instantaneous."

Having come to these views as a result of experience, experiment, and observation, it will readily be understood why I am disposed to regard what is known as the "single flash" monster coil with at least a friendly eye. Nothing else is possible if one is to be consistent. I have had no personal practical experience, for unfortunately there are very few examples in this country, but long before I had an opportunity of seeing one that is in regular use and the most excellent results obtained, I had committed myself to the opinion that the system was not only sound but in all probability would ultimately prevail. Having seen and

examined the working of the installation at King's College Hospital, through the kindness of Dr. Robert Knox, and the extraordinarily fine results regularly obtained there, the matter is, so far as I am concerned, at least no longer one of conjecture. The apparatus is a revelation of simplicity, silence, and high efficiency. Exposures are the same for all parts, only the intensity of the discharge being altered by the primary resistance to meet the varying thickness of the different parts of the body. The best results are obtained with tubes of very low resistance, giving most beautiful contrast, a wealth of detail, and absolute sharpness of outline, even in the case of the heart, or in the event of movement on the part of the patient—voluntary or otherwise. As regards the effect of these intense discharges on the life of the X-ray tube, this is much less prejudicial than time exposures; we have already learned this from experience with the multi-anode electrolytic interrupter. I must leave further details of this system to those who have had practical experience with it. It is my firm conviction that the future of radiography is bound up more or less inseparably with the "single flash" system, and manufacturers would do well to give this matter their very serious consideration.

In the course of these comments by way of opening this discussion on behalf of the radiologist, I have not attempted to cover the whole ground, and there are aspects of the question that must be left to others more competent to deal with them. I have indicated means by which existing apparatus may be improved in efficiency and also the problems to be faced in designing better methods of exciting the X-ray tube. In advocating what is in effect a return to the coil—suitably modified—in preference to the transformer for radiography, I do not mean to decry the latter. I have both admiration and respect for a machine that has served radiology so well, and also for its inventor. In conjunction with the Coolidge tube there is likely to be an increasing demand for it in radio-therapeutics, but I think it should be so modified as to eliminate at least the lower half or even two-thirds of the wave. This alone will raise its efficiency to the level of that of the coil as ordinarily used, as I have already proved, while its greater output will give it corresponding advantages. But if the "single flash" coil will do all we want in radiography—and there seems to be no doubt on this point—I can see no advantage for the transformer. It is no small matter to get away from large and rapidly rotating parts with their inherent liability to break down, and the constant noise and vibration, inseparable from such machines, that are so undesirable in the

examination room of hospital as well as private practice, however much they may be minimized by careful construction. And, after all, it is not impossible that the final and ideal apparatus may be quite different from anything we have at present, in which perhaps resonance or harmonics play an important part; but this is where I must give place to others conversant with these intricate phenomena.

I have been asked whether the two diagrams of oscilloscopic pictures (p. 83) are not figments of the imagination. They are nothing of the kind. They represent the appearance which you get if you take a fairly hard tube and connect it with a high-tension transformer. It is due entirely to the fact that the resistance of the tube was high. The entry of the current into the tube is delayed, and when it does enter it enters suddenly, and you get a high impulse at first, as you see. Remember that the width of that impulse is the image of the electrode—it bears no proportion, in time, to the rest of the phase—and as you spin the mirror faster and faster that image remains practically unaltered.

In opening these remarks supplementary to the paper already circulated I cannot do better perhaps than state what may be described as the text of my discourse.

Efficient radiation from an X-ray tube depends directly upon electron velocity. Such electron velocity as results from the flow of an undulating current through an X-ray tube is too low to produce efficient radiation. The radiation efficiency of any current applied to an X-ray tube will be increased directly as the character of its inflow partakes of the element of shock.

Since sending in the MS. of my paper I have been making some graphic records of what actually takes place with each electrical impulse through the X-ray tube in terms of resultant radiation. For this purpose I made a shallow trough of wood, covering the top with sheet lead, but leaving a slit approximately 0·75 mm. wide across the middle. A small frame that would readily pass along the trough under the lead cover was provided with a catch and spring release—the motive power being an elastic thread. The frame acted as a film carrier, the strip of photographic film being enclosed in black paper. In use the X-ray tube is centred over the slit, the frame loaded with a film and the elastic in tension. All being ready the tube is turned on for a second or two while the frame is released carrying the film past the slit at a speed of about 20 ft. per second which gives a convenient spacing under ordinary

circumstances. For testing efficiency in apparatus, X-ray tubes, or photographic plates and films this simple expedient is most satisfactory and gives results that are comparable with a minimum of error. I made it a rule to develop all the films exposed during a test in the same tank and at the same time. Strips about 1 in. wide are convenient to handle and leave room to cut away parts that may have been damaged during manipulation. In the lantern slides now shown narrow strips of these films are bound up between two cover glasses. These investigations have taught me a great deal, and were I writing my paper again it would be altered in some details of a minor character, but on the whole they tend to bear out my ideas very satisfactorily.

The top film (fig. 5) was made under conditions as nearly as possible the same as when Dr. Lewis Jones and myself first used the oscilloscope: 10 in. coil, motor mercury break, 24 volts, and a Chabaud tube passing about 0·75 ma. With a trifling allowance for diffusion the slit and its image on the film are exactly the same width, and the image is not appreciably wider with the film travelling at about double the speed—exactly in keeping with our original oscilloscopic appearances. As the power of the apparatus increases a second image of the slit appears immediately after the first one, but much fainter. It remains practically the same with a higher film speed though the width of the space increases, showing that there is a definite time factor here at least. The second strip is from my own coil showing four images, more easily seen in the third where an intensifying screen was used. The film speed was lower in this instance and the images closer together. The fourth strip is the result of one interruption from the "single flash" coil at King's College Hospital which I obtained through the kindness of Dr. Knox. The film speed was about 30 ft. per second. It will be noticed that the impulses are appreciably sustained and as a rule ten can be made out, gradually diminishing to zero. So far as my observations go the use of an intensifying screen does not increase the number of these images—it merely enhances the visibility of those seen without its use—and the same applies to chemical intensification after development and fixing.

This slide (fig. 6) is to show the influence of film-speed—that of the middle strip being almost exactly double that of the other two. The lowest one shows the effect of the intensifying screen, and between the low speed and the fluorescence of the screen the images have run into one another almost to the extent of continuity. The images of the slit do not widen with the increase of speed.

As we increase the distance between the anticathode and the film the images naturally get fainter, the weaker ones disappearing rapidly. The upper pair were from a coil, the lower from a transformer (fig. 7). In all, the current was 12 ma. Anticathode-film distance 5 in. for the first and third, 16 in. for the second and fourth. I happened on a poor film for these but show them because all the conditions were the same and are therefore comparable. A soft tube was used—the resistance equivalent to $2\frac{1}{2}$ in. of air—for these and the same tube for the films shown in the next slide.

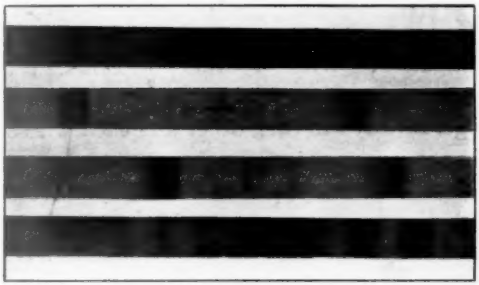


FIG. 5.

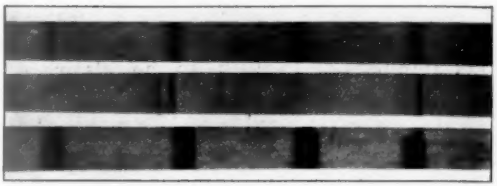


FIG. 6.

The first three strips are from a tube excited by a transformer at a distance of 15 in. from the film, the current being 20, 30, and 40 ma. respectively; the last is a coil record (12 ma.) for comparison (fig. 8). The shading bears a direct relation to the wave form and shows that only the peak is of any value for our purpose. Increase of current through the tube does not produce anything like a proportionate blackening of the film, from which it appears that the useless current bears a rapidly increasing proportion as the total current through the tube is increased. I have known for long that we do not get propor-

tionate shortening of exposures with greater current from the transformer as we do from the coil. These films are at least a help in understanding this matter.

This last slide (fig. 9) has interested me very much. The tube was excited by a 16 in. coil worked through a synchronous break from the alternating main—220 volts, 50 periods. I remember when examining this coil with the oscilloscope a few days before, the unusual image it gave in the rotating mirror. It is a difficult thing to describe, but was easily seen by those present. About one-third of the way from the commencement there was a fairly deep notch with convex sides, continually varying in width, even disappearing at times, with the fluctuations of the speed of the mirror. I was puzzled but had to leave the matter for future investigation. When I came to examine these films I saw what I think is the explanation. As I understand, these coils have two primary windings, reversed to each other and each with its own pair of contact blades in the interrupter. The connexions are such that all the impulses through the primary are in the same direction. Alternate impulses through the tube are generated by one primary winding, the intervening impulses by the other, and this film shows that the two sets are far from being equal. What I saw in the oscilloscope was the result of superimposing two dissimilar sets of waves which would give an altered appearance with every degree of departure from accurate registration. I venture to say I have found out a feature of this coil not known or even suspected by its maker. Why this great difference in the two sets of impulses? Is it due to the relative positions of the two primary windings, or, what is perhaps more likely, that the "break" in one does not take place at the best point of the wave? Whatever may be the reason I mean to have the weaker brought up to the level of the stronger—a reversal of trade union policy—and thus eliminate causes for dissatisfaction with the performance of this X-ray set. I would also draw attention to the absence of secondary images such as we found when working with direct current. I think it will be agreed that this method of investigation with the slit and rapidly moving film has a very high value for our purpose, with the added merit of extreme simplicity, and yet capable of elaboration into one of great accuracy.

In addition to the points I have briefly indicated, it shows us that we can get sustained impulses by increasing the size of our coils; it also teaches us, so far as these huge coils are concerned at least, that we must revise our ideas as to the functions of the condenser. Instead

of being a mere dissipator of energy, it is a reservoir, and by its oscillations gives rise to impulses through the secondary winding and the X-ray tube that result in useful X-rays. What I do not like about the system is the reverse impulses that must traverse the tube, to its detriment. Possibly these might be controlled by valve tubes, but this is a matter for future investigation.

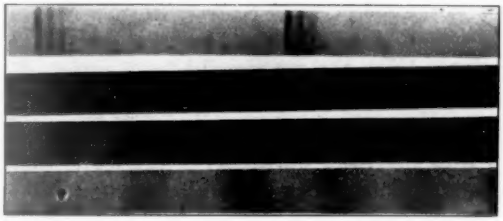


FIG. 7.

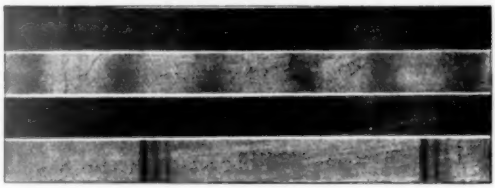


FIG. 8.

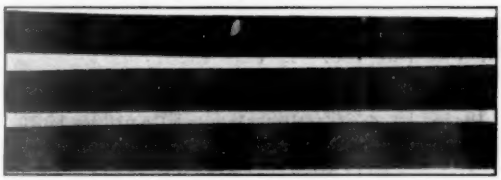


FIG. 9.

These films also seem to support my view that our chief requirement for successful work is a supply of extremely high voltage impulses more or less sustained. The higher efficiency of the coil up to the limit of its capacity is demonstrated beyond all question; and almost with equal clearness are the weak points of the transformer brought out—not the least of which is the failure of the voltage to rise with the increase

of current, so that when we increase the current through the tube we do not get a corresponding increase of electron velocity.

I think it of the highest importance that we should come to some agreement as to the real cause—personally I do not feel competent to give an authoritative opinion, but I believe it to be partly due to the self-induction and impedance of the secondary winding. I am aware that this counts for very little in ordinary transformers, but there must be a point where it begins to exert a restraining influence on output, and in the class of transformers we are considering, I believe we are well past that point. With the necessarily large number of secondary turns this impedance asserts itself long before the peak of the wave is reached, especially when drawing off a large current, so that by the time the peak comes into play, the only part of the wave we have any use for, its influence is severely discounted. By suppressing the lower three-fourths of the wave we introduce the necessary element of shock and get that sudden rush of current characteristic of the coil without its limitations.

There is another point that may be of even greater importance, and one on which I desire to have the opinion of physicists—being deplorably weak in this branch of science myself. The advantage of the sudden impression of a high voltage impulse on the tube is that the initial rush of current has to be carried by a limited number of electrons which can do so only by virtue of a high velocity. The rest of the electrons come into action immediately afterwards, and as there are plenty of them their velocity falls. With the gradual rise of the transformer current all the electrons are brought into action by the time the peak of the wave comes into play, nullifying its influence in producing the high electron velocity so necessary for our purpose. If this theory is untenable I shall be glad to be put right, but it appears to harmonize with many of my observations. It is in accordance with this idea that I am of the opinion that a continuous current would be unsuitable, even if we could get it; also that there would be no advantage in having the impulses sustained beyond a very brief period—not unlikely I am wrong in speaking of sustained impulses at all in this connexion, but at present I cannot help feeling that they might be given a just appreciable time factor with advantage. The interval between successive impulses need be no longer than sufficient to allow the electrons to come to rest. Most coils and transformers give impulses at the rate of about 100 per second, but the films I have shown indicate that the interval is very long compared with the period during which useful radiation is taking place,

and a much higher periodicity is permissible if it can be obtained. I consider it essential, however, that the electrons should be at rest at the moment the high voltage impulse arrives.

As to the most desirable wave form, a sudden abrupt rise to an extremely high peak is essential; what happens after is of no consequence whatever. It can produce only heat and useless radiation. As slide No. 8 shows, a gradual rise to the peak is hopelessly inefficient.

It being very generally agreed that a high electron velocity is essential for our purpose, we may with advantage consider the usual method of attaining velocity. Take the familiar instance of a golf ball; no golfer thinks of giving his ball velocity by means of a gradually increasing pressure—he gives it a smack! and he well knows that if the moment of impact does not synchronize with what corresponds with the peak of the curve the result will not be entirely satisfactory. This is one of the reasons why I consider the current should not flow until the voltage has risen to or near its highest point. The analogy may not be a very perfect one—few of them are, and I fear it recalls many painful memories—but it will serve. We may carry it a step further: The golfer occasionally finds his ball almost submerged in a pool of water, yet within reach. With doubtful wisdom he decides to play it without lifting under a penalty stroke. If you happen to be one of his companions in the game you will be more interested, for the moment, in what happens to the water, than to the ball, and place yourself accordingly. I want you to do so now, imagining the water to be the electrons in the X-ray tube and the head of the club the high voltage impulse. The instant the club meets the water the first particles are projected from the pool at high velocity, but as the club head gets further in the number projected increases and the velocity falls—in the end there is displacement of a large quantity of water at a very low speed. Applying this analogy to the X-ray tube, it is only the first impact that gives us the needed high electron velocity; nothing is gained by prolonging the impulse beyond a very brief period, and the sooner it is cut off afterwards the better. It further illustrates why I consider that anything in the nature of a continuous current would not suit us at all, as the electrons must be allowed to come to rest between successive impulses. Such electron velocity as would be produced by the flow of a continuous current at any practicable voltage would be too low to generate useful radiation. Just as in the drive at golf, the moment of impact is the only instant in the whole cycle that produces useful result,

and I consider that investigations directed towards increasing the intensity and frequency of impact are most likely to lead to a realization of our desires. As to frequency, we may learn something from the film made by the "single flash" coil. "Single flash" is a misnomer—the film shows the "flash" to consist of at least ten impulses, of which any number up to five produce useful radiation. The duration of the flash is said to be approximately one-hundredth of a second, so that a frequency of 1,000 per second is quite feasible so far as the tube is concerned. I am now in a position to state what I consider the ideal current for radiological purposes: simply an intermittent current with a periodicity of 1,000 per second at a voltage equivalent to the first or even the second one of the series that make up the "single flash" I have shown. This will give us everything we can possibly want for any medical purpose—at least so far as one can foresee at the present time.

I am quite prepared for disagreement with my views. Let me say, however, that they have not been arrived at hurriedly; they are the outcome of years of experiment and thought on the part of one who has no axe to grind or any particular fad to push to extremes, other than to advance the science of radiology in common with yourselves. Therefore I ask those who may feel disposed to dismiss my ideas as absurd or fantastic to first give them careful consideration; I believe they will bear critical examination so long as it is free from prejudice.

I am quite alive to the fact that my views are rather severely critical of transformers of the Snook type, but I wish to repeat what I have said in my paper, that it is far from my wish to decry this class of machines. They have served radiology well, and some very fine results have been secured with their aid. The principles underlying the system are quite sound, and I believe an almost ideal X-ray generator could be made on the same lines. I do not approve of the way they are at present constructed, and I venture to say that the maker who has the courage to produce one designed in harmony with the views I have expressed, will score a triumph. Since first using one my desire has been to improve them, and never more so than at the present time; I regret to say, however, that I have found the majority of makers of X-ray apparatus reluctant to profit by the experience of those whose business it is to use these machines for the purpose for which they are intended. The science of radiology is still in the stage of development, and the time is not yet for conservatism or standardization.

Problems of Interrupted and Fluctuating Currents.

By Major C. E. S. Phillips, O.B.E., F.R.S.E.

The velocity and quantity of the electrons in an X-ray bulb are the principal factors controlling the design of electrical apparatus required for radiography in all its numerous branches.

Since the potential difference between the electrodes of the tube determines the speed of the electron and therefore the quality of the X-radiation produced, the electrical problem resolves itself mainly into one of engineering.

The potential difference necessary is, however, exceptionally high, and the machines and appliances require to be specially designed.

Medical experience, for example, points to the need of an electron speed such that the resulting X-rays shall be as penetrating as the γ-rays from RaC. But even with a difference of potential of 170,000 volts between the electrodes of the tube we can only get a ray about half as penetrating as this.

Unfortunately the wave-length of the most penetrating γ-rays from radium turns out to be shorter than was originally anticipated, and recent experiments go to show that the minimum potential difference required to produce a similar ray from an X-ray bulb would be 600,000 volts. Even this figure can only be approximately hinted at; it may actually prove to be nearer 2,000,000 volts.

Possibly the great electrical difficulties thus foreshadowed may hearten the shareholders of radium companies, but it is nevertheless a disappointment to radiologists to find that the prospect of producing such a radiation is still far off. In addition to the requirements of medical practice there are other calls for highly penetrating rays; metallurgists are asking for a machine that will give 300,000 volts. For instantaneous radiography in medical institutes large currents are also needed at these high voltages, so that the demand in this respect is, in fact, always ahead of the supply. In some cases, the power put into the tube itself has exceeded 14 kw., or nearly 20 h.p.

This represents one end of the scale. On the other hand there is the radiography of wood and a wide range of other uses necessitating only moderate electrical plant. There are also the needs of the Army and Navy, as well as many special applications, and it therefore becomes

a question whether one type of apparatus can best fulfil all these requirements. It is, in fact, a large subject and difficult to generalize upon. In some cases the highest power is needed, and weight and portability are comparatively unimportant, whereas in other applications portability is the chief desideratum.

So far, the principle of electrical induction provides the most practical way of producing the high-tension electricity for X-ray work, but let us not be neglectful of other possible means. If we now look mainly to electrical engineers to solve the difficulties connected with the large-scale production of electricity for this purpose, there is much left for the physicist to do in trying to improve still further the X-ray tube itself. We must find some new plan whereby the energy supplied to the tube will reappear as X-radiation rather than as heat radiation. At present the tube is the most inefficient part of a modern high-power outfit.

Whatever differences of opinion the discussion may bring out as to the merits of this or that scheme of work, it is clear we shall agree at least that all present-day methods are complicated, and usually wasteful. We shall also probably agree that for work requiring a large expenditure of energy the alternating-current transformer meets the case better than any other apparatus so far devised.

One outstanding feature of the induction coil, however, is the suddenness of the rise of secondary potential to a sharp maximum when the primary current is broken. This gives less heat in the tube and is a great advantage over the more rounded peak of the secondary potential curve obtained with an alternating-current transformer. I shall refer again to this later on and venture to suggest a plan by which the transformer may be persuaded to imitate the coil in this respect. But first I should like to refer briefly to coil design.

The induction coil has much to recommend it. The simplicity and absence of moving parts are great advantages. In reality the interrupter is the "fly in the ointment" here, for therein lies our principal limitation, and our troubles are divided between it and the condenser. Curiously the shape of the condenser is of importance, for of two condensers with the same capacity the one having long narrow leaves is not so effective as one in which the leaves are nearly square. I do not remember having seen this question referred to previously, and it will be interesting to know what has been the experience of others.

It must be borne in mind that electricity supply is not universally on the alternating-current system, and an a.c. generator is an expensive

addition to an already costly outfit. For smaller installations the coil will hold its own for a long while yet, I think, and attention should be given to its design, and especially to the question of the improvement of interrupters. One of the causes of coil breakdown during the war was fracture of the wax during transport. It is suggested that a fairly thin oil could be used as an insulator, thin enough to draw off before transport, and it should be possible to replace the oil without creating air cavities. The secondary could be wound with No. 36 aluminium wire and thus save one-third of the weight of copper. An improvement from an army point of view could also be effected by using a removable core. A coil so designed would be extremely portable on account of the distribution of weight. From what has been seen of captured German X-ray apparatus used during the war, it appears that the question of dividing the several sections of the outfit, so that many small transport cases are used instead of a few very large and heavy ones, has received careful attention. If aluminium wire is used there is no need to increase its thickness owing to inferior electrical conductivity as compared with copper, because the current-carrying capacity of No. 36 copper wire is already well in excess of what is actually required. It would seem, in fact, desirable to use more turns of a thinner gauge so as to obtain a higher potential difference at the secondary terminals, but experience shows that for some unknown reason No. 36 is the best size.

The weakest point of a coil outfit is undoubtedly the condenser. Since the end of the war dozens of coils have been returned from France quite sound in all other respects. I would suggest that an alternative path should always be provided to carry off any excessive surges of current into the condenser. Recent experiments in this direction at the War Office X-ray laboratory have shown that a condenser, while in use, may be short-circuited through a water resistance (3 in. of tap water, using fine copper wires just dipped below the surface) with only a reduction of 20 per cent. of the coil output. When an oscilloscope tube replaced the water, no appreciable loss of output was noticed' although the vacuum tube lighted up brightly. Its life was short. Finally, three 16-c.p. 250-volt carbon-filament lamps in series joined across the condenser terminals proved quite satisfactory.

The filaments glowed a dull red, and irregular surges of current were indicated by momentary brightening of the lamps, but there was no appreciable reduction of coil output and the plan will, I hope, reduce condenser breakdown in future to a minimum.

Now the value of the interrupted current that may be safely used

with a coil is limited, owing to the flare when the primary current is broken. Interrupters are frequently imperfectly designed and constructed. Experiments with tantalum blades are required, for in tests already made there is indication that flare can be reduced by this means. It is interesting to notice that, although tantalum is not "wetted" by mercury, it nevertheless gives a perfect contact. The arc at break occurs in that case between the practically non-vaporizable tantalum and the mercury, and the speed of the motor assists in giving a sharper interruption of the circuit. In other cases the high surface tension of the mercury determines the suddenness of the rupture.

The behaviour of an interrupter may be conveniently studied by screwing a cup-shaped vessel into the body of the apparatus and stretching over its wide mouth a small sheet of thin rubber. The pulsations of this for variable gas pressure within can be communicated to a suitably light lever system and records taken upon a revolving drum. Experiments at the War Office X-ray laboratory show that undue flaring is immediately detected by this means, and comparisons may readily be made. Absence of flare is also necessary to ensure the efficient action of the mechanical rectifier. An attempt is being made to design an interrupter that will break 20 amp. quietly and continually without overheating. I wish to emphasize the importance of accuracy in the construction of interrupters and to direct attention to the need of careful alignment of all symmetrical parts. It may also be appropriate to refer briefly to the constant high-tension machines made by Messrs. Evershed and Vignoles and to see what prospect there is of utilizing them for the generation of X-rays.

The maximum potential difference obtained so far with any one small machine is 2,500 volts, and the usual practice consists in linking up three machines in series, each to give 1,800 volts. Thus a set for laboratory use gives about 5,000 volts, the potential difference being directly proportional to the speed. An output of 15 ma. can be obtained in this way, but insulation difficulties at higher potentials seem to exclude the possibility of developing this plan for X-ray work.

Passing over the question of measuring instruments, I come finally to some notes upon the transformer. Most of the discussion that is to follow will probably centre mainly around this means of supplying the X-ray tube with energy on a large scale. The design of transformers has reached a high degree of perfection. Their electrical efficiency is remarkable, but where very high-tension work is concerned (such as in X-ray technique) and a rectifying disc is spun

by a synchronous motor, the weak spot of the installation practically always seems to be the insufficient guarding of the armature windings from induced surges of current. But with the rectifier out of action the plant is useless, and it is hoped that this vital point will receive attention during the discussion. Earthing through a condenser, or other means of assisting the insulation of the armature to withstand the strain put upon it, is essential. The reliability of this type of apparatus in skilled hands is, however, remarkable. I know of one institution where over 60,000 radiographs have been taken during the war with a large outfit, and the apparatus is still in perfect working order in spite of the fact that about 40 ma. were used for each exposure.

One serious objection to the transformer arises, however, from the form of the secondary P.D. wave. The behaviour of an X-ray tube appears to show that the electrons are not instantly fired out from the cathode on the application of the potential difference to the electrodes. Where a coil is used, the potential difference rises so rapidly to a maximum that there is not much time for the electrons to be shot out before such a potential gradient is created that their speed is high and their number considerable. The slower rise and fall as indicated by the shape of the transformer P.D. curve give time for electrons to be ejected at speeds too low for useful X-ray work, and thus the target is heated by impacts that merely waste their energy in heat.

It is generally agreed that a steeper P.D. wave is required with transformers, and I venture to suggest a plan whereby this may be obtained for any given installation. In 1895, and about six months before the discovery of X-rays, Messrs. Barr, Beeton and Taylor published a paper in the *Electrician*[1] dealing with the question of changing the shape of alternator wave-forms. They keyed to their experimental alternator a special commutator called by them an "injector," by means of which inductance or capacity, or both, could be introduced into the circuit at regular intervals. In this way remarkable distortion of the original wave-forms was obtained without sparking.

In 1911 another experimenter constructed an alternator that gave a sharp peak on one side of the zero line and a suppressed loop on the other. It was found to work well. But in actual practice the power

[1] *Electrician*, June 21, 1895, p. 257.

has almost invariably to be taken from a main supply and we have to make the best of the wave-form given by the generating station. This is governed by several factors, the shape of the pole pieces in the alternator, the arrangement of the armature coils, &c., but it is never an accurate sine wave and generally rises first quickly, then tends to flatten a little, and finally rises more rapidly to a rounded peak and falls symmetrically. It is therefore suggested that this wave be distorted synchronously by keying an "injector" to a small synchronous motor and connecting it in series with the main supply and the transformer. In this way a wave-form could be obtained which would render the rectifying disc unnecessary. The installation would not only be less costly, but weigh less, occupy less space, and be highly efficient for the requirements of X-ray technique.

High-tension Transformers for X-ray Work.

By Russell S. Wright.

There are probably very few scientific instruments on which so little systematic research work has been accomplished as the subject of this paper. We have not yet even reached a satisfactory classification as regards either output or efficiency. As regards induction coils, which are of course a definite type of high-tension transformer, we have these as a rule merely catalogued by spark length and occasionally advertised as "intensified," a wonderful phrase suggesting that the instrument has been accorded some special treatment which mysteriously increases its output.

In the case of an induction coil especially it may be admitted at once that the problem is a difficult one. We are dealing, not with a continuous output, or even a sinusoidal wave, but with a succession of "peaked" oscillations of varying wave-form and varying periodicity, interspersed with an "inverse" discharge which must be eliminated, and in the elimination of which some of the useful output is bound to be similarly absorbed.

An advance upon the spark-gap method of measurement has undoubtedly been taken by stating how many milliamperes a certain coil will pass through an X-ray tube of a given alternative spark gap, but this classification is still anything but satisfactory. It is quite

certain that, not only does an ordinary moving coil milliammeter not measure either the peak value of the discharge or what is known as the "root mean square value," but the reading indicated may vary with different types of milliammeters and also with induction coils of varying wave-form, interrupters of different patterns, &c.

Our science will never rest upon a sure foundation until we can accurately rate both an induction coil and a closed circuit high-tension transformer in terms of their value to the radiologist, just as we can give a galvanometer a definite "figure of merit" or ascribe a certain "numerical aperture" to a microscope objective.

In approaching the problem, it is both an advantage and a disadvantage that "efficiency," as understood by an electrical engineer, matters little or nothing. I have never yet met the radiographer who cared in the very least about his electric current consumption, as the factor is a negligible one compared with economy of time, tubes, plates, &c. This, as I have said, is in one sense a drawback, as it renders any figures based upon "input" absolutely valueless, the least efficient instrument from an electrical standpoint being possibly the most valuable from that of the radiologist. The present very humiliating position is that we know next to nothing of the whole subject. We do not yet know, for example, whether a succession of high-peaked waves are better or worse than a smaller number of oscillations longer maintained; neither do we know as yet whether the tops of the peaks only are of value, or whether outputs of lower potential play their parts as well, possibly in bringing out details of the softer tissues of the body.

In the case of an induction coil, I am not convinced that we have, as yet, any means of ascertaining what the peak value of our discharge really is, as it is quite certain that the point of highest potential is only maintained for an exceedingly short interval of time, and it is, I think, a very doubtful point whether any recording instrument at present available will really measure it. There seems every indication that the wave-form most suitable for radiography is not equally so for therapy. If this is the case, the question arises as to whether one instrument can be made from which outputs of sufficiently variable wave-form can be produced at will, or whether two instruments, one for radiography and one for therapy, are necessary; or even whether the same type of discharge is always suitable even for, say, radiography alone. Do we, for example, want the same wave-form (apart from questions of milliamperes and potential) for the radiography of (1) a hand, (2) a bismuth meal, (3) a section of steel? If not, what variations in wave-form are

required, and can they be obtained from the same instrument, and by what means? These are questions on which the manufacturer has at present no definite information on which to work, and our ignorance of what is really required is at present a bigger obstacle to progress than the corresponding problem of how to obtain it.

I have no ready-made solution to offer, but I would suggest that the time is now ripe for a definite move forward, and that radiologists, electrical engineers, and physicists should, so to speak, come into

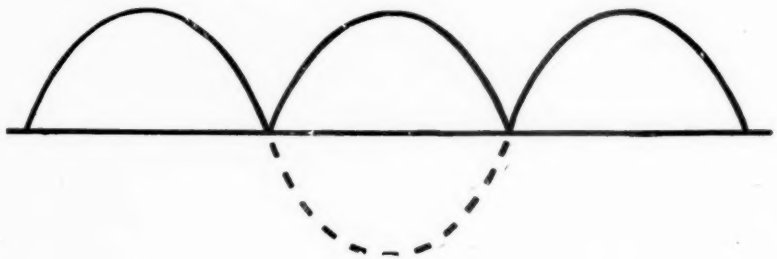


FIG. 1.

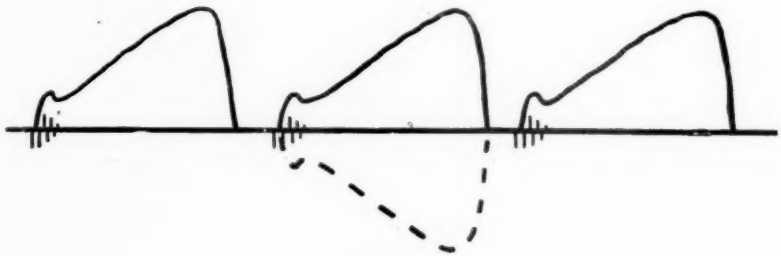


FIG. 2.

partnership in the attempt to solve a very baffling problem, and I would further suggest that the British Scientific Instrument Research Association is the proper body to co-ordinate and direct the work of the three so as to produce reliable results.

In the case of the high-tension transformer so-called—and I think it would be well if the term were by consent *not* applied to the piece of apparatus known as an induction coil—the problem is a simpler one, as we are at all events dealing with a curve of sinusoidal character, somewhat modified and even distorted, it is true, but still essentially sinusoidal.

In the original interrupterless machine as introduced by Snook, we had in fact a real sinusoidal curve with every alternate wave inverted as shown in fig. 1. In the modern machine, however, the curve has been somewhat modified and is no longer strictly sinusoidal or even symmetrical, but may be represented as in fig. 2. It is fairly certain that the direct measurement of such a current by means of an ordinary moving coil milliammeter is both more correct and more consistent than is the irregular discharge from an induction coil, where the wave-form tends to vary with the condition of the interrupter, the adjustment of the contacts, and various other causes.

It is a matter of common knowledge that the conditions of work with an interrupterless machine can be more exactly reproduced from day to day than is the case with an induction coil, and equally certain, I think, that the readings of the milliammeter are more reliable; though again it is a matter of common knowledge that a milliampere from an average coil is more effective from a radiological point of view than one from an interrupterless machine, probably, I think, because in the former case the *real* current is much more than the instrument indicates.

A closed circuit transformer also adapts itself far better to the indirect measurement of its output than does the induction coil, for the simple reason that its electrical efficiency is far higher and more easily computed. This question of efficiency, as I have pointed out before, is absolutely unimportant in one sense, but if it can be ascertained—and in the case of an alternating-current transformer this can be done with fair accuracy—it assists enormously in calculating the secondary output.

I should be inclined to suggest, therefore, that an alternating-current high-tension transformer be taken as the basis on which to work, and that an attempt be made to classify induction coils according to their effective outputs for the purpose of the radiologist and compared with a transformer of standard size and efficiency.

Assuming, therefore, that this suggestion is accepted, how should a transformer be rated? Before deciding this it must be borne in mind that what to an electrical engineer is a big overload is to the manufacturer of X-ray apparatus a normal condition, as in X-ray work machines are in practice only used to their full output for a fraction of a second at a time. An overload rating is therefore, I think, perfectly justifiable, and our earlier machines were so rated by about 100 per cent., but when a machine was put on the market by an enterprising

foreign firm, rated at an overload of well over 1,000 per cent., I felt that the position was getting ridiculous, and I notice that now practically every responsible firm refrains from mentioning outputs in kilowatts at all.

It is, however, essentially a question for an authoritative decision, and I think that the joint Societies here represented might well pronounce their opinion as to what extent overload may be allowed in estimating outputs of what are known as "interrupterless" machines. Reverting to the question of wave-form, manufacturers still want to know just what is required, and so far we have not been enlightened. For example, we were told one day that "peaked" waves are what is required, while the next day we are confronted with a demand for auto-transformer control, the effect of which, as every electrical engineer knows, is in most cases to flatten the wave-tops as compared with a transformer controlled by an ohmic resistance. Auto-control undoubtedly has great advantages when working with a Coolidge tube, but we shall not get so peaked a wave; which do medical practitioners want?

There is one more point I should like to raise in connexion with both induction coils and high-tension transformers, and that is the question of security against breakdown. It must be admitted that the "life" of one of these instruments is far less than it was ten or even five years ago, and few of the equipments being turned out to-day will permanently withstand the strain of modern conditions. The continual use of a coil for "deep therapy" is undoubtedly the most prolific cause of breakdown at the present time, and has raised problems which have given manufacturers a good deal of anxious thought.

If one considers that an output of, say, 3 ma. at a 12-in. spark-gap, from an instrument capable of giving, say, 100 ma., means that it is kept at a high potential on practically open circuit for long periods at a time, one will realize at once the problem that we are up against. Examining the exposed end of a secondary winding in this condition, we can see that it is in a constant state of effluve and that the discharge is literally spitting away in all directions.

It is impossible to look at this effluve without feeling that sooner or later the insulation is bound to go, and go it invariably does if the strain is continued long enough. Just now the position is made worse by faulty material: wax that is not wax, paper that is not paper, and copper wire that is not up to pre-war standard does not help the situation, but in any case I am convinced that a coil or transformer, constructed to give a heavy output for radiographic work and with dry

insulation, cannot continually be used for deep therapy without the risk of trouble.

Various devices can be used to safeguard more or less the end windings, but the only real remedies are two: either we must wind an instrument for deep therapy alone, or we must adopt oil insulation. The former course entails the radiologist installing two pieces of apparatus—viz., one for therapy and one for radiography. The latter will do all the work with one, but will be heavier, larger, more difficult to transport and more expensive.

Again we should like guidance from our medical friends; shall we concentrate on oil insulation or would they prefer two separate instruments? In answering this question the problem referred to before as to whether the same wave-form is needed for both purposes must not be overlooked. These are definite problems, and I have ventured to put them forward, as their solution or partial solution will do much to place the industry upon a more satisfactory footing than it is at present, and I commend them therefore for the consideration of members.

On the question as to whether we only want a very high potential and nothing more, it is well known to us that the X-ray discharge from a static machine is different from that of either a coil or a transformer. It is supposed—and I think it is correct—that the current from a static machine is almost a continuous high-peak oscillation, and the effect is extremely great penetration. With a very big static machine, I have seen, with a fluorescent screen, through six fully grown men, one standing behind the other. I have never seen such penetration from any transformer, or any coil. The penetration from a machine of that type is remarkable: but there is practically no contrast and no detail. I throw this out tentatively, because Dr. Morton and others have gone further into this research work than I have. But I think it is doubtful whether you want a certain amount of the lower part of the wave in order to give detail in the case of the softer tissues. I do not think we must rule out, right away, the possibility that a part of that lower wave may be of some use to us as well as the peak.

I now want to refer briefly to the question of safety. The development of deep therapy has thrown an enormous strain on high-tension X-ray apparatus. Coils and transformers used for ordinary radiographic purposes some years ago, which would stand, year in and year out, perfectly well, have a disastrous way of suddenly giving out somewhere when they are used for deep therapy. I have not found any particular

danger of condensers going, and I think a properly constructed condenser, with a well designed coil, ought never to go. But the question of the secondary insulation is another matter. I have brought here a skeleton coil with nothing but a primary tube, a primary and secondary winding, and without cover. I shall run that for a moment under deep therapy conditions, with the lights down, and I want you to note the brush discharge which takes place in the end windings.

We are always being blamed by members of the medical profession if anything goes wrong, and I thought an exposition of what we are up against might be of some help. I take it that a strain like that is too much for anything to stand permanently: any coil which is constructed, however efficiently, when worked under those conditions is going to give out, sooner or later, if it is constructed with dry insulation. I am quite satisfied that a coil can be made for deep therapy with dry insulation, but it will be a therapy coil only, it will not give the heavy output required for radiography. If we are to get efficiency in a modern coil or transformer, coupled with the ability to stand up against deep therapy conditions, I am convinced there is only one possible way of doing it, and that is with oil insulation. That introduces difficulties of its own: it is heavier and clumsier. But I think we are coming to this: that if the medical profession demand a combined instrument for both radiography and deep therapy, it must be oil. If they are satisfied with two separate instruments, one for therapy and one for radiography, then I am inclined to think that they can have what they want with dry insulation. But it is a serious problem which we are up against. This coil has been wound for radiography, and it will probably last for many years, but for therapy, especially deep therapy, I think you will agree, having seen the discharge which takes place, that it is almost impossible that dry wax can hold up permanently against such a strain as we have witnessed.

EVENING SESSION.

Discussion on the Efficiency of the High-tension Transformer as compared with the Induction Coil.

Professor SALOMONSON: The high tension transformer has in many cases taken the place of the induction coil in the Röntgen laboratory. This is nearly always to be found when an unlimited supply of electricity is available, either in the form of the continuous or of the alternating current. Only for purely therapeutic purposes the induction coil still largely maintains its original position of supremacy over the closed iron-circuit transformer. If we try to find why this should be, we have to consider many questions. The first is the problem of the very-high-tension generator. In radio-therapeutics the required tension is generally extremely high; we want to use only the most penetrating X-rays.

In the induction coil we have a double transformation, first, from a low tension interrupted direct current to a higher tension alternating current. This is properly a deferred transformation. Secondly, this alternating current of peculiar form is transformed into a pulsating current of extremely high potential. The first transformation asks only for an insulation of the primary circuit to stand some 2,000 volts. The second transformation needs an insulation of the secondary circuit of at least 200,000—300,000 volts according to the spark length, the transformation ratio being something between 150—300. With the closed iron-circuit high-tension transformers we have generally a rate of transformation of between 600—1,200. If it had to be used for therapeutical work and especially for deep-seated radiations we must be able to get at least 250,000 volts from the secondary terminals. This means nearly always a very large number of secondary windings to be compressed in a narrow space. The difficulties of a satisfactory insulation become very great so as to render such an instrument a very costly one, much more costly than an induction coil where the secondary windings can be distributed over a much larger space. A break-down of the insulation means costly and difficult repairs with both instruments, but, just as an induction coil can easily be produced at less than half the price of a high-tension transformer, so it is also with the costs of repairs with both instruments. Though a few radiologists seem to prefer the transformer to the induction coil for therapeutic work, I feel sure that the induction will not be supplanted by the transformer for the production of very penetrating X-rays. As in radiography we need only very penetrating rays in metallo-diagraphic work, we can expect that for general radiographic and radioscopic work the transformer will be preferred.

The construction of a closed iron-circuit transformer with a secondary

voltage of 100,000 volts does not present too much difficulty; it meets every requirement in general work except the production of the most penetrating rays, and in its present form a break-down of the insulation but rarely occurs. If we use it in preference to an induction coil we have only to consider its efficiency. Of course, there are some minor points, as the ease of working it, the form of the secondary current waves and so on, but I think all these points are of secondary interest. The question of the wave form has still some interest with gas-filled tubes but not with the Coolidge tube. Then only the question of efficiency remains. A few years ago I read a paper before the Röntgen Society in which I demonstrated some methods of measuring the efficiency of an induction coil. I could also give a few figures for the efficiency. Later on I have worked out another method, which, I think, is a distinct improvement on the former methods. The whole secondary output was again transformed into heat, and the number of calories generated per second was carefully measured. The resistance in these experiments was simply a glass tube through which water flowed in a steady current. The temperature of the water was taken before and after the passage of the secondary electric current as soon as a state of thermal equilibrium was reached. Ordinary mercury thermometers could not be used as they were spoiled by internal glow-discharges. Therefore I had a series of alcohol-filled instruments made in Amsterdam divided into one-tenths of a centigrade and very carefully calibrated. The construction of these instruments caused some difficulty as the divisions were not equally distant in different parts of the scale. But after some trouble I got a set of instruments agreeing well within 0·1° C. with a standard mercury thermometer over the whole scale. The measurement of the water-flow also proved to be less simple than I thought at first, but it was also worked out in a very simple and reliable way. The correction for loss of temperature during the experiment proved to be very simple. With this new apparatus, which was entirely insulated from the earth, and in which any desired resistance could be inserted with the greatest ease by putting in a new glass tube of appropriate dimensions, I measured the secondary output of some induction coils under different working conditions, whilst wattmeter-readings were taken about the input. As the primary and secondary currents were measured with hot-wire instruments the copper losses could be taken into account. For the high-tension transformer I was able to use a much simpler method. The secondary terminals of one transformer were connected to the secondary terminals of another transformer. In this way the first transformer was used as a step-up transformer, the second as a step-down transformer. The input of the first and the output of the second one were both measured with a wattmeter on the low-tension side. A hot-wire milliammeter was inserted in the high-tension circuit; and instruments of the same kind in both low-tension circuits. The resistance of the primaries and secondaries being known the copper losses could be calculated. The iron losses were calculated on the base of an equal division between the two transformers. I have also used this method with three induction coils.

I shall now just give a few figures for alternating current 50 p.s. :—

TABLE I.

(1) *Oil-insulated Transformer by Snook.*

Input	2·98	Kilowatt	...	Efficiency	0·903	...	Output	2·69
,,	2·02	,,	...	,,	0·882	...	,,	1·78
,,	1·10	,,	...	,,	0·864	...	,,	0·95

(2) *Dry Transformer by Koch and Stertzel.*

Input	2·83	Kilowatt	...	Efficiency	0·890	...	Output	2·52
,,	1·92	,,	...	,,	0·870	...	,,	1·68
,,	1·01	,,	...	,,	0·854	...	,,	0·86

(3) *Induction Coil by Max Kohl—12 in. Spark Lengths.*

Input	970	Watt	...	...	Efficiency	0·809	...	Output	785 Watt
,,	840	,,	...	...	,,	0·790	...	,,	663 ,,
,,	553	,,	...	...	,,	0·791	...	,,	438 ,,
,,	222	,,	...	...	,,	0·788	...	,,	175 ,,

(4) *Instantacoil—16 in.*

Input	970	Watt	...	...	Efficiency	0·788	...	Output	764
,,	807	,,	...	...	,,	0·801	...	,,	646
,,	501	,,	...	...	,,	0·799	...	,,	400
,,	222	,,	...	...	,,	0·767	...	,,	170

(5) *Therapeutic Coil, Siemens-Halske.*

Input	807	Watt	...	...	Efficiency	0·757	...	Output	610
,,	618	,,	...	...	,,	0·771	...	,,	476
,,	501	,,	...	...	,,	0·775	...	,,	378
,,	347	,,	...	...	,,	0·764	...	,,	267

With the transformers the efficiency is not quite so large as we might expect, but both instruments belonged to the very first ever made for Röntgen work. In both the quantity of iron is considerably less than we might expect in a 4-kilowatt transformer. I suppose that the newer transformers, which are now regularly made and supplied everywhere would yield better results, otherwise the figures are normal in this respect, in that they show a better efficiency with a higher input. With a quarter load these are still appreciably better than 85 per cent. With the induction coils the results are more irregular, and I have not been able to trace the cause for it. The smallest coil gave the best results. Probably this coil was loaded up to its fullest capacity whereas the instantacoil was hardly loaded to more than half the maximum capacity and the large Siemens coil to not more than perhaps a quarter load. Perhaps we have also to consider the fact that the last-named coils were much better insulated so as to be able to give at least a 16 in. spark.

The Siemens coil is used every day during at least four hours, working on a very hard Röntgen tube with a parallel spark of more than 12 in. The induction coil used as a transformer has an efficiency of between 0·75 and 0·81. But we hardly ever use a coil in this way: it is practically always fed with an interrupted current. And in this case we get very different figures, depending on the way the coil is used. If the coil works on a water resistance, absorbing both the secondary impulses, the closing as well as the opening impulse, I found the figures given in Table II.

TABLE II.

Small Coil by Max Kohl—12 in.

Primary input	Secondary output	Efficiency
74·3 Watt	50·3 Watt	0·677
130·0 ,, ...	92·7 ,, ...	0·713
Instantacoil—16 in.		
63·2 Watt	49·0 Watt	0·776
84·3 ,, ...	66·4 ,, ...	0·787
127·0 ,, ...	96·1 ,, ...	0·757
201·0 ,, ...	154·1 ,, ...	0·767

The instantacoil with its large iron core gives better results than the smaller coil, the mean figures for the larger coil being 0·77, for the smaller coil 0·70. With the Morton rectifier in the secondary circuit I found the following figures.

TABLE III.

Small Coil—12 in.

Primary input	Secondary output	Efficiency
34·0 Watt	19·3 Watt	0·568
60·9 ,, ...	36·4 ,, ...	0·598
122·7 ,, ...	74·9 ,, ...	0·610
167·5 ,, ...	105·0 ,, ...	0·637
232·2 ,, ...	157·3 ,, ...	0·678
282·6 ,, ...	198·5 ,, ...	0·703
Instantacoil—16 in.		
82·2 Watt	49·8 Watt	0·606
92·6 ,, ...	89·8 ,, ...	0·600
126·4 ,, ...	75·2 ,, ...	0·595
159·0 ,, ...	95·1 ,, ...	0·598
417·4 ,, ...	274·0 ,, ...	0·633

All these figures were obtained at the primary and secondary terminals, and represent the efficiency of the coil alone. Therefore they are somewhat misleading: we ought to consider the coil together with the interrupter. Then we find for the Max Kohl coil:

TABLE IV.

Input		Output		Efficiency		Interrupt
343·5	...	185·7	...	0·540	...	126·0
350·8	...	183·2	...	0·522	...	112·0
387·1	...	193·0	...	0·499	...	110·0
535·7	...	212·5	...	0·397	...	79·0
587·0	...	208·0	...	0·354	...	74·0

The efficiency is better with a high rate of interruptions than with a smaller number. The smaller the current broken in the interrupter, the better the efficiency of the combination. A very large amount of energy is absorbed in the interrupter, and the more so the greater is the current strength at the moment of the interruption. I suppose that especially with stronger currents a short arc is formed at the contact which absorbs a good deal of the primary energy. It does not seem improbable that some means might be found to lessen these interrupter losses.

Professor FORTESCUE: It was with some reluctance and trepidation that I decided to accept the invitation to take part in this discussion. As a radiologist, my experience is practically *nil*, but I have been in touch with developments in the last few years that may have a great influence on the future of radiography. The papers of Dr. Reginald Morton and Major Phillips have brought out one outstanding point—viz., that for practical X-ray work the demand is for the highly penetrating rays, which are produced by the quantity of electricity which passes through the tube at the high voltage. They have shown us that it is useless to pass a large quantity of electricity through the tube if the voltage is low: it only leads to over-heating.

Whether we use a coil or a transformer, some part of the discharge is at a high voltage, and some part at a low voltage: but if we use, by a method which has been developed recently for wireless telegraphy purposes, a hot filament rectifier and a condenser, I think it is possible that the whole discharge through the tube—at any rate through a Coolidge tube—could be at the high voltage. There was a point raised by Mr. Russell Wright, that the low voltage discharge was also essential, but that point I am not in a position to discuss: I can only suggest methods whereby for a Coolidge tube a high voltage discharge can be obtained. For wireless purposes, we have latterly used for the generation of high voltage direct currents from alternating current supplies, a circuit, shown in fig. 1, which is taken from a paper I have previously given before the Physical Society of London.[1] The rectifier is almost the same as a Coolidge tube. F is a heated tungsten filament, supplying us with a source of electrons, and P is the plate to which those electrons will pass when that plate is at a positive potential with respect to the filament. C is a condenser, on one side of which the positive charge collects after the passage of the electrons. On the other side of it will be the corresponding negative charge.

[1] *Proceedings Phys. Soc.*, vol. 31, v, August, 1919.

The action of the rectifier is as follows: The filament is surrounded by a mist of negative charges and during the half cycle of the alternating voltage, when the plate is positive, by the ordinary rules of the attraction of a positive body to a negative body, that charge will be drawn away and will pass across the very high vacuum space between the filament and positive electrode. In the course of the alternating cycle in which the plate is negative, the action of the negative plate on the free electrons round about this filament is one of repulsion, and consequently no current will pass so long as the vacuum is high enough to prevent a discharge of the usual kind. The result is that in spite of the alternating nature of the power supply, negative charge will be lost from the upper plate of the condenser, which will be equivalent to giving that plate a positive charge.

Fig. 2 shows, diagrammatically, the action which takes place. The round shaped wave is the wave of the voltage given by the transformer. The full line which is zig-zagging across the dotted line is the voltage that is available as a steady, or nearly steady, supply from the condenser C in the previous figure. When the voltage of the alternating current rises above this steady value, there will be a conduction of current through the vacuous space inside the tube.

The curve of fig. 3 gives some idea of the actual tube efficiency which can be obtained with a rectifier of this kind. It will be noted that it is possible, by a suitable proportion of the variables—which I have not time to explain now—to obtain something like 85 per cent. in ordinary wireless practice. That practice is a lower voltage practice than radiologists require. At the voltage with which radiologists work, that efficiency could be taken up to the order of about 90 to 95 per cent.

I suggest that this is a possible means of securing a steady high voltage supply for use with a Coolidge tube.

There is a difficulty, which I have no doubt will suggest itself to many minds, and that is, that the tube itself is subjected to the voltage of the condenser, *plus* the half-wave of the transformer when the latter is in the non-conductive direction. This is a very severe strain on the insulation. I understand that a very large number of radiologists make use of the Snook machine, and it is conceivable that with the combination of rectifier valve—and by the term "valve," I mean a high-vacuum hot kathode valve, not the gas valve which has been already spoken of—and Snook machine, that difficulty would be overcome, and there would be available a source of current which would be operative at a constant voltage, and not at the variable voltage given by the Snook machine as used in the ordinary manner, or as given by an induction coil.

Major Phillips remarked that he hoped that no one would say that "America is doing it." I am afraid, however, that with all deference to Major Phillips, I must point out that in America, in 1915, hot filament rectifying tubes were in use capable of rectifying as much as 200 ma., and working in 180,000 volt circuits. The experience I have had personally of rectifiers for this purpose

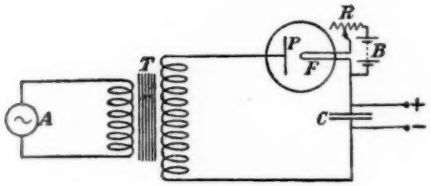


FIG. 1.

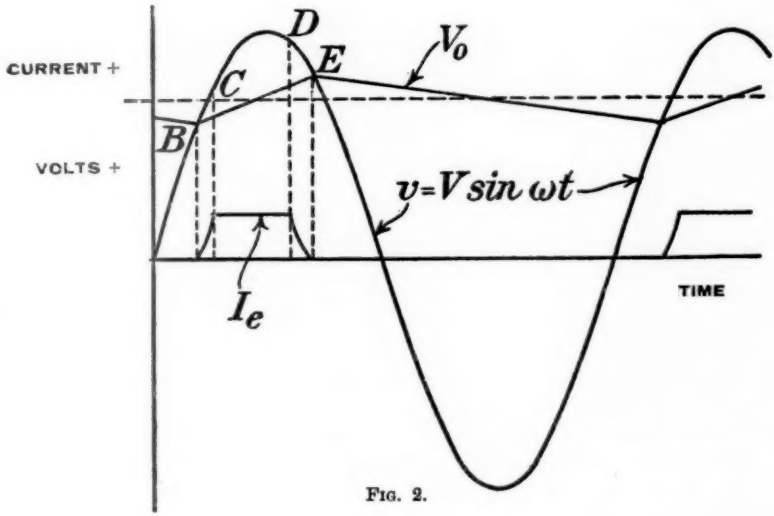


FIG. 2.

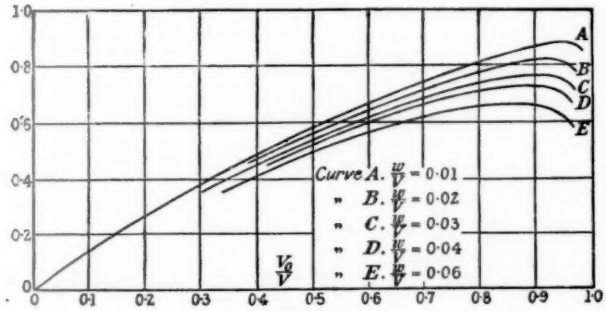


FIG. 3.

W = filament watts per ampere of electron current.
V = peak value of alternating voltage.

does not go as far as that, or anything approaching it. The highest voltage I have had occasion to work with is what would be regarded as ridiculous by radiologists, namely 7,000 or 8,000. In the course of that experience I have never heard of a rectifier tube which gave any trouble owing to the high reversed voltage. Trouble had occasionally occurred owing to the valves being overloaded in the conductive direction, but that was easily overcome and would not be a serious difficulty for X-ray work.

One further point arising from the demonstration which Mr. Wright has given to-day. The "brushing" from a high-voltage supply is a difficulty which every wireless engineer has been up against, and I would suggest to the Electro-Therapeutical Section of the Royal Society of Medicine that they should discuss this question of high voltage and brush discharge with wireless engineers who have to deal with voltages of 100,000 under very adverse conditions. The difficulty is overcome to a great extent by a careful study of the elementary laws of the electric fields.

Major G. W. C. KAYE : I would like to point out to those of you who are not radiologists how the present situation has arisen. X-rays were discovered by a physicist ; they were promptly handed over to the medical profession, not all of whom sought that co-operation with the physicist which might have been good for both parties. Latterly, I think, the situation has changed for the good, and the position now is, that radiology is hampered by lack of units and standard methods of measurement. There is, for example, no kind of standard or unit of radiation. The Röntgen Society is devoting some attention to that matter, and perhaps we shall hear of a solution before long, but we have not got a practical unit at the present time. The radiologist's methods of measuring current through the X-ray tube are, as has been pointed out already, extremely ineffective. He does not as a rule know the wave form either of voltage or of current. Again, not one radiologist in a hundred ever takes the trouble to measure properly the voltage across his tube. The average radiologist does not know sometimes within several hundreds per cent. what voltage he has on his tube, contenting himself, as he does, by measuring the spark between points. As the American Institute of Electrical Engineers has shown, the ordinary point gap may be 300 or 400 per cent. in error as regards voltage, at any rate on large gaps. They have also shown that if you want to measure high voltages accurately the handy way is to use a sphere gap, and you must use spheres as big as the maximum gap you are going to employ. I doubt if there is a radiologist in Great Britain who is doing anything like that. Very few of them, even, are using sphere gaps. I have gone into the question of measuring peak-voltages on sphere gaps, and I found that Dr. Russell had anticipated all that I could have thought of doing in that direction. I hope, in a written contribution to to-night's proceedings, to take advantage to some extent of Dr. Russell's work and to cast a very good spark-length formula that he has given into tables for the use of radiologists ; but I do not propose to go into the question in detail in these spoken remarks.

What the radiologist does need more than anything is the co-operation of the physicist and of the electrical engineer, who are used to precise measurements. We shall never make much progress unless we can get them interested in radiology, and that is what I hope will emerge more than anything out of to-night's meeting.

In conclusion I would like to point out that the kind of efficiencies we are using in the X-ray world are such as to appal the electrical engineer. You start by using either a transformer or a coil—a transformer with an efficiency of the order of 90 per cent. or a coil with an efficiency of the order of 50 per cent. The efficiency of an X-ray bulb is something of the order of one part in a thousand. In a coil-driven tube I suppose something like 20 per cent. of your rays are useful rays; the rest are of no good to you; in the case of a transformer-driven tube perhaps 10 per cent. is a fair figure. So with either method of exciting the bulb, your final efficiency is something of the order of one part in ten thousand. It is time for us to put our heads together and improve that state of affairs.

Dr. A. E. BARCLAY: It is common knowledge among X-ray workers that the current from the high tension transformer differs widely from that produced by the coil. The heating of the tube by the high tension transformer is out of all proportion to the X-ray output; and the milliamperage through the tube is no indication of the efficiency and character of the discharge from the tube. From high frequency apparatus connected to the high tension transformer the discharge is almost entirely of a brush character. There is an extraordinary tendency for the high tension transformer current to leak and char such substances as vulcanite. I have brought a series of records of the impulses from various types of X-ray apparatus, made by passing a double-coated film rapidly below a slit in a sheet of lead above which the X-ray tube is in operation. The rate at which the film is passed through, by hand, increases in each case, and you will note how the gap between each impulse widens out in consequence of this.

The records show that the impulse from the coil actuated by a mercury break give instantaneous radiographs with a time factor that is too infinitesimal to show even when the film is travelling at the rate of 40 ft. a second. Good records were obtained even with $\frac{1}{2}$ ma. passing. In the high tension transformer records the wave nature of the impulse is well shown, and also the fact that the time factor during which the tube is in operation is very considerable. The efficiency of the small impulses from a 10-in. coil is most marked, and suggests that it is the sharpness, the bang, of the impulse that is responsible for the comparative efficiency of the discharge in comparison with that of the high tension transformer. In the case of the latter machine the suggestion is that it is the duration of time during which the current is passing that is responsible for the heating of the tube. The comparison of milliamperages obtained from the coil and the high tension transformer is no guide whatever to the efficiency of the two types of machine: milliamperage is a complete fallacy in X-ray work as a guide to efficiency.



FIG. 1A.



FIG. 1B.

(*A*) Dipper break. Impulses at the rate of about ten a second, 1 ma., from a coil of 16 in. Note the sharpness of the outline of the slit which is almost, if not the actual size of the slit in the lead. As the speed increases (fig. 1, *B*) the width of the slit does not widen out, although the film was travelling at the rate of 40 in. a second. Even at this speed each image is sharply defined and is not appreciably widened out. (Flaws on the film.)

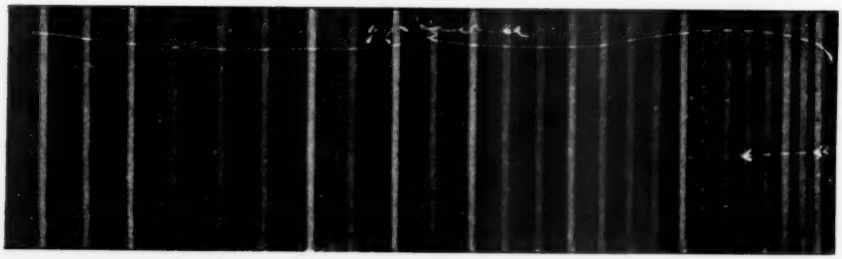


FIG. 2.

Motor magnetic break, 10 in. coil, 12 volts, $\frac{1}{2}$ ma. As the speed of the film increases the width of the image does not widen out at all. Note that some of the impulses are more powerful than others.



FIG. 3.

Motor magnetic break, 10 in. coil, 70 volts. The slit was irregular, but the shadow is exactly similar in character to the results in fig. 2. It shows how the impulses are "staggered" by the overloading of the voltage, giving a poor impulse.



FIG. 4.

Dreadnought break, 12 in. coil, 1½ ma. The break needed cleaning and was missing impulses. Nevertheless each impulse is as sharp as with the other types of mercury break.



FIG. 5.

Gunstone apparatus, 10 ma. Note the fairly sharp nature of the impulse and that there are several secondary impulses following (presumably from the condenser). If the film had passed through more rapidly these shadows would have been spread out further.

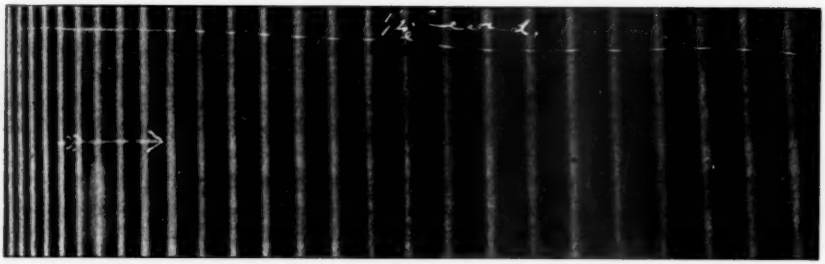


FIG. 6.

High tension transformer, 40 ma. Note how each impulse indicates the apex of the wave character of the impulse—i.e., works up to a maximum in the middle of the impulse. Also note the very marked time factor in each shadow. The width of the slit in the lead may be gathered from the narrowness of the first bands as the film moves comparatively slowly. The same remarks apply to fig. 7.

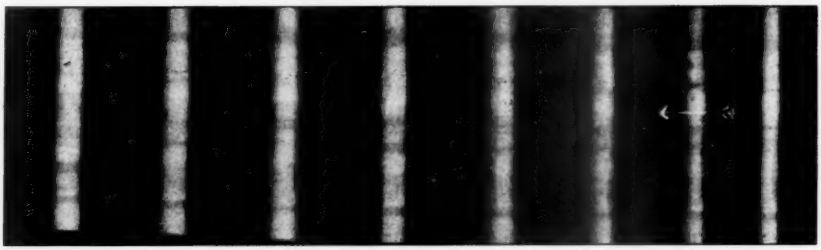


FIG. 7.

High tension transformer, 43 ma. The impulses widen out as the speed of the film increases. The slowly gathered impulse and the fading away of each shadow show more distinctly the faster the film is passed through below the slit. There is one feature of this film which calls for comment—there are streaks across in various parts of the band, and exactly identical streaks were noted in another record in which an aluminium filter was interposed. These bands were not noted in any other of the records.

Mr. R. C. CLINKER: There are several points in Dr. Morton's paper in which I was very much interested purely as an electrical engineer. I have had very little experience of X-ray apparatus for medical work—only, in fact as a sort of side line—but as an electrical engineer there are several things that strike me in connexion with the papers. With regard to the question of what is shown by the oscilloscope, I should like to know whether it is intended to show the voltage or the current that is supplied to the tube. It seems to me that it is very necessary to separate the meanings of the words "voltage" and "current" before we can get any clear idea of what we want. I notice in part of Dr. Morton's paper that he first uses the word "current" then he uses the

word "voltage," and then later on he uses the word "current" again. For instance, fig. 1, on p. 76 shows a very high peak given by the coil in the positive direction, followed by a small peak in the negative direction. If that is voltage produced by the coil I do not think it can be correct, because there is one thing we can say definitely about the induction coil, and that is, that the area of the voltage wave on the positive side at "break" must be equal to the area on the other side at "make." I do not say the maximum, I say the area. The maximum at "make" is, of course, very much lower, and you require it as low as possible.

There is a further point with regard to the comparison that Dr. Morton shows between the induction coil and the transformer that I should like to mention. I made a little experiment a year or two ago with a Coolidge tube supplied with an alternating current source with a synchronous mercury interrupter, and it gave a very interesting result, which I think perhaps is rather to the point in the comparison between the induction coil and the transformer, although it was a coil only that I was using. I started the apparatus up and I first connected it to the tube in the correct direction, that is to say, when the "break" occurred the target was made positive. The current through the tube was $2\frac{1}{2}$ ma., and there was a bright illumination of the fluorescent screen. Then I reversed the primary current, so that the *make* voltage was applied to the tube in such a direction as to make the target still positive. The result was that 10 ma. passed through the tube in the correct direction, but there was no illumination at all upon the screen. That shows very clearly the difference between a very high peak wave and a wave of a very low peak, but lasting much longer. You have four times the mean current passing through, yet you have absolutely no illumination at all, whereas in the other case, with the smaller current, you get a bright illumination. I was using a Coolidge tube in which I could of course adjust the filament current to any desired extent, but the readings were taken at the same heating. There is an additional fact, which, I think, illustrates in a different way Dr. Morton's point—namely, that a Snook type of apparatus, with rotating rectifier, with which I am acquainted, gives no screen illumination at all below a 2 in. gap, and I take that to mean that the voltage does not help medical men until it gets far above that gap. So that what you want is a high voltage, and you want your current through the tube to be passed at that high voltage. Professor Fortescue has already described the very excellent method of getting that high voltage by means of a valve, and I should like to add a few remarks to what he has said in that respect. If accurate measurements are to be made, and you have this high voltage continuous steady current applied to the tube, you want to get rid as much as possible of the ripples, and in order to do that it is necessary to use a high frequency generator of 1,000 or 2,000 cycles per second; otherwise, if you only use a 50-cycle supply you will find that your "smoothing out" condensers and inductances are of undue size. On the other hand, if you use 2,000 cycles you can produce an almost uniform voltage. Having got a voltage that you can regulate between zero and, say, 150,000, the only other thing to

do is to adjust your tube to pass the required current, and that will give you all the results you want for the present (until we get the 600 million or so volts that have been referred to!)

I was rather surprised that the question of gaps was not touched on in the paper. Major Kaye has alluded to that point, and I should like to add a few further remarks to what he said. I should like to ask Dr. Morton whether the comparisons between the induction coil and the transformer have all been made at the same maximum voltage, because, if so, one cannot help thinking that the transformer and the rectifier give a larger quantity of X-rays than the coil. If you have the same maximum voltage, the rounded shape of the transformer wave gives you a longer time at or near that high voltage than the coil which gives you a peak potential. No doubt there is a lot at the lower part of the wave which is useless, but there is also a great deal more at the top part of the wave. Supposing these voltages are measured with a spark gap, do we get that comparison? It depends entirely on the type of spark gap used. As Major Kaye said, it is useless to use a point gap if you want a strict comparison. You want to use a sphere gap which has a very small time lag. The voltage at which a pointed gap discharges depends upon the rate of rise of potential. If you have a low rate it will go over at a low voltage, and it does not give you a comparison of the maximum voltage in the two cases of coil and transformer. I am convinced that the sphere gap is the thing you want.

Mr. Wright referred in his paper to the question of the shape of the wave. He said that you could obtain a more peaked wave by using a resistance in series with the transformer. I think he is right, and that that is the direction in which one should work. Major Phillips referred in his paper to a paper by Messrs. Barr, Beeton and Taylor published in the *Electrician* dealing with the question of changing the shape of alternator wave forms. I remember that very well. They injected into the wave a capacity or inductance in order to get what shape they wanted. It is quite probable it can be done in that way, but I think a great deal can be done by working your transformer at a very high magnetic density in the core and using a resistance in series with it. You can get a very high peak in that way. It often happens that in ordinary electrical engineering we have to avoid the very thing which it is necessary to obtain, either in wireless telegraphy or in medical work. When we test insulation by a transformer we always try to avoid having to regulate the current with a resistance in the primary circuit, because you get a high maximum which is difficult to measure on a voltmeter, but for your work I think that such a transformer, with a high density in the core, placed in oil, and regulated with a resistance, would give you as good a result as the coil. There is one final word I want to say on the question of the measurement of peak voltage. There are methods by which it is possible to measure accurately the curve of your peak voltage, but they require considerable precaution to insure accuracy. I refer to the use of a hot cathode high vacuum valve—the type that Professor Fortescue spoke of. In doing some work during the war on

magnetos, I used that method for voltages up to 15,000 or 16,000. A paper has been published by Messrs. Campbell and Patterson on the precautions necessary in using that method, which consists in putting your valve in series in an electrostatic voltmeter. When the voltage rises to the peak the charge goes through the valve to the voltmeter; when it falls the charge cannot go back. I found that a very reliable method of measuring the maximum, and if research is undertaken, that is the method which should be used.

Dr. E. A. OWEN: We are dealing in the X-ray tube with two main factors, namely, the voltage and the current. The voltage on the tube determines the wave-length and the current determines the intensity of the radiation. Both these factors, in my opinion, are important in radiography. The form of the voltage curve given by an induction coil is similar to that represented diagrammatically in Dr. Morton's paper—there is one prominent peak, followed by a series of smaller ones. It has been shown experimentally by Professor Taylor Jones that the X-rays are given out at the same time that the high peak is reached. No impression is obtained on the photographic plate corresponding to the smaller maxima of the voltage curve, so that this part of the curve does not serve any useful purpose in the production of radiation. In fact, the current passing through the tube during this time only serves to heat up the anticathode, and for this reason it should, if possible, be entirely suppressed. This is partly done in certain rectifying devices where a spark between two points or two spheres immediately follows the discharge through the tube. Dr. Morton suggests, in his paper, that the efficiency of an apparatus for the production of X-radiation, should be measured by the radiological result obtained from a given number of milliampere-seconds on a pastille. This was done by Levy some time ago (see *Röntgen Journal*, January, 1917), and it may be of interest to quote his results here. The intensity of the radiation emitted by a bulb was measured by the rate of change of colour of a pastille of barium platino-cyanide, and the current passing through the tube was read on a moving coil milliammeter. There are objections to taking the indications of the milliammeter as a measure of the current through the tube, since the readings of the milliammeter depend upon the current wave form, and this is not the same for a coil as it is for a transformer. Levy found, with the coil outfit at his disposal, that the milliampere-minutes required to change the colour of a pastille to a given tint increased rapidly with increase of current through the tube, whereas, with the interruptless transformer the milliampere-minutes required to turn the pastille to the same tint were practically constant for all currents through the tube. The comparison was made with a Coolidge tube under definite conditions as to equivalent spark gap and distance of pastille from the anticathode of the tube. The curves showed that up to 3 ma. through the tube the coil was more efficient than the transformer, but for higher currents the latter was by far the more efficient. Some experiments carried out quite recently at the National Physical Laboratory on the measurement of X-ray intensity by means of barium platino-cyanide pastilles,

corroborate Levy's results with the coil outfit. In this case a standard Coolidge tube with tungsten anticathode was excited by a Watson 16 in. induction coil, the current through the primary of which was interrupted by a Sanax mercury break. The following results were obtained with the Sabouraud-Noiré pastille placed at 10·3 cm. from the anticathode. The figures in the table give the times of the B dose corresponding to certain values of the equivalent spark gap (between point and plane) and current through the tube as measured by an ordinary milliammeter. The other conditions, such as rate of interruption of primary current (about 88 interruptions per second), time of contact, &c., were kept constant.

Current	Spark Gap (between point and plane)				
	5 cm.	8 cm.	11 cm.	15 cm.	20 cm.
Milliampères	Minutes	Minutes	Minutes	Minutes	Minutes
1	36·0	13·0	9·1	5·5	3·4
2	22·8	9·2	7·6	3·5	2·0
3	18·7	8·0	4·5	2·9	1·5
4	16·6	7·5	3·9	2·5	1·3
5	15·3	7·1	3·5	2·4	1·2
6	14·4	6·9	3·3	2·2	1·1
8	13·3	6·6	3·0	2·1	1·0
10	12·7	6·4	2·9	2·0	0·9
12	12·3	6·2	2·8	1·9	—
14	12·1	6·1	2·6	1·8	—

The table shows the futility of increasing the current through the tube above a certain value. For instance, with a spark gap of 15 cm., when the current is increased from 4 ma. to 12 ma., the time of dose changes only from 2·5 minutes to 1·9 minutes, so that in this particular case when the current through the tube is trebled, the time of dose is only reduced by one-quarter of the time of dose with the smaller current. In the case of the transformer, however, under the same conditions we would find, according to the results already mentioned, that if 4 ma. through the tube require 2·5 minutes to change the colour of the pastille from tint A to tint B, then for 12 ma. it would require approximately 0·8 minute; that is, the number of milliampere-minutes would be the same in each case. It has already been pointed out that the coil has an advantage over the transformer in that it has no moving part, and furthermore the wave-form of the coil discharge is very suitable for the production of X-rays. The accessory to the coil, namely the break, is however a source of trouble. I believe there is room for great improvement in this part of the coil outfit, and it is a point which should be attended to without delay if the coil is to hold the field with the transformer. Until quite recently there was no satisfactory theory to account for the performance of the induction coil, but lately a theory has been put forward by Taylor Jones from which deductions have been made which agree remarkably well with practical results. The application of this theory to practice should lead to important developments in coil design.

Dr. G. HARRISON ORTON: I have a few remarks to make as a mere radiologist. It is, indeed, a resuscitation of the old, old story. The race between the coil and the transformer has been running for so long a time that one is beginning to wonder whether either of them will ever reach the winning post, but I hope at least that the discussion to-day will give us some information which will help to develop further this important science of radiology. Dr. Morton, in his paper, says, "The cry of the radiographer was still for more milliamperes," and it seems to me that this cry has probably been one of the chief obstacles in the path of advancement. It must be evident to all who have studied the matter that the milliamperemeter is a very misleading instrument, and is really no reliable guide to the amount of useful radiation. We say a transformer is not so efficient as a coil because the 60 ma. from it are only equal to, say, 30 from a coil, and so on. Is this correct? Why should we assume that the milliamperemeter reading on a transformer current indicates the same thing as that of a coil current? Speaking from practical experience, it is evident to me that it does not, but I will leave it to others to explain this. The maximum impulse from a coil is of extremely brief duration, much more so than that from a transformer—so brief in fact that the milliamperemeter as at present constructed is incapable of recording it. I would suggest that if the true milliamperage of a coil current could be measured it would be found that its output in proportion is less efficient than the transformer; the milliamperemeter in the case of a coil records probably less than half of the true current. Again, Dr. Morton assumes from his experiments that the curves from a transformer and from a coil are very different, but I am inclined to doubt that also. I would suggest that the curve in the case of the coil is of such brief duration that its characteristics cannot be properly gauged from what can be seen on a rotating mirror. It is an undoubted fact that a very hard tube is not good for radiography. What then is the use of the extra height of the coil curve, and at what point on this curve do we get the most useful radiation? Surely the peak is of value only when we are using a very hard tube, as is the case in radiotherapy. If, then, we are agreed that a hard tube is not required for radiography, why is the top of the transformer curve of less value? With regard to the extra heating effect of a transformer current, may not that be explained by the fact that in the case of the transformer there is a shorter period of rest, so to speak, between each curve?

The explanation of the superiority of the coil over the transformer, Dr. Morton says, has puzzled many radiologists, few more than himself. I admit it is very difficult—I might say almost impossible—to explain a superiority which probably does not exist, for I firmly believe that, if again we can only bring ourselves to disregard the milliamperemeter, or at any rate not assume that that instrument is recording the same thing when connected to a transformer circuit as when connected to a coil circuit, and judge by the effect, say, on a photographic plate, we shall find that this assumed superiority of the coil is non-existent. It is true that the potential of a coil capable of bridging 12 in. or more of air is enormous, but, when we connect this to a tube that does not

require more than, say, a 6 in. gap, does this total potential come into play, and are not the potential of a coil and the potential of a transformer operating a tube of similar spark gap practically the same? Does not the potential drop to the requirements of the tube? If not, what does happen? Why does not the current bridge the 12 in., or, say, even the 6 in. gap? In other words, is not the high voltage impulse of a transformer sufficient for radiography, and is not the useful part of the coil wave somewhere between its highest and lowest points? In the case of therapy our arguments are quite different. There we require the most penetrating rays we can get, and no doubt the peak of the wave is the most useful part of it. Personally I do all my therapy with a coil and radiography with a transformer, and it seems to me that that gives the most satisfactory results at the present time. The bulk of the transformers in this country will not stand the constant overloading necessary to obtain suitable rays for deep therapy—though I have no doubt they can be constructed if necessary—and it is more difficult to get a transformer repaired and replaced in a short time than a coil. Further, with the double installation, if one breaks down the other is available and the work is not arrested.

Mr. Wright, in his paper, implies that radiologists do not know what they want. We do not pretend to know how what we want is to be manufactured, but I think we may say that for radio-therapy we require a machine that will give us a current of very high potential, capable of operating a tube having a 12 to 14 in. spark gap; furthermore, it must be able to stand such potential for long periods without breaking down, and the current must be sufficient to enable us to give a full dose through 4 to 5 mm. of aluminium in a reasonable time, say four to six minutes, or less. In the case of radiography such high potential is not necessary, but we want a machine that will enable us to obtain radiographs of the chest at a greater distance from the tube and in less time than is at present possible, and without an intensifying screen. In designing such a machine, I would suggest that the type of tube to be used must be taken into consideration. The current through a gas tube is dependent on the ionization of the gases in it by means of a high voltage current, and therefore an initial inertia of very high potential is necessary to produce this ionization. In the case of the Coolidge tube, this, of course, does not apply, and there is nothing gained by building up a coil of a higher potential than is actually required to operate the tube; what we want is sufficient potential and more current available at this potential.

Mr. BURNAND: I think the difference in the results obtained from the X-ray tube, excited by means of a coil, and alternatively by a transformer, is very largely due to the great rise in useful rays from a tube as the input is increased. If you take it that the output of useful rays varies as the cube of the applied voltage, you are likely to be under rather than over as a first approximation. If those curves of potential shown on the screen are plotted out on that cube basis, you will see the upper parts of the voltage curves are

the only parts which matter as regards production of useful rays; the other chiefly heats up the tube.

Major Phillips gives us some rather tough figures to work to, if we are asked to supply transformers of 2,000,000 volts. I do not know much about X-ray tubes, but I know enough about transformers to say that if anyone asked me for a 2,000,000-volt transformer I would refer him to somebody who knew very much more, or very much less, than I do about them! I believe a 1,000,000-volt transformer, or possibly one or two of them, have been made in the United States, and I believe they have worked at a million volts, though not for long. They would prefer to work them at 500,000 volts, and even that is a great undertaking. I have seen insulators for one of these 10 ft. long. For a 2-million volt, they would have to be about 30 ft. long. And, taking a 1-million volt transformer, described some time ago in the technical press, and judging by dimensions, it would cost something over £10,000, and probably would weigh over 20 tons. So I think those are out of it, for the time being.

The only hopeful part of the business is provided by the demonstration, which is, I consider, clear from the curves which have been thrown on the screen, showing the very short duration for which this high voltage is required. It is evident that the amount of energy required in the tube is a comparatively small item. Although it seems impossible with present-day materials to keep that high voltage for any considerable period of time on the transformer, we might develop an apparatus which would maintain it long enough to give the required dosage for the tube; I think one two-hundredth of a second would be long enough. The apparatus I would propose for that would be, substantially, a transformer, but with an air gap in the magnetic circuit, and excited by a small direct current. By that means you can have a reasonably sized transformer, and you could pass for instance a direct current of about 500 watts for a matter of five seconds. That would be enough, and the energy represented by that would be mainly stored in the air-gap, could be discharged when the primary of the circuit is broken in one two-hundredth of a second; it would give you well over 200 kw. for that short period of discharge, allowing liberally for all losses, and I think it will satisfy any man owning a tube for a long time.

My object in bringing forward this proposal to-night is, that I am constructing such an apparatus. It will weigh about 2 tons, so it will not be very portable. Still, if any enthusiast would like to bring his tubes and test them, when it is ready, I will risk the transformer. We should get a show in any case, because we can regulate it; we have it under perfect control. I have a variable air-gap, so that we can vary the intensity of the discharge as well as the quantity, and we can start at low value, and increase it until one or the other gives out. A useful feature is that with a given air-gap and a given primary current there is always the same amount of energy stored in the system, and discharged when the primary circuit is broken.

The CHAIRMAN: I hope someone in the audience will accept Mr. Burnand's offer.

Mr. F. J. HARLOW: I think the first thing which should be taken into consideration in designing a generator for high tension current is the X-ray tube. It is most important that we should decide definitely whether the gas or the hot cathode type of tube is going to be used, because there is a distinct difference in the characteristics of the two tubes and it is conceivable that the machine employed to develop the high tension current will have to be different in the two cases. I am convinced at any rate that little progress can be made in research by using the gas tube as one cannot control the various factors independently; for research, the hot cathode tube is indispensable in order that the conditions may be definitely fixed.

Slide No. 1, shows roughly the difference in the characteristic curves of the gas and hot cathode tubes. Curve (*a*) shows the way in which the current changes with voltage in the case of the gas tube, excepting probably the lower part which shows the kind of thing we should get if there were some source of ionization in the tube. As the voltage rises the current attains a saturation value, and it is not until we get ionization by collision in the tube that the current rises. It is the last portion of the curve—the portion corresponding to ionization by collision—that we have to deal with in the gas type of X-ray tube. In the hot cathode tube, on the other hand, we have a curve of the type (*b*) or (*c*), the former corresponding to the higher temperature of filament. For the lower voltage, the current is quite small, but it attains a certain saturation value which depends upon the temperature. If the saturation stage is reached, however much the voltage is increased, the current does not increase, a result quite different from that with a gas tube. It is thus conceivable that the most suitable wave-form may depend upon the type of tube used; at any rate, the characteristic curves must be borne in mind in interpreting any results such as those given by the oscilloscope. The paper which Dr. Morton has contributed describes the oscilloscope as indicating voltage; the oscilloscope indicates current approximately, not voltage, except in so far as the former is dependent upon the latter. The characteristic curves already shown reveal that the current is by no means proportional to the voltage.

The next slide, No. 2, shows the result of an oscilloscope experiment I carried out after reading Dr. Morton's paper; it illustrates the point extremely well. The experiment was performed with a high-tension transformer kindly lent me by Sir Archibald Reid, and I believe it was one of the first to be made in this country. A hot cathode tube was used and for the lower voltages, curves approximating more or less to the sine form were obtained. As the voltage gradually rises, the indication of the oscilloscope follows the sequence of curves shown until at a certain stage the top begins to flatten out, and however much the voltage is increased beyond this point the current does not rise further, but we get an absolutely flat top at a constant height, the curve broadening out until its sides become almost vertical. From the character of these curves and bearing in mind the shape characteristic of the hot cathode tube, one would think the dotted lines were voltage curves, but, owing to the saturation stage having been reached, they are not indicated in the

oscilloscope. As a matter of fact, though I have not had time to take definite readings, it is quite obvious that if we plotted length of glow on the electrode of the oscilloscope against applied voltage we should have obtained the second curve shown in this slide which is similar to the characteristic curve (*b*) of the first slide.

Slide No. 3 shows some oscilloscope indications with the instrument in series with a hot-cathode tube excited by a 16 in. coil. I want to show you that the same sort of indication is obtained in this case. The first figure is for a low voltage. We get the initial peak, of which Dr. Morton makes so much, but the curve quickly tails down to zero. If a spark gap is introduced into the secondary circuit the effect, as shown in the second figure, is to cut off the tail, which means, as Dr. Morton concludes, the cutting off of low voltage current. It will be noticed that the milliamperage is reduced, although no alteration in the heating current was made. If the tube is hardened up by increasing the primary current, you will notice that the curve assumes quite a different form, the maximum height being the same as before—viz., that corresponding to the saturation current. The height was in fact, for the same heating current, exactly that obtained with the high tension transformer. The effect of a gap in the secondary is much the same as before, but there is the same saturation current with, however, a slight initial peak, not visible with the gap absent. I think this is due to an accumulation of electrons during the period of rest, and the more sudden application of the high voltage with the spark gap in the secondary gives a higher current-density for a very short interval, after which it settles down to the saturation current until the voltage again falls below the saturation value.

Slide No. 4 shows a similar experiment with a gas tube, and it is seen that a different result altogether is obtained. I need not go into details, for the sake of time. The various conditions are indicated on the slide. There are oscillations taking place after the initial maximum has been reached, and the height of the curve is very different for different conditions, a result which is to be expected from the nature of the characteristic curve. So, really, I do not think the oscilloscope can be taken to indicate voltage at all well; but the experiments I have made certainly agree with Dr. Morton's in showing that the effect of the spark gap in the secondary circuit is to cut out the low voltage current. I think the proper wave form is one in which the voltage rises immediately to the high value corresponding to the requisite penetration of ray, remains there for the time it is required, and then returns to zero, so that you do not get any of those low voltage currents passing at all.

It does not follow that there will be no soft radiation under these conditions, as seems to be supposed by many. Slide No. 5 shows the form of the continuous X-ray spectrum obtained with a constant voltage of 40,000 volts applied to the X-ray tube. The curve is copied from a paper by Duane and Hunt. You will notice that ionization as a measure of intensity is plotted vertically against wave-length horizontally. Although we have a constant

voltage applied to the tube, it does not follow that we are getting homogeneous rays; in fact we get a whole spectrum, and it is just the minimum wave-length of this spectrum which is related to the voltage by the well-known quantum relationship.

There is one other point of interest upon which I would like to touch, and I think it is important. Mr. E. J. Evans has carried out in my laboratory a series of experiments, the results of which are not yet published, but which he has kindly permitted me to mention. The quality of X-radiation from tubes excited by different machines has been examined by absorption methods. Slide No. 6 shows the type of result which is not altogether new except that the curves have been obtained under more practical conditions than previously. Similar results have been obtained by Russ and by Rutherford. The logarithms of the intensities of the radiation getting through various thicknesses of aluminium are plotted, and it will be observed that after absorption through 10 or 12 mm. the curves become straight, which indicates that the transmitted radiation is sensibly homogeneous. It would therefore appear that after transmission through the human body, assuming the absence of secondary characteristic radiation, or through a thick filter, the X-rays approach homogeneity. I do not think this fact is sufficiently well realized by radiologists.

Slide No. 7 shows the type of absorption curve, the ordinates in this case being a measure of intensity, and it will be seen to what a large extent the radiation is cut down by 10 mm. of aluminium. In the example given in the slide, taken from results with the American army pattern portable transformer, it amounts to as much as 94 per cent.—i.e., only 6 per cent. of the emitted radiation is transmitted through the aluminium.

We have been trying to correlate the hardness of the homogeneous end radiation obtained from various machines with the voltage applied to the tube, but without success. For the same spark gap different hardnesses of end radiation are obtained with different machines. We have used a coil, a Snook transformer, and an American portable transformer. Spheres of 5 cm. diameter have been used for measuring the equivalent spark gap. It appears, therefore, that the spark gap is not a proper indication of the hardness of the end radiation.

However, if we take a different criterion—viz., if we take the same hardness of end radiation for different machines, we find, on plotting out our absorption curves, that they are coincident within the limits of experimental error. It would, therefore, appear from this result—and we are hoping to investigate the matter more closely—that we get the same quality of radiation emitted from the tube by whichever type of machine we excite it, so long as the conditions are such that the same end radiation is produced. The surplus energy supplied, which is different apparently with the various machines, is absorbed in the tube, and we have the same radiation emitted. The difference in the amount of energy supplied is probably the low voltage energy referred to earlier, which does not produce any useful X-rays. I think Mr. Evans'

observations, if they prove ultimately to be correct, are extremely important, and will help to elucidate the problem of X-ray production.

In conclusion, I may say that I think the future of the question under discussion to-night depends very largely on the physicist. I do not think we are really in a position yet to tell the engineer exactly what we want. I would like to make an appeal to the manufacturers of X-ray apparatus. I think if they would lend apparatus to physicists who are working in this direction a great advance could be made. The difficulty at present is that with our limited types of apparatus we are not able to vary the conditions of X-ray production sufficiently to enable us to ascertain in what direction improvements should be made.

Mr. H. M. DOWSETT (communicated): The important fact which has been established is that the milliampere discharge from an induction coil is much more efficient for radiography than the same current from a transformer. Mr. Wright is inclined to blame the induction coil ammeter for reading low, but there is no reason why this should be the case provided a hot-wire instrument is used with a period which is long compared with that of the coil interrupter, and it is constructed so that it is absolutely independent of frequency, instruments of this type being now easily obtainable. One advantage of the induction coil is that the highly damped discharge involves superposed current frequencies ranging from 500 cycles to 5,000 cycles and possibly higher, so that the initial current and potential rises are very steep and a minimum amount of energy is therefore wasted in heat before the potential applied to the tube reaches a value which is useful for radiography. The X-ray tube appears to be triggered by a fifth or ninth harmonic of the natural period of the coil secondary, while the working current is provided mainly by the fundamental and third harmonic. Another advantage is the interval between the discharges which enables the X-ray tube partly to de-ionize and also to cool. The result of this is that at the first moment of each discharge the electrons have an extra high speed, and as the temperature-rise is reduced the tube has a longer life. The interval is, however, needlessly long, particularly with large coils, and one object of design should be to lessen it. Before proceeding to offer some criticisms of suggestions put forward by one or two speakers, I should like to give my conception of how an induction coil works. A discussion of this subject should not be out of place here, and ought to be beneficial, particularly as no complete theory of the machine has yet been evolved, although the useful investigations of W. H. Wilson in 1912 and E. Taylor Jones in 1914 may be mentioned as having added to our general knowledge. My conclusions are based on the results of some recent tests which I have made on a Marconi 10-in. coil. The induction coil comprises three tightly-coupled oscillatory circuits: (1) The secondary circuit which has a fixed period and is considerably damped, and comprises the secondary winding. (2) The primary circuit which has an adjustable period and less damping than the secondary and comprises the primary winding and the coil condenser. (3)

The interrupter circuit which has an extremely short period and high damping, and comprises the interrupter condenser, the leads to the interrupter, and the interrupter itself across which a discharge takes place as long as the direct-current arc at the break continues. The secondary winding on open circuit oscillates as a coil with two free ends; the primary winding on spark load as a coil with one free end, that connected to the condenser; the interrupter circuit with one side of the condenser (that connected to the mains) practically earthed, while the other side makes a strong capacity coupling to the primary winding. Energy is first stored up in the form of a magnetic field which cuts both the primary and secondary windings by passing a direct current through the primary. It is then released by the stoppage, more or less complete, of the direct current owing to the opening of the interrupter contacts, and back electromotive forces induced simultaneously in both circuits cause in-phase currents which flow in one case into the interrupter condenser, and in the other into the self-capacity of the winding. If the coil is to produce the longest spark possible in the first oscillation the whole of the magnetic field must be available to be taken up by the secondary winding. This means that the magnetic energy taken up by the primary must be given up again to the iron within the period of the first oscillation of secondary current so that it can feed this oscillation. Such an adjustment is obtained when the primary circuit has half the period of the secondary circuit. The primary oscillation then dies out feeding the secondary oscillation, being also damped by the resistance leakage across the interrupter contacts, and if a suitable spark discharge takes place all the coil energy disappears. But if the discharge is not complete and current remains in the secondary winding, it forces an oscillation at its own period back on the primary, which results in a leading current, high potential on the interrupter condenser, and a knocking break. As the interrupter contacts open and the direct current falls the back electromotive force produced by the falling magnetic field besides creating a rising condenser current also tends to maintain the direct current by an arc between the contacts. The sudden charge of the condenser, however, starts a high frequency current in the interrupter circuit which is superposed on the arc, upsets its unidirectional character and finally destroys it by its periodic oscillations through zero. I have not been able to trace any effect in the secondary circuit which can be said definitely to be due to the direct action of these high-frequency currents. Indirectly, of course, the more effectively the arc is quenched the more magnetic energy is available for transformation into secondary current. Given a properly adjusted coil the maximum secondary voltage, and therefore the spark length, is proportional to (1) the magnetic field and therefore the primary ampere-turns; (2) the proportion of this field cut by the secondary, and therefore the coupling between the two windings; (3) the rate at which the field dies away, and therefore the natural frequency of the secondary circuit; (4) the inductance of the secondary winding; (5) the inverse of the decrement of the secondary circuit. The sudden method employed of starting oscillations in the two windings is a form of shock excitation and therefore results in the

oscillations being full of harmonics. When a spark is taken at less than the maximum the current is not given time to rise to its full value, and the oscillations show the frequency of the harmonic corresponding to the shortened period. Some values obtained from the Marconi 10 in. coil experimented with are: Secondary winding inductance, 470 henries; secondary winding self-capacity, 210 cm.; secondary winding resistance, 6,650 ohms: secondary winding natural frequency, 480 ~ per second; critical damping resistance, 2·84 by 10^6 ohms; coupling with primary circuit, 70 per cent.; proportion of total field taken up by secondary on open circuit, 41 per cent.; proportion of total field taken up by secondary on spark load, > 41 per cent. and < 70 per cent.; primary winding inductance, 0·0375 henry; primary winding resistance, 0·2 ohm; capacity of interrupter condenser, 0·7 mfd.; primary circuit natural frequency, 984 ~ per second; critical damping resistance, 462·6 ohms; frequency of primary circuit oscillations from oscillogram, about 500 ~ per second: interrupter circuit natural frequency, 265,300 ~ per second; interrupter circuit inductance, 0·5 micro-henry; critical damping resistance, 1·73 ohms; interrupter vibrations max. spark length, 45 per second.

When sparking with a 4 in. bulb X-ray tube on 2 in. point spark: interrupter vibrations, 75 per second. Frequency of primary circuit oscillations from oscillogram, about 1,500 ~ per second.

Some hard and some soft 4 in. X-ray tubes were tested and found to have resistances when un-ionized from $1·5 \times 10^{12}$ ohms down to 3×10^{10} ohms and, when glowing, resistances which ranged from 15×10^6 ohms down to $1·5 \times 10^6$ ohms. The electrostatic capacity of such tubes is about 20 cm. It is clear from the above figures that if the spark discharge has a greater resistance than 3 megohms it will be entirely unidirectional, if it has a less resistance the discharge will be oscillatory, and in the case of some of the X-ray tubes tested each train should contain a few oscillations. But it must not be overlooked that as long as the electrostatic capacity of the tube is small compared with the self-capacity of the coil, there will be an oscillatory current in the secondary winding independent of whether the discharge is oscillatory or not. The resistances of air-gaps and tubes become less as ionization proceeds, and the second and third secondary winding oscillations may under certain conditions be quite useful in providing additional energy for the discharge at a lower potential. It should now be clear why a smaller wire than No. 36 copper, as mentioned by Major Phillips, does not give better results in the average coil. The same length of wire will take up smaller space so that both the self-capacity and the inductance will be greater and the resistance will also be greater. The period will therefore be slower, the maximum terminal potential difference and the secondary current will be less and the damping greater, and a larger condenser will be required across the interrupter. If aluminium wire is used which has the same gauge and insulation as the copper, the only constant altered will be the internal resistance. The maximum spark and secondary current will be less, and the heating in the winding will be greater, and the liability of the wire to break from ageing and mechanical shock will also be increased. The suggested use of oil for coil insulation would

involve the use of a metal container. This would cause an increase in the capacity of the secondary winding, leading to a decrease in spark length, an increase in oscillation period, and, for best results, an increase in the capacity of the interrupter condenser. In comparing the induction coil with the high-tension transformer it is not fair to either to disregard the opposite principles by which they work. The induction coil is essentially a shock machine and gets rid of all its energy in the first kick of the current. The transformer either transmits the same amount of energy to the circuit every cycle, or else by the aid of resonance gradually works up the energy in the circuit to a maximum. An attempt to combine the two machines in one by injecting a condenser into the transformer circuit is likely to lead to the sacrifice of the best points of both of them. From the films shown by Dr. Morton, the King's College single-flash coil appears to have a period quite comparable with that of the supply used for the high-tension transformer. As each discharge in a spark train on the coil film appears to grow and fade away gradually, the wave-form must be more sinoidal and the oscillations less damped than is usual. This is to be expected from the size of the coil and its consequent high inductance. Finally, as regards the measurement of peak voltage, if the point gap is likely to give erratic readings, the sphere gap from its capacity is likely to introduce an error. A gap between two spheres each 10 in. in diameter and 5 in. apart will break down at 200,000 volts (maximum), but the capacity of these spheres will be 273 cm. which is quite comparable with the self-capacity of a coil likely to provide such a spark and will therefore reduce the coil terminal potential difference. To avoid such error the sphere gap could be charged through a suitable battery of high-tension thermionic valves which pass no reverse current, using a similar circuit to the one employed by Messrs. Paterson and Campbell[1] for the measurement of magneto peak voltage, except that the sphere gap would replace the electrostatic voltmeter.

Mr. H. S. HOLBROOK (communicated): It is interesting to note that Dr. Morton and Major Phillips consider an induction coil preferable to a transformer, and that the reason suggested is the form of the secondary E.M.F. wave as shown in fig. 1 of Dr. Morton's paper. If the penetration is proportional to the maximum value of the voltage it is easy to see that such a curve improves the penetration; but why should it give more actinism? It would be useful if comparative tests were made on some other wave-forms, using for example a static influence machine or the new high-tension machine of Messrs. Evershed and Vignoles mentioned by Major Phillips. Have either of these authors ever obtained curves of the current through an X-ray tube by an oscillograph, cathode-ray tube or otherwise? These curves could help to explain the different heating of the tube when excited from a coil or from a transformer. The curves would not be the same shape as the voltage curves seeing that an X-ray tube has not a constant fixed resistance throughout the voltage cycle. With a constant resistance the heating is proportional at every instant to the

[1] *Philosophical Magazine*, 1919, xxxvii, p. 301.

square of the current at that instant, and over a complete cycle of current the heating is proportional to the R.M.S. value of the current. This is usually a very different value from that shown by an ordinary milliammeter, which measures the average value of the current. Have either of the authors used in their experiments an instrument giving R.M.S. values of the tube current such as a thermo-ammeter? The wave-form shown in fig. 2 of Dr. Morton's paper is typical of a single-phase transformer and a mechanical rectifier. Has Dr. Morton ever tried a three-phase transformer and a set of six kenotrons (hot-cathode valve tubes) or other suitable rectifier? Such an arrangement gives a wave-form with a variation of only 13 per cent. from maximum to minimum—i.e., it varies from a maximum down to 87 per cent. of maximum value and then back again, unlike the single-phase arrangement which varies from a maximum to zero. In consequence the hardness of the X-rays from a tube excited from such a three-phase set would be very uniform, practically all the soft rays being eliminated. Moreover, for the same maximum voltage the three-phase transformer set would pass approximately 50 per cent. more current through an X-ray tube than a single-phase transformer operating under the same conditions would do. Would not this be a great advantage in therapy and the radiography of metals and help to shorten exposures? I do not think there will be any difficulty in building transformers for any voltage that future developments may require. In the testing departments of cable makers and the big manufacturing companies there exist already many high-voltage transformers. For example, I have a photograph here of a single-phase transformer in the high-voltage laboratory of the General Electric Company of Schenectady, U.S.A. (makers of the Coolidge tube) which transformer is rated at 750,000 volts (R.M.S.) or for a peak voltage of over 1,000,000 volts. That transformer occupies a floor space of 13 ft. by 8 ft., is 15½ ft. high to the top of the tank, and 28 ft. over the terminals. These dimensions are large, but are necessary to prevent the disastrous "effluve" mentioned by Mr. Wright. I can assure him that the average customer of power transformers will not tolerate the least sign of effluve or corona. It is common practice to test the terminals of such testing transformers by applying 50 per cent. over normal voltage in the dark. Of course the customer has to take the manufacturer's word that things are all right underneath the oil, but some customers call for the drawings and choosing a part they think looks doubtful pay for a full-size model of that part to be made and tested in a tank under oil, and again in the dark but with otherwise unimpeded vision, to see if they can detect the slightest sign of leakage. Another photograph which I have here shows a smaller transformer by the same company, rated at 300,000 volts (R.M.S.), and a further photograph shows an arc between terminals 43 in. apart. The arc was produced at an R.M.S. voltage of 362,000 or a peak of 510,000 volts and was 25 ft. long. I gather that the milliammeter reading of the current through the X-ray tube is no certain measure of the quantity of X-rays produced. Some years ago I heard of an "intensimeter" using a selenium cell which was said to indicate quantity directly. Have the authors had any experience in the use of such an instrument?

Section of Electro-Therapeutics.

President—Dr. A. E. Barclay.

The Treatment of Exophthalmic Goitre by Radiations.[1]

By Arthur Burrows, M.D., and J. M. Woodburn Morison, M.B., C.M.

For the purposes of this paper except for certain obvious factors we regard the effects of X-rays and radium as identical. The differences of usage are mentioned later.

Definition.

In this paper exophthalmic goitre is regarded as a well-defined disease in which the hyperthyroidism is definite, persistent and pathological and not a slight over response to a physiological or natural need.

Classification.

It is not easy to define the exact border-line at which functional hyperthyroidism ends and exophthalmic goitre begins, but usually before the patient comes to the radiologist he or she has been for some time under the treatment or observation of a physician who, finding the symptoms either increasing or intractable, refers him or her to the radiologist with a view to X-ray or radium treatment. In this way it is not as a rule difficult in British radiological practice to recognize a case of true exophthalmic goitre.

We have not been able to discover or to think out any classification of this disease on a sound pathological or anatomical basis. We believe

[1] At a meeting of the Section, held March 19, 1920.

that at present there is none. So that for general purposes the rough and ready division into severe, medium and mild cases is all that can be attempted although there are a few patients who from the point of view of ray treatment can be put into a sub-group of their own. These will be mentioned later.

Ætiology.

As the causation of the disease has an important bearing on its treatment and prognosis in relation to the ray treatment, we have divided the cases into the following groups from that standpoint, viz. :—

(1) Cases which are the pathological continuation of a physiological process such as the pathological conditions following pregnancy and childbirth. These may overlap the next two classes.

(2) Cases following shock, which initiates the disease through the sympathetic nervous system. These may be subdivided into: (*a*) Those following a shock or strain which is mainly psychical. (*b*) Those following a shock or strain which is mainly physical. (*c*) Those in which shock accentuates or brings out a slight degree of exophthalmic goitre already existing. It is also probable that when patients are in a state of physiological hyperthyroidism they will respond excessively to sympathetic stimulation and thus the disease may become established. This may account for the greater prevalence of the disease in women, who as a rule have a regular increase of thyroid activity at the time of menstruation and pregnancy.

(3) Infective group—following pneumonia, influenza, fevers, pyorrhœa alveolaris, &c.

Therefore when treating any case of exophthalmic goitre by radiation of the thyroid gland all the physician's armamentarium must be brought into play to eliminate sources of chronic infection, to diminish or mitigate the direct effect of shock and to provide as far as possible natural mental and physical rest. To do so makes the work of the radiologist easier and improves the prognosis of the case.

The Technique of X-ray and Radium Treatment.

(*I*) *X-ray.*

It is not necessary to describe in full the apparatus used and the technique employed by radiologists. All kinds of apparatus and different types of tubes appear to give equally good results in skilled hands.

The technique varies considerably, so we briefly describe here the methods adopted by us of irradiating the thyroid gland.

We endeavour to dose the whole of the gland and any accessory substernal thyroid at each treatment. In this way the thymus cervical sympathetic ganglia are included, although we do not attach much importance to this, having come to the conclusion that successful treatment depends largely on the thorough radiation of the thyroid gland. It is wise to consider the comfort of the patient, in combination with the accessibility of the area to be treated. This we think is best obtained in the supine position with the head and shoulders slightly raised. The head is turned to one side so that the whole of one lobe of the thyroid and a portion of the isthmus is exposed. A measured and filtered X-ray dose is given. The other lobe and portion of the isthmus is then treated in a similar way. The extension tube used is oval in shape and completely covers the lobe, reaching from just below the clavicle to a little above the upper pole of the gland. It is directed upwards and inwards. The distance of the anticathode from the skin surface is 10 in. By this method both lobes of the thyroid and the isthmus receive equal doses, the isthmus receiving half of the dose given to each lobe.

The question of dosage and filtration must be decided by the radiologist according to the needs of the particular case. The dose given to each side of the thyroid, measured by Hampson's radiometer after filtration, is 3 H. With an alternative spark gap of 6 to 7 in. and the milliammeter registering 3 ma., the time taken is from nine to ten minutes. This dose is consistent with the preservation of the skin over a long period of treatment. The filters used are aluminium and felt, 1 mm. to 3 mm. of aluminium and three layers of felt.

The majority of the cases of which the statistics are given, received two treatments weekly for three to four weeks, then the interval is lengthened and one treatment given weekly. As the case progresses the intervals are extended, first to fourteen days, then to twenty-one days, twenty-eight days, two months, three months and finally six months. When patients partially relapse, an event which happens in a good number of cases even whilst under treatment, the intervals are shortened until satisfactory progress is obtained.

From the above description it follows that in this method the interval between treatments is an important factor. The dose given is approximately the same on each occasion.

The cases should be kept under observation for some time after the

disappearance of the symptoms. In this way relapses, if they occur, can be quickly dealt with, and apart from relapse, occasional doses, even one in six months, tend to preserve the perfect regulation of gland function, which after all is the aim of ray treatment. Most of the cases from which statistics are given have been under observation from two to five years.

(*II*) *Radium.*

No novel nor original method of application is employed in the radium treatment of exophthalmic goitre. The great majority of these cases are irradiated by applying plates of radium or radium emanation to the skin over the enlarged gland. For all-round use plates of a strength of 2·5 mgr. of radium element or 2·5 millecuries of radium emanation to the square centimetre are the best. The size and number used varies with the degree of severity of the case and the size of the thyroid gland. Thirty to fifty mgr. are sufficient for a mild or ordinary case, but this is frequently increased when necessary to twice the amount—viz., 100 mgr. or mc.

The plate is screened by 1·5 to 2 mm. of lead and the secondary radiation is effectually stopped from injuring the skin by twenty-four to thirty-six layers of black photographic paper. The primary rays, however, have some direct action on the skin. There is no particular advantage in the use of lead except that it has a marked stopping action on the rays (perhaps a disadvantage after all). It is also cheap and malleable. But certainly silver, aluminium, gold or platinum of the requisite thickness could be just as well employed.

Each piece of apparatus is enclosed in good rubber sheeting and a piece of lint or wool may be put over the paper before the apparatus is made up in order to make it softer and more comfortable for the patient. Exposures of twenty-four hours are given every six weeks, the apparatus being fixed to the skin by adhesive strapping and bandage. Heavy treatments at long intervals (such as six weeks) are better than smaller ones more frequently given.

A careful watch must be kept on the condition of the patient's skin. If it appears at all red treatment should be postponed. Again after six to twelve months of regular treatment the interval may be lengthened if the patient is making satisfactory progress. After twelve months in any case, even if the skin appears healthy, it is best to miss a treatment now and again.

Looking at radium treatment of exophthalmic goitre from the

general point of view the bigger the treatment that can be given at a time the better. One case will illustrate this point. A young woman, aged 23, was admitted to the Manchester Royal Infirmary suffering from well-marked exophthalmic goitre. She had a pulse of 104, prominent exophthalmos, severe palpitation and a large bilateral swelling of the lobes of the thyroid gland. Two plates of radium emanation of the usual strength were fixed over the lateral lobes for twenty-four hours. By some accident the right plate only was screened by 1·5 mm. of lead. The left had no screen placed over it and was only protected by its brass base which was roughly equivalent to 0·5 mm. of lead. On the right side the reddening of the skin was slight, on the left a large ulcer formed which took two months to heal. At the end of that period the right lobe was slightly reduced but the left was flat and no longer palpable. All the symptoms had distinctly diminished and the patient was able to return to household duties—i.e., after one treatment. The skin is thus seen to be the obstacle in the way of adequate dosage unless the slow and probably less satisfactory method already described is adopted.

This raises the question of the treatment of exophthalmic goitre by burying tubes of radium in the gland. It has the obvious disadvantage of being a surgical procedure, terrifying to a patient already in a state of chronic fear, but the scar at the point of introduction is practically invisible and the skin is saved. This procedure has been adopted twice by one of us at Manchester, but two cases are not enough upon which to base opinions, or sufficient in number to establish a guide to dosage. One patient left the infirmary three weeks later rather improved, but did not again attend for examination. The other was decidedly better for the treatment but not perfectly well and treatment was continued with plates. It is probable that the dosage was not sufficiently large, because too big a dose could not be given to the very first patient. In any case it is satisfactory and helpful to know that neither of them died.

Telangiectases are liable to appear anywhere, and the occurrence of areas of pigmentation is even more difficult to foretell or to prevent, but speaking generally they follow what is an overdose in relation to the skin. If care is taken not to treat an area in which some signs of reaction persist, or in any case to drop a treatment now and again after six to twelve months, they will be reduced to a minimum. So long as ulceration is avoided it is a nice point how far the skin should be considered in giving adequate treatment to a thyroid gland. Con-

siderable judgment and experience are necessary to balance these factors satisfactorily.

The great advantage of radium is its quietness and portability. It does not frighten, and can be carried to the bed of the sick patient. Patients can also have emanation plates applied to their necks, go home with them on, and return for their removal the next day. In addition to this, the long interval of six weeks between treatments makes it more convenient for patients who live a long distance from a main medical centre. Its scarcity is its great limitation.

General Features.

The problems of the ray treatment of exophthalmic goitre are closely related to the known facts and theories in connexion with the genesis and severity of the disease.

We take the modern view that exophthalmic goitre is an example of the vicious circle in disease. Thus the sympathetic nervous system by some means is roused to excessive action; this leads to stimulation of the thyroid gland, perhaps also of the suprarenal, by the secretion of which in turn the sympathetic nerves are again stimulated and their action accentuated, and so on. Ray treatment is given only to the thyroid gland, one part of the cycle, and a satisfactory inhibitory action on it removes one link from the circle without disturbing the nervous factor directly for good or for ill. But in the meantime the nervous system may and often does right itself with or without the aid of the physician. That recovery of the nervous condition, the primary process of the disease, may in a similar way result in the cure of the disease without radiation is clear, but the medical methods of dealing with part of the disease seem often inefficient, and treatment of the thyroid gland by X-rays or radium is always a help. Facts mentioned later in this paper bear out and more than emphasize this last point.

If an active goitre is treated by a widespread irradiation which includes the anterior aspect of the neck and upper chest often the results seem better than those obtained by more local applications. This was first done with the idea of radiating the thymus gland and the superior cervical ganglion at the same time as the thyroid gland. However, the evidence that the thymus gland is really important in exophthalmic goitre is very slight, and the action of rays on the cervical ganglion, already probably a degenerating structure, is likely to be of

little value. It seems more probable that widespread raying results in a bigger dose of rays getting to the thyroid gland through the larger skin area, thus bearing out the point already made that the bigger the dose the better.

The immediate effect of irradiation seems to be an inhibition of the secretory function of the gland and the pulse-rate will rise again after an interval if no further treatment be given. Prolonged treatment sets up a more permanent fibrotic process, so well seen by surgeons who attempt excision of part of the gland after prolonged X-ray treatment. This is possibly an exaggerated manifestation of the normal process of auto-recovery.

Severe cases of exophthalmic goitre usually follow a severe nervous cause. The thyroid gland is not necessarily greatly enlarged. Moreover it is possible that the quality as well as the quantity of thyroid secretion is changed. Removal of the thyroid factor may still in bad cases leave behind ameliorated, but still severe, nervous symptoms such as palpitation, exophthalmos, and indigestion or diarrhœa, though not usually the last. These symptoms, however, commonly abate a great deal as time goes on. It is in connexion with these cases that the interval of complete stoppage of both X-ray and radium treatment already mentioned is so useful. Here a warning may with advantage be uttered against too rigorous a rest and dietetic treatment being insisted upon in chronic cases, for it may engender in the patients' minds a feeling of depression and hopeless chronic invalidism hindering rather than aiding recovery. They should return as nearly as possible during that interval to normal life.

The most notable features to be observed in judging the progress of a case are as follows :—

(1) The first sign of improvement is a feeling of well-being. This can usually be recorded by the end of the third week. The patient gains confidence and is anxious to continue treatment.

(2) *The Pulse and Palpitation.*—The pulse-rate as a rule, after a quick drop followed by a slight rise, gradually gets slower. It may fall as low as sixty beats per minute sometimes, but we have not seen a case of artificially produced myxœdema after even the most prolonged and severe treatment. If it is possible, and such cases are reported, it is obvious that the radiologist has an excellent method at command for stopping hyperthyroidism. Not infrequently some slight quickness in the rate of the pulse may remain, together with a little palpitation. *Heart :* The screen examination of a large number

of hearts in exophthalmic goitre, and the estimation of the condition of the left ventricle by the triangulation method, point to permanent damage occurring in a number of cases. In mild cases the heart muscle maintains its tone, and there is little or no trouble with the cardiac condition after the hyperthyroidism is cured. In medium cases of long standing and in severe cases the heart muscle loses tone. The screen examination presents the typical appearance of the "lying down heart," the cardio-radiographic index increasing from the normal 7 to 14 mm. to 20 to 25 mm.

(3) *Weight.*—Thyroid secretion in association with the sympathetic nervous system increases katabolism greatly. Hence loss of weight is a prominent feature of exophthalmic goitre—failure to assimilate food is another factor. Examination of the intestinal tract by means of "the bismuth meal" reveals no constant nor definite lesion beyond a marked "intestinal hurry" in certain cases. Increase in weight is the best indication of good progress towards recovery and with ray treatment it is often a remarkable phenomenon. In fact some patients justly complain of getting too fat, a sure sign that the katabolic processes are diminished and that food is better assimilated.

(4) *The Symptoms.*—No particular symptom or sign goes first but all gradually diminish except that exophthalmos is the last to go, or may be the only sign to persist in a virtually cured patient. When the thyroid enlargement is not of long standing it steadily grows smaller under treatment and after.

(5) Intercurrent infections and attacks of diarrhœa delay the favourable progress of a case.

(6) In all cases in which there is a definite focus of infection, whilst some improvement may be obtained by X-ray and radium treatment, no satisfactory progress will take place until the source of infection is removed.

(7) *Blood Changes.*—The red blood corpuscles and the hæmoglobin index are normal except in those cases associated with secondary anæmia. There is little alteration in the total number of leucocytes, the tendency being towards a slight increase in our cases. Other observers state that a mild leucopænia may exist. The differential blood count nearly always shows a diminution of the polymorphonuclear leucocytes and an increase of the lymphocytes. We do not find that it bears any definite relationship to the severity of the disease or to the progress of the case under treatment.

EXAMPLE OF TYPICAL BLOOD COUNT.

Female, single, aged 22: exophthalmic goitre, medium severity.

Systolic blood pressure	118
Erythrocytes	5,216,000
Hæmoglobin value	90
Colour index	0·8
Leucocytes	8,800
Eosinophils	0
Polymorphonuclear	52·5
Lymphocytes	38·5
Large mononuclear	4
Transitional	4·5
Mast cells	0·5

PROGNOSIS.

The matter of prognosis is unfortunately not in a very advanced or well-developed state. The definite facts that are ascertained from our figures are that mild cases do the best of any. Severe ones such as those brought up in a stretcher to the radiologist are often more spectacular and memorable in their progress, but the end results are certainly not as good as with mild cases. They come mostly into the "good functional" group in our statistical tables.

The most difficult to treat are those in which the exophthalmic type has developed in a case of old simple goitre. The treatment has to be prolonged and progress is often exceedingly slow but gradual improvement is the rule.

Acute toxic cases are hopeless and are best left alone. In the radium statistics there is one death (the only one) under the heading "not improved." It was a case of this type.

The prognosis in exophthalmic goitre will probably be best established in the future by a careful investigation of the facts that can be gathered relative to the state of each patient's general or special metabolism.

RESULTS (WITH STATISTICS).

In these figures "perfect" means that no symptoms or signs remain. "Good functional" that the patient can follow an ordinary, not too arduous life. "Improved" indicates that the patient is undoubtedly better than when the treatment was begun—i.e., there is less palpitation, the pulse-rate is diminished, and there is an increase of weight. The number of cases in which the patients have "abandoned treatment" is very small and includes military patients and those who have moved from Manchester.

It is a noteworthy fact that patients suffering from exophthalmic goitre flock to X-ray and radium departments and persevere with their treatment until discharged.

These tables are for a series of a hundred consecutive cases treated by X-rays, and in the case of radium comprise the first hundred treated in Manchester :—

	X-rays	Radium
Perfect	27 ...	20
Good functional	49 ...	20
Improved	20 ...	40
Abandoned treatment, not improved	4 ...	20
	100	100

We realize that the personal factor is a varying one in the estimation of results. The X-ray cases are private patients, the radium cases mostly hospital patients.

General Conclusions.

Before the use of X-rays and radium became established physicians always gave a very guarded prognosis when these patients came under their care. They claim a certain number of cures by purely medicinal treatment. This no doubt may be so in some cases, but there are few statistical records from the conclusions of which outsiders can form a just estimate. But the general care of a skilled physician is invaluable to the patient and helpful and stimulating to the radiologist.

The surgical method of treatment may produce brilliant results, but there are drawbacks to it. First of all comes the 2 to 4 per cent. operation mortality experienced by the most skilful specialists. Secondly, surgeons of experience do not claim that one operation even of partial thyroidectomy is certain to be efficient. It is always possible that it may extend to three. Post-operative cases frequently come under the care of the radiologist.

In the matter of extended observations on the progress of cases of exophthalmic goitre the surgeon is at a disadvantage not so much experienced by radiologists. When his cases have been operated upon the patients again become dispersed over the country, but the radiologist sees his regularly over a period of years.

In conclusion we do not think that there is evidence that any treatment of exophthalmic goitre gives such safe, uniform and promising results as irradiation of the thyroid gland.

DISCUSSION.

Dr. FRED BAILEY (Brighton): During the past ten years, at the Royal Sussex County Hospital, patients have been regularly sent to the X-ray department there, for X-ray treatment of exophthalmic goitre. The routine treatment I have given has been that of using a tube having an equivalent spark gap of 8 in., equal to about 9-10 Wehnelt, at 10 in. from the right side of the neck (anti-cathode distance). A filter of 4 mm. aluminium and two layers of tungstate lint, the latter lying on the skin, are used. The time dose is 3 ma. for ten minutes twice a week. Originally, we tried to work with the Sabouraud measurement, but reverted to the time method. The question that appears to me to be yet in doubt is whether the Sabouraud measurement gives an accurate measurement of the amount of hard X-rays, as it does of the soft rays. It has not seemed to me to do so, therefore Sabouraud measurement between the anti-cathode and the filter would not be an accurate method; also, is it an accurate measure of the hard rays on the skin side of the filters? I doubt it. Is not a milliampere-minute measurement as accurate as the Sabouraud? I should like to hear the opinion of the writers of the papers on this point. Our principle has been to use such hard rays as would not produce dermatitis—as they would be too penetrating—and to use as much of them as the time of the department would allow. We do not, of course, have any skin effect except very occasionally a gradual slight browning, and we stop when a definite diminution of the circumference of the neck is becoming apparent. Most cases are benefited, but those patients improve more quickly who are taking a dose of 5 minims of tinct. iodi and 1 dr. of sodii phosphate three times a day than those who are having other mixtures such as digitalis, &c. But, irrespective of the other agents in the treatment of the disease, such as medicine, rest, freedom from worry, &c., I feel sure that X-ray application has definitely reduced the time needed to improve or cure our cases. The number of treatments of exophthalmic goitre cases at the hospital per week now averages about twenty-five, this being an equivalent to about half the number of patients. Some doubt was raised as to whether any appreciable amount of X-rays got through when the thick filter was used; we found, however, that, in using the same procedure for our breast-cancer patients, if a fluorescent screen was held at the back of the patient, it lit up strongly, although hardly any definition of the ribs could be made out on account of the hardness of the rays. I am not sure about this, but I think it took about sixty minutes to darken a Sabouraud pastille fully when exposed under the 4 mm. of aluminium and two layers of tungstate lint. That is, it was turned after being exposed under six patients.

Dr. HERNAMAN-JOHNSON: There is some risk of the work of the older electro-therapeutists being forgotten. Long before the advent of X-rays very fine results in the treatment of exophthalmic goitre were often obtained by such simple means as galvanism and faradism. As to the possibility of the production of telangiectases by X-rays, and the impossibility of

certainly preventing them, however rare they may be, it is little consolation to the individual victim—the 10,000th case—to know that 9,999 have escaped. Therefore, if we can produce equal benefit with less radiation in the treatment of exophthalmic goitre we ought to use less. My experience is, that by the use of various electrical modalities as an adjunct, it is possible to reduce the X-ray dosage by quite 50 per cent., as compared with what would otherwise be necessary. Moreover, certain distressing symptoms of the disease can often be dealt with successfully by electrical procedures. The *size* of the goitre is often more affected by galvanism than by X-rays. *Exophthalmos* can be influenced to some extent by the use of interrupted currents to the back of the neck, and these produce a feeling of well-being in the patient. When headache, sleeplessness and nervous symptoms in general are pronounced, cerebral galvanism has a distinctly soothing effect, the positive pole being applied to the forehead. As to measurement of X-ray dosage, I agree with Dr. Bailey that the distance-filtration (aluminium)-milliamperage-spark-gap system is the best available at present. For repeating doses with a given apparatus under fixed conditions it is very accurate; and its general applicability would be enhanced if radiologists could be got to adopt standard ball electrodes for spark-gap measurement.

On the Examination of Patients by Means of Ultra-violet Radiation.

By J. C. Mottram and S. Russ.

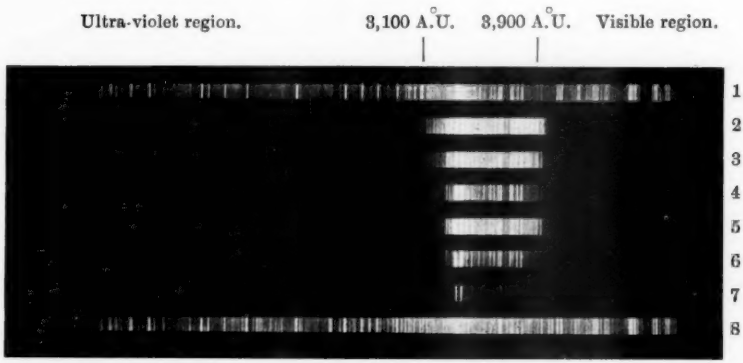
In the *Proceedings of the Physical Society of London*, vol. xxxi, part 4, there appeared a short paper entitled, "A Demonstration of the uses of Invisible Light in Warfare," by Professor R. W. Wood. He obtained invisible radiation by screening a mercury arc with a piece of glass of his own composition. This glass is practically opaque to light, but allows a considerable amount of ultra-violet radiation to pass through it. A similar glass is made by Messrs. Chance Bros. Our observations have been much facilitated by the courtesy of Professor Wood and of Messrs. Chance Bros. in giving us pieces of the black glass, for the composition of which they are individually responsible.

The degree of transparency of the glass to ultra-violet radiation depends considerably upon the thickness used. The accompanying photograph (*see* figure), taken with a quartz spectrograph using a copper arc, shows that the main wave-lengths of the radiations to which the glass is transparent lie between 3,100 to 3,900 A°.U. It is also

slightly transparent to red light in the region of 7,000 A°.U., as may be shown by using a tungsten arc as the source.

Viewed by means of this ultra-violet radiation some objects remain black and invisible, whilst others are seen to glow more or less brightly, this being due to the fluorescent light excited in the object by the incident ultra-violet radiation.

A great many substances, not hitherto observed to be fluorescent, were found to be so when examined in this manner, and amongst them the surface of the human body. It would seem, therefore, that this method of examination is likely to serve a useful purpose in certain branches of medicine, more especially in the case of the superficial lesions, such as diseases of the skin.



1, Arc spectrum of copper, exposure 5 seconds; 2, 3, and 4, radiation transmitted by Chance glass, thickness 3·5 mm., exposures 60, 15, and 3 seconds; 5, 6, and 7, radiation transmitted by Professor Wood's glass, thickness 5 mm., exposures 60, 15, and 3 seconds; 8, arc spectrum of copper, exposure 3 seconds.

The following is a brief description of the normal appearances. The skin presents a generalized violet fluorescence, but over the face, and to a less extent over the rest of the body, dark non-fluorescent spots are seen, numerous in some individuals, almost absent in others. Freckled persons are usually thickly spotted, but on the other hand, perfectly clear complexions may present a similar appearance. There are also to be seen, especially on the face, minute brightly fluorescent spots, having the appearance of stars. They are most numerous on the brow, eyebrows, and bridge of the nose. The teeth are brilliantly fluorescent, but false teeth come out black. The nails are also very fluorescent. The

eyes have the appearance of a boiled eye of a fish, due to fluorescence of the lens. What Professor Wood calls a "lavender haze" fills the whole field of vision when one looks at the source of radiation; this is likewise due to the fluorescence of the lens.

Attempts have been made to photograph these appearances, but certain difficulties were encountered. Any object upon which the ultra-violet rays fall will reflect some of them, and if these reflected rays are allowed to enter the camera they will give an image of the object; but it will not correspond with the visual appearance, as this is entirely due to fluorescent light. To reproduce the visual appearances, only the fluorescent light must be permitted to enter the camera. This can be accomplished by interposing a piece of Crookes' glass, which almost completely cuts out the ultra-violet radiation. Prolonged exposures (two minutes) are however necessary, and thus the possibility of movement must be carefully guarded against. Some interesting abnormal appearances have been already observed. It remains to be seen to what uses this method of examination can be put; our only object now is to bring it to the notice of medical workers.

Section of Electro-Therapeutics.[1]

President—Dr. A. E. Barclay.

The President.

As you are aware, we are to hear the first Mackenzie Davidson Memorial Lecture. Our debt to Mackenzie Davidson is great; he was one of the pioneers, who saw the possibilities of X-rays. Indeed, it is partly due to his foresight that we are here to-day and that the subject has been so successfully developed in England. His personal contributions were very considerable in the early days, especially in the matter of localization.

On looking round to see who would be most suitable to deliver the first Mackenzie Davidson Lecture, we decided that the best was a recently elected Honorary Fellow of this Society, Sir Ernest Rutherford, and we are grateful that in spite of his heavy engagements he consented to be with us to-night, and we are conscious of the great honour he does to our Section.

My first introduction to our lecturer was over twenty years ago, at a time when I was much keener on sport than on science. I was to enter for an examination in Engineering, and I went to a coach, somewhere in Cambridge. He was a cheery sort of individual, and he took an interest in a difficult task, and, thanks to his efforts, I managed to pass. My coach was Sir Ernest Rutherford. He has gone far since then, and it has been his privilege to teach the world many new facts concerning radium, and his work will go down through the ages as, perhaps, that of few men will. It is with the greatest pleasure that, in your name, I heartily welcome Sir Ernest Rutherford, to give us the Lecture.

[1] At a meeting of the Section, held April 16, 1920.

The Development of Radiology.

MACKENZIE DAVIDSON MEMORIAL LECTURE.

By Sir Ernest Rutherford, F.R.S.

It was with some hesitation that I acceded to your President's request to address your Section, for I felt there was little of value I could tell a gathering of experts in that subject; but it may be of interest to record the impressions of a layman who has taken an almost fraternal interest in the development of Radiology from its infancy to its vigorous manhood. I can well remember the extraordinary interest of all classes of the community excited by the discovery of X-rays in 1895—an interest much more general and intelligent than the recent outburst in Relativity. These stirring times are brought to mind by the faded X-ray photograph so often seen on the walls of a physical laboratory, and viewed with proprietary pride by the professor as the first of its kind in his city or country. It is a striking contrast to recall the simple Crookes tube and primitive accessories of the early days and then to view an up-to-date installation in a large hospital, replete with every mechanical and electrical aid unthought of in the old days, for taking in a few seconds or even in a fraction of a second, a beautiful X-ray photograph showing every detail of the structure under examination.

If the advance seems great as regards the material appliances and means of control, it is still greater when viewed with the eye of the mind, for the modern X-ray tube and particularly the latest development, the Coolidge tube, is an embodiment of many of the principles brought to light in some of the most notable investigations of our age—the proof of the existence of the swift electrons, their copious release from glowing bodies and the great advances made in the production of high vacua, not to mention the use in quantity of resistant metals unavailable at the beginning of the X-ray epoch.

When we consider the remarkable potentialities of the X-ray tube, whether as an aid to medicine and surgery or as one of the most powerful means of unravelling the mysteries of the constitution of atoms and molecules, it is not a matter of surprise that to a man of scientific imagination an X-ray tube in action excites almost a feeling

of mystery and constitutes a never-failing stimulation of scientific interest. Possibly for this reason the devotees of radiology exhibit to an unusual degree an infectious enthusiasm and faith in their subject, and an unflagging optimism for its future. Of this type of man, endowed with scientific enthusiasm and imagination, was the late Sir James Mackenzie Davidson in whose honour the lecture of this evening is given. While I had not the good fortune to know him so intimately as did many of you, I would like to say a few words of appreciation of his yeoman service to the cause of radiology, by his inventions and discoveries, and still more by his unflagging zeal in promoting the welfare of his subject, his unwavering belief in its future, and his ever-ready and unselfish assistance to all workers. He was long rightly regarded as the pioneer of radiology in this country. He was the first to point out the advantage of stereoscopic methods in locating the exact position of a foreign object, and devised a number of practical methods of attaining this end. His inventive and acute mind first saw the advantages of the rotary mercury break for giving more intense X-rays, and this apparatus was the forerunner of the great variety of types in use to-day. But his influence was not confined to his positive contributions to science but was even more due to his readiness to help his colleagues by showing them his apparatus and giving freely of his ideas.

I recall a very interesting evening spent at his house, where he showed me the numerous clever devices he had originated, and where I saw a number of striking radio-active experiments for the first time.

It is to the enthusiasm and devotion of men like Mackenzie Davidson and his colleagues that we owe the great advances that have been made in radiology and the steadily increasing recognition of its importance in medicine. We have heard from other speakers of the great part played by X-rays during the war, and on reading contemporary history we cannot fail to be impressed by the evidence of the zeal and almost devotion that were shown both by men and women in charge of mobile or fixed X-ray units.

While the radiologists may justly view with pride the great advances made in their subject in the last two decades, I think all are conscious that much more remains to be done if we are to reap the full possibilities of the application of X-rays, radium rays and other electrical agencies in medical therapy. We have arrived in a sense at a parting of the ways, for the old haphazard organization will hardly suffice if we are to keep a steady rate of

advance in technique and knowledge. Under the present conditions, the radiologist is usually worked at full pressure and fully occupied with routine. It is almost impossible for him to spare time or energy to investigate the numerous important problems that arise in his work. Even if time were available, few institutions have the necessary laboratories and facilities for carrying out such researches. It is true that many hospitals have a fine X-ray equipment, and a few even possess comparatively large quantities of radium. The management of many of these institutions are usually satisfied that everything necessary has been done when provision has been made for a good X-ray department and a supply of radium in charge of capable radiologists. A few are more advanced and recognize that if progress is to be made, it is of the utmost importance to give every encouragement to the prosecution of research in the problems of radiology. This entails not only the provision of a well-equipped laboratory, but—of even more importance—the provision of research workers. If the radiologist is to be in a position to carry out such researches, it is essential that all his time should not be taken up by routine duties, and also that the scale of payment be such that he can feel himself free to devote his spare time to research. Experience has shown that it is only in this way we can hope to obtain the services of a body of keen and progressive men, competent to carry out their work in the most efficient manner, and capable and desirous of still further advancing professional knowledge. It is only by such a policy that we may hope to obtain the full return, in service to the public, for the benefactions so generously made to many institutions. The movement made by the authorities of the Middlesex Hospital to provide adequate teaching and research in radiology is of good omen, and it is to be hoped that that Institution will have many imitators.

It is the recognition of this imperative need that has led to the very influentially signed appeal for subscriptions to a Mackenzie Davidson Memorial Fund. It is felt that the most fitting memorial to this pioneer of radiology would be the foundation of an institute to which a professorial chair in one of our Universities would be attached, and which would serve as a centre of teaching and research in radiology in general. Its activities should cover the whole field of the utilization of X-radiation and similar agencies, not only in medicine but also in industry. It would be difficult to exaggerate the importance of a flourishing institute of this kind. It would fill a want not adequately covered by any single existing institution and would serve as a powerful agency for advancing our knowledge in this important

department of science. It would also indirectly help to unify the efforts of all those interested in the advance of this subject, and so provide a stimulus to a large band of devoted workers who would feel that the importance of their work and subject was receiving some sort of public recognition. I am sure that both radiologists and physicists are in complete accord in their hope that this scheme will receive sufficient financial support to be put into active operation in the near future.

There is another important development in radiology with which I have been personally connected in its later stages, and on which I would like to say a few words. There has in the past been little or no systematic teaching in radiology, but the present workers have had to acquire their knowledge by laborious study of the widely scattered literature and by the sacrifice of much time and energy outside their professional hours. It must be remembered that the expert in radiology must not only study the medical aspect of the question, but must have an expert knowledge of the working of his apparatus and a general acquaintance with the contributions of physics to the knowledge of the radiations which he employs. It speaks volumes for the enthusiasm of many of the workers in radiology that they have surmounted many of these difficulties and have acquired a working if not a profound knowledge of both the physical and medical side of their subject.

The radiologist who has acquired his knowledge in this slow and painful way is the first to recognize the great importance of providing adequate instruction in both the physical and the therapeutic side to those who will be the radiologists of the future. It must always be borne in mind that radiology as practised to-day is a highly expert and technical profession, and that no one is competent to undertake this important work without a considerable training and education in his subject. We all hope that the day is past when anyone can pretend to give X-ray, radium or electrical treatment with little knowledge of the effects of the radiation, and still less of the imposing apparatus he employs to impress the simple-minded patient.

I have already spoken of the infectious enthusiasm of the radiologists for their profession, and this, to my mind, is evidenced in a striking way by the time and energy which many prominent radiologists have freely given to organize a definite course of instruction for those who wish to become radiologists. After a full discussion of the problem, the University of Cambridge has agreed to issue a diploma in radiology to properly qualified students who have attended the requisite courses

and passed a satisfactory examination, or have presented a satisfactory thesis and given evidence of a practical training. The arrangement of the syllabus and conduct of the teaching, &c., is left in the hands of a committee composed of well-known radiologists and representatives of the University. The representatives of radiology have very wisely insisted from the beginning on the importance of adequate instruction not only in the medical application of radiology but also in the physical principles involved in the apparatus and radiations employed. In this way we may hope that the radiologist of the future will have sufficient knowledge of physics to understand fully the working of his apparatus and the character of the agents he employs in his work.

The need for such a course of instruction is evidenced by the number, about thirty, who are taking the first course now in progress in London. The instruction in the physical side, including both lectures and practical work, is being given in University College under the direction of Sir William Bragg, whose help is highly valued. On the medical side, a number of prominent radiologists have agreed to give lectures in the various divisions in which they have an expert knowledge, and I am sure many veteran radiologists, on reading the syllabus covered by the lectures, will wish that they had the time and opportunity to take the course themselves. If numbers permit, it is hoped later in the year to arrange a similar course in the University of Cambridge. This may prove especially attractive to those who are able to spend full time on the course, and to candidates for the diploma from overseas. From the point of view of the convenience of the profession, it is an obvious advantage that more than one centre of instruction should be available, but until the diploma is well-established, it seems to me important to have a well-organized system of instruction, with a number of expert lecturers, in two or three convenient centres, and to leave the question of still further multiplication of teaching for future decision.

The provision of these systematic courses of instruction, seems to me a step of great importance to the future of radiology in this country, and it is hoped that it will not be many years before all intending radiologists will feel it essential to obtain the diploma before engaging in professional work.

There is one subject of more technical scientific interest to which I should like briefly to refer before concluding my lecture—viz., the problem of the ultimate origin of the therapeutic effects produced by

X-rays and the rays from radium. I think there is a general consensus of opinion that the effects produced by X-rays are very similar in character to those given by radium rays, and especially by the β and γ-rays, which are most generally employed, although it may be more advantageous in many cases to employ radium on account of the ease of localization of the effect that is sought for. Such a similarity is to be expected from the physical standpoint, for the effects observed are probably not directly due to the X-rays and γ-rays themselves but to the swift electrons that are liberated by the passage of these radiations through matter. I do not know how far I am in agreement with radiologists, but it has always appeared to me that in the ultimate resort the therapeutic effects observed must be ascribed mainly to the chemical changes produced by the radiations, and that the direct physical effects are secondary in character. In making this statement, the term chemical change is to be interpreted on broad lines, and it is far from my intention to undervalue the importance of the changes of a physical character that may be the accompaniment or the result of the chemical changes.

We all know what marked chemical effects are produced by the action of these radiations, and particularly by the rays of most energy—viz., the α-rays. In a radium solution, water is rapidly decomposed into hydrogen and oxygen, while some ozone is also produced. These chemical actions are in general of two kinds: (1) dissociation (2) recombination. For example, not only is water decomposed into hydrogen and oxygen by the α-rays, but hydrogen and oxygen in the free state under the action of the radiations recombine to form water again. In a radium solution that has been kept sealed for some months, an ultimate equilibrium is set up between these two opposing processes. Some of the simpler chemical actions have been closely examined, and in many cases it appears certain that the amount of chemical action stands in close relation with the amount of ionization of the gases concerned—a quantity that can be directly measured by physical methods. In the case of the recombination of hydrogen and oxygen under α-rays, there appear to be more molecules formed than pairs of ions in the gases, and the detailed work of Lind indicates that the ratio is 4 to 1, nearly. A similar result appears to hold in several other cases examined, so that the amount of chemical action seems to be related closely with the ionization, if not nearly proportional to it. In one case only, the recombination of hydrogen with chlorine, the effect observed is several thousand times as great, but this appears to be an exceptional case.

If this interesting connexion between ionization and chemical activity is true generally, our knowledge of the radiation allows us to follow clearly the general nature of the distribution of this chemical action with different types of radiation. You are all probably aware of the beautiful method devised by Mr. C. T. R. Wilson for showing the trails of α, β and γ-rays and X-rays in passing through a gas, where each gaseous ion by a sudden expansion of the moist air becomes the centre of a visible globule of water. In this way the nature and distribution of the ionization in gas can be seen at a glance. These effects are well shown by the beautiful photographs obtained by Mr. C. T. R. Wilson, of which a few are reproduced for illustration. Fig. 1 shows the tracks of the α-rays, which originate from a minute quantity of radium on a needle point. This photograph brings out the essential



FIG. 1.

Tracks of α-rays in moist air.

properties of the α-rays—viz., their rectilinear path and the abrupt termination of the effect showing the definiteness of the range of the particle. In fig. 2 is seen the essential difference between the effects produced by an α and β particle. The density of ions produced along the track of the α particle—viz., about 30,000 per centimetre, is so great that the photograph of the water drops shows as a continuous line. The effect of a β particle shown in the same photograph is very different. Not only is the path of the particle through the gas much more tortuous, but the density of the ions is so much less that the individual water drops representing single charged ions are clearly visible.

In fig. 3 we see in a wonderfully concrete way the effects produced by the passage of a beam of X-rays through the gas. The ionization is

not uniform but is mainly due to the swift electrons or β-rays set free from a few of the atoms by the action of the radiation. Here again in a number of cases we can follow the tortuous track of a single electron through the gas until its energy becomes too small to ionize the gas.

The general distribution of ions produced by the passage of the radiations through human tissue must be very similar, but the actual length of the trails of the particles is probably only about 1/1000 of those in air and the density of ionization correspondingly greater.

The examination of these trails of ionizing particles do not, however, give us any information about the carriers of these electric charges, for the condensation of the water is due to the charge on the atom and not to its mass or its chemical composition. In order to get information on this point, we have to resort to the chemical effects which result from the radiation. We must bear in mind that all of these radiations have ample reserve of energy not only to decompose complex molecules but to disintegrate molecules into atoms and possibly, but only in exceedingly rare cases, to disintegrate the atom itself.

It has been shown by Duane, for example, that the passage of α-rays through hydrogen probably leads to the decomposition of the hydrogen molecule and the liberated hydrogen atoms, or new compounds formed by them give rise to marked chemical actions. In a similar way, we may be fairly confident that all the different ways in which dissociation is possible actually occur under the radiations although in very variable degrees.

When we pass from such chemically simple cases to the consideration of the effects on a living organism, the problem is one of very great complexity. The passage of the rays through highly complex organic molecules may produce a great variety of changes, for not only may the molecule as a whole be dissociated but the individual component parts will be acted on as well. Some of these may recombine again to form new compounds and it is not difficult to imagine the extraordinarily complex effects that may arise in consequence. If we are to form any definite idea of the relative importance of the various chemical actions, it seems to me very necessary to examine in much more detail the chemical effects produced in a number of typical cases. I know this is not an easy problem but with large quantities of radium available, the difficulties should not be insuperable.

The chief problem in the application of radium rays and X-rays in medical therapy is of course to produce a destructive effect on the disease cells or germs without too serious a disturbance of the healthy

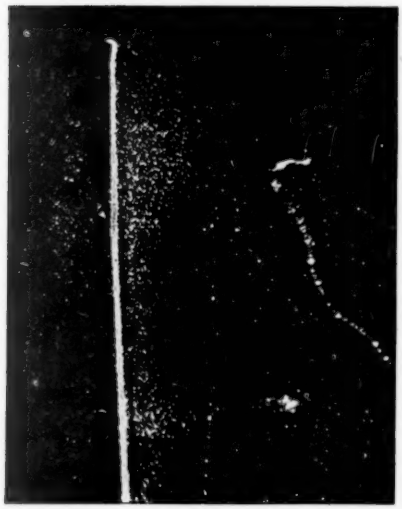


FIG. 2.

Track of α particle (left) and β particle (right).

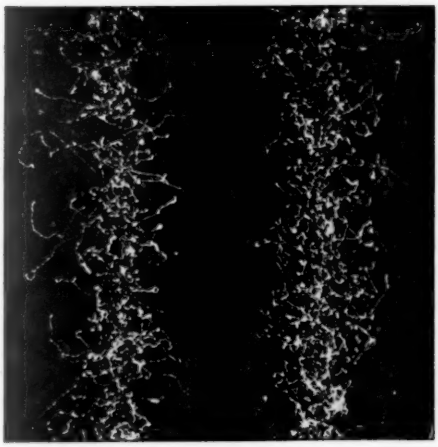


FIG. 3.

Tracks of swift electrons (β-rays) liberated by the passage of X-rays through a gas.

cells. It is to be anticipated that such a marked differential action of the radiation may only show itself in a few instances and that, in other cases, no obviously beneficial but even a harmful effect may be produced. I have sometimes speculated whether it might not be feasible in such cases to produce the desired differentiation by the introduction of special salts or solutions into the region under treatment. Such solutions may in themselves be quite innocuous, but under the influence of the radiations new chemical changes may take place which may lead to the desired results. If such a method were at all practicable, it might open up a new method of attack where the ordinary methods entirely fail. In whatever direction we turn, it is obvious how much remains to be done before we can reach a position to gauge rightly the possibilities of these new agencies in medicine. The only royal road to knowledge is steady research and experiment of highly qualified men who follow up every promising line of attack and are not daunted by the obvious complexity of the problem. In such investigations in which in many cases not only a knowledge of medicine but also a detailed knowledge of physics and chemistry is required, it is an obvious advantage if the representatives of the different sciences agree to work together. Such a combination in one case I call to mind, viz., the combined work of Mottram and Russ, has already produced happy results, and it is to be hoped that such liaisons will be more frequent in the future.

DISCUSSION.

Sir ARCHIBALD REID, C.M.G.: This is one of the occasions on which one wishes one could have a thousand tongues. My single one speaks haltingly, but this is one of the easiest tasks it has ever had—namely, to propose a cordial vote of thanks to our lecturer. But, before congratulating Sir Ernest Rutherford, I think we ought to congratulate our President on having secured such an excellent lecturer for the first. You know what an extraordinarily busy man Sir Ernest is, and it was only the persuasiveness of Dr. Barclay which secured him, and we in the Section know that what he suggests, we have to do. Certainly what we have been given to-night has been an intellectual treat. I think I may say we are all absolutely in accord with him in his keen desire for a combination of the physicist and the radiologist of a more intimate nature than heretofore. The present conditions are such as almost to make one wish one was of that band who are starting the study, for, compared with the workers of former days, they have the radiological world more or less at their feet. This is the parting of the ways, and they have a splendid opportunity to go forward, with great promise for the future. With regard to what he said concerning the baleful effects we can produce on the victims of radiation

by dissociation, I ask you to try to imagine what will happen if you go home and take a combination of carbon, hydrogen, oxygen in varying proportions, and then go into your X-ray room and turn the tube on. I am sure you will support me in proposing a very hearty vote of thanks to Sir Ernest Rutherford for his extremely illuminating and interesting lecture.

Mr. THURSTAN HOLLAND: I find it very difficult to second the vote of thanks which has been so ably proposed by my friend Sir Archibald Reid, because he has said just the things I would like to have said. Two points occur to me in connexion with it. The first is Sir Ernest Rutherford's exceedingly interesting and fascinating lecture, and the other is to remember that it is associated with the name of our old comrade Mackenzie Davidson. I am sure nothing would have given him greater pleasure in his life than to have known that when he passed away his memory would be perpetuated amongst us, by such a lecture as we have listened to this evening. Various reflections have come to me as the proceedings have been in progress, and one had reference to your association, Mr. President, with the lecturer. You gave us a dramatic account of your neglect of work whilst you were at Cambridge, and how you wasted your opportunities, and we have heard from Sir Ernest how he got you through your examination. I do not know which of you deserves the greater credit, but I think you both overlooked one important item, and that was the courtesy and consideration of the examiners. Sir Ernest said, in one part of his lecture, that some of us older workers, starting with practically no knowledge and having to pick our way laboriously in the earlier years in regard to X-ray work, had a painful career, and that the young men starting now have better opportunities than we had. But I do not agree with that. I look back upon my career—and I think in this I can speak for every early worker—and feel that I had a wonderful time. When I started I knew very little about electricity, and scarcely anything of photography, but gradually one got to be acquainted with the induction coil, with the Crookes tube, and later was able to take radiographs of a sort. But to describe that process as a painful one does not quite represent the situation. Certainly I look back upon my past years in this work with the greatest pleasure. The student now coming forward, with the advantage of attending lectures in Cambridge and London, has an advantage which it has rightly been suggested could well be appreciated by older practitioners. This student will not have quite such a good time as some of us had in the early years. Fortunately, I live in Liverpool, and, therefore, being far from both those centres of learning, the temptation to attend those lectures will not be thrust in my way. It has given me the greatest pleasure to be present to-night, and our thanks to Sir Ernest Rutherford are something more than a formal vote: our gratitude to him for having come to give us the first Mackenzie Davidson lecture, and giving us such a very good start, is very real.

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Section of Epidemiology and State Medicine.

President—Dr. E. W. Goodall, O.B.E.

Trench Fever: Its Epidemiology and Endemiology.[1]

By W. Byam (Major R.A.M.C.), and Ll. Lloyd (Captain R.A.M.C., T.).

Though the title of our paper is "Trench Fever: its Epidemiology and Endemiology," it will be well to begin by giving you a description of the disease, as its causative organism is at present unknown, and there is no single sign or test by which it may be recognized with certainty.

Trench fever has also been known as "five-day fever" and "Wolhynia fever." It is a blood infection communicable from man to man by means of the louse (*Pediculus humanus*); and is characterized by recurrent pyrexia, headache, giddiness, pain in the lumbar region, pain in the limbs, chiefly in the legs, and often of considerable severity; conjunctival congestion, sweating, moderate leucocytosis at the height of the fever, and slight enlargement of the spleen. In some cases a period of irregular fever follows, and eventually a certain percentage of the patients pass into a stage of chronic ill-health. Infection is often very persistent, and acute febrile relapses may occur after months of quiescence. The fact that trench fever is not a form of enteric was clearly proved by the American Red Cross Medical Research Committee working under Strong in France during 1918.

Though evidence is forthcoming which shows that trench fever has been known to physicians in Poland as "Febris Wolhynica" for some time past, it may safely be said that the disease was first recognized as a specific entity during the Great War, when it appeared on all the

[1] At a meeting of the Section, held October 24, 1919.

European fighting fronts—800,000 being the approximate number of cases occurring in France among the Allies during four years. If the difficulties connected with the diagnosis of a disease such as that just described be borne in mind, it will be realized that no reliable statistics of the incidence of trench fever exist or are likely to be compiled in the future.

From Europe the disease was conveyed to Egypt and Mesopotamia, and sporadic infections have been reported among those coming into contact with returned troops in England and elsewhere; and it is safe to say that the long periods during which the blood of trench fever patients remains capable of infecting lice make it probable that the disease will have a far wider distribution in the future, when those infected in Europe have returned to their homes all over the world. In this connexion it is well to point out that the distribution of the other louse-borne diseases by no means corresponds to the distribution of lice, and that a study of the range of typhus and louse-borne relapsing fever is probably our most reliable guide to the future endemic areas of trench fever, though Australasia will probably become infected as well.

Trench fever being transmitted by lice alone, as far as our experimental evidence at present shows, its occurrence depends on the presence of these insects and of human carriers of the infection. As with other insect-borne diseases, the vector requires to be in considerable numbers before an epidemic can break out, and close contact between human beings is necessary to facilitate transference of lice.

The spread of lice is due, to some extent, to their own active habits, for when their host is in warm surroundings, so that the temperature outside his clothing approximates to that inside, they are liable to migrate from him and pass on to others in the immediate neighbourhood. This takes place especially in beds where two people are sleeping together. To sleep with one who is infested with lice is a certain means of becoming verminous. Lice, as we shall see later, are also liable to leave their host when he is febrile and his skin becomes too hot for them. They also leave him at his death and cling to his bedclothes and surrounding objects, and are then very likely to adhere to anyone coming in contact with these bedclothes and surroundings. They may be dislodged by brushing and fall to the ground, and it has been stated that they have been blown off by the wind and carried to a distance. They may also leave discarded garments of their own accord. Lice spread abroad by any of these means may be termed "stray lice,"

which are in a very helpless condition for finding fresh prey as compared with the human flea or the bed-bug. The flea jumps into the air when disturbed by a sudden draught of wind, such as is caused by a foot moving near it, lights on anything it meets with in its course, and thus finds its temporary host. The hungry bed-bug hunts down its victim, probably guided by his scent, often travelling long distances to find him. The stray louse can only wait till a fresh host comes into contact with it, or wander aimlessly about on its legs which are not very well adapted for travelling on anything except rough cloth or hair. Peacock showed that it is improbable that lice are attracted to man in any way by his smell, since they take no notice of a sweat-impregnated shirt placed near them. They are guided in their movements, to some extent, by a sensitiveness to light. When well fed they creep into dark places, but hunger drives them towards the light again. This habit, however, helps them little in their search. They are very sensitive to heat, being adapted to the temperature which exists between the skin and the clothing—86° to 90° F.—and what guidance they get in finding new hosts they probably obtain from this faculty alone. Stray lice in a bed very quickly find out a man who sleeps in it. The temperature of their surroundings has a profound influence upon their movements and vitality. At 104° F. they are extraordinarily active, running round and round with the rapidity of bed-bugs. At 90° F. they are moderately active, and if unable to feed, digest what food is in them, and die about the second day from starvation. At the temperature of a warm room, about 70° F., their activity is little marked, and their vitality is so reduced that they may survive a week without food. At still lower temperatures they become moribund and die slowly, some having been known to survive ten days at the freezing point, and this is the longest period during which lice have been known to live without food. While they remain active the distances that they are able to travel are somewhat surprising, their movements, though slow, being fairly persistent. A well-fed female louse was observed to walk along a stretched thread of cotton for a distance of 4 ft. in thirty minutes, in a warm room, the impulse to walk being produced by placing the louse at the end of the thread near the window. Peacock observed that two lice travelled a distance of 5 ft. in an hour in a place where they had apparently no particular stimulus to guide them. By means of such wanderings the louse is no more likely to find a fresh host than by remaining where it happens to fall. If it is on a smooth surface, such as a board, and a cloth surface brushes over it, it immediately attaches itself to the cloth, and

we have occasionally availed ourselves of this habit when lice have accidentally fallen on the laboratory floor, by passing a turkish towel over the place where they fell and at once recovering them. Again, if the insect is on a cold cloth surface and a warmer one is pressed against it, the louse will immediately attach itself to the warm cloth. In this way lice may be picked up in public conveyances with cushioned seats. As these chances do not occur very often, it is certain that the vast majority of stray lice die without finding a new host. The more congested the community the more likely are they to be picked up. It follows also that when lice infest a discarded garment the best chance for them is to remain where they are in the hope of the article being worn again. This is what the majority of them do. A stray louse, when it finds a new host, can, of course, only multiply if it happens to be a fertilized female. A single louse, however, is enough to give rise to an attack of any one of the louse-borne diseases, should it be an infected one. We have a record of an officer who received a single louse upon his skin, scratched himself, and, in due course, developed trench fever, and in like manner we have produced trench fever experimentally on several occasions by means of a single infected louse or its excreta.

An unoccupied dwelling cannot be infested by lice in the manner in which it may be infested by bed-bugs or fleas, and the presence of lice denotes recent occupation. Peacock, who studied the dissemination of lice among our troops in France, discusses in detail the reputation which certain dug-outs get for being lousy. He comes to the conclusion that this lousiness is due to the presence of infested men and not to any inherent quality of the habitation. The dug-outs with the worst reputation were the largest, those in which most men congregated.

Bedding, however, is a most important source of spread, and it is courting disaster to sleep in that recently used by an infested person. Most convincing figures in proof of this are quoted by Nuttall. Dr. Hamer, of the London County Council, had the beds examined weekly in some common lodging-houses, which were largely used by people of the tramp class. Throughout one year, in the different months, from 12 to 31 per cent. of the beds were found to contain lice; the number being higher in winter and lower in summer. Incidentally, the result of his inspection over a number of years, was to reduce these percentages to less than 5, owing to the increased care engendered in the keepers of the houses. There is no reason why lice should not lay eggs on blankets,

since they do so on the outer garments to which blankets correspond during the night. Beds unused for several weeks might therefore harbour lice, since the hatching of the eggs may be retarded by cold and the incubation be completed later.

Lice also spread by means of garments, and this is probably the main source of spread in armies, where clothing is largely communal property. It has been repeatedly noticed that when lousy garments are discarded, the lice are liable to congregate outside them, and these lice are very likely to get access to clean clothing which comes in contact with them. They have also been observed to creep out of the necks of kitbags and may, in this way, pass on to clean kits. The ordinary processes used in a laundry do not necessarily kill lice and their eggs, since the water used for washing is often not of the lethal temperature, and soaking in cold or only warm soapy water does them no harm unless the immersion is very prolonged. Garments apparently cleansed from dirt may therefore harbour vermin, and those who put them on become infested.

Lousiness.—If garments containing lice are worn continually day and night, the vermin increase and multiply in a remarkable manner. Cases are on record in which single garments have held thousands. These are unusual cases, and indicate either extreme helplessness on the part of the infested person or, what is more likely, utter indifference to the filthy condition. In attempting to arrive at an average estimate of lousiness in troops, Peacock excluded these extreme cases, and found that where 95 per cent. of the men had lice upon them, the average number was twenty lice per man, the range being from ten to thirty. In another series of men he found about 3 per cent. with more than 350 lice each, while one shirt he examined was estimated to contain 10,428 lice and 10,253 eggs.

It is important to remember when inspecting people for lousiness, that the eggs of the louse may be laid on the hair of the body. In this position they are very difficult to see, but careful inspection and a good light will often reveal them. It is practically impossible to say whether isolated eggs found in these positions are those of the body-louse or of the head-louse, the only difference between these two forms of lice being the very elusive one of size. The matter has been the subject of controversy, as some observers, perhaps unaware that the head-louse may infest the body-hair, have recorded the nits of the body-louse in these positions in large numbers. That body-lice do lay eggs on the body hair we finally proved in experiments in which men

were artificially infested with body lice for a night. In one experiment we found a dozen freshly laid eggs on the pubic hair of one of the men who was, of course, louse-free before the experiment. To make quite certain that they had been laid during this night, the eggs were incubated, after the hair on which they had been laid was cut off, and lice emerged from them in due course.

Body-lice are found on man all over the world, and there is probably no tribe free from them. They are, however, less prevalent in tropical than in temperate and cold climates, and in temperate regions are less numerous in summer than in winter. This is in correlation with the different habits of people in the winter. Then underclothing is more likely to be of wool than of cotton, and lice prefer wool. It is also the custom with many people in this country, for some reason that is difficult to understand, to change woollen garments less frequently than cotton ones. Among certain classes in winter, the day clothing is also worn at night. People also at this time keep more indoors and crowd together over stoves. All these habits are in favour of the spread and increase of lice, and there is no creature in creation more ready to seize time by the forelock than the louse.

Soon after the first cases of trench fever were recognized in France, it was realized that they usually occurred in groups, the victims most frequently being sleeping companions or persons coming in contact with one another; the explanation of this being that lice tend to migrate from a fevered host and so spread infection in the immediate neighbourhood. We were able to prove this by carrying out the following experiments. Some of the soldiers under treatment at the New End Military Hospital, Hampstead, and the civilians who were allowing us to infect them with trench fever, offered themselves for these exceedingly unpleasant experiments, being willing to spend highly uncomfortable nights in the interest of science. The experiments were carried out in a small room with distempered walls and boarded floor. It was not artificially heated, and the work was done in February when the weather was cold and raw. A bed was made up on the floor of the room consisting of two mattresses placed side by side and covered by a white blanket, with ordinary pillows and pillow-slips, and four white blankets to cover the men. Into this bed the men, clad in flannelette pyjamas, went in pairs, and 200 body-lice were released on the abdomen of one of them in the region of the umbilicus. The lice used were, in each case, adults and well-grown nymphs, since young

larvæ might have proved difficult to retrieve. The men were instructed not to get out of bed; not to touch the insects; to avoid scratching if possible; to interfere in no way with their roaming. They were also asked to note when and where they were bitten.

Every two hours the temperatures of both men were taken, and notes were made as to the condition of their skins, whether moist or dry. Sometimes a brief observation was made as to the scattering of the lice at the time of our visits, and the men were questioned as to the biting. The experiments were allowed to proceed for varying periods, generally about sixteen hours, and the lice were then again collected and their distribution on the men and blankets noted. It was found necessary, in each case, to rip along the tape holes of the pyjama trousers and to open any seams into which the lice might have obtained access. In this way complete data of the migrations of the lice were obtained. Six of these experiments were carried out, in three of which the man upon whom the lice were released, called the primary host, was febrile; and in three he was normal. His bed fellow, the secondary host, was normal in each case. The febrile man was in each case suffering from trench fever. There was a very marked difference in the behaviour of the lice in the two series of experiments, there being an increased migration from the primary host when he was febrile. When both men were afebrile, 62 per cent. of the lice remained on or near the primary host, 35·5 per cent. being inside his pyjamas; while only 20 per cent. passed on to or near the secondary host, 9·5 per cent. of these being inside the pyjamas. When the primary host was febrile, only 44 per cent. of the lice remained on or near him, 15·5 per cent. as opposed to 35·5 per cent. being inside his pyjamas, while 38·5 per cent. instead of 20 per cent. passed on to or near the secondary host, 12·5 per cent. being inside his pyjamas. It is therefore seen that the febrile condition of the primary host nearly doubled the migration. When the primary host was febrile, the second man felt the biting of the lice very much earlier than in the other cases—in the first, second and third hour respectively, whereas in the afebrile series the secondary host was never bitten within five hours from the commencement of the experiment — so soon in fact that it is indicated that some of the lice migrated either before they fed on the febrile man, or, any rate, before they had obtained a full meal. Where the first man was normal the interval before the second was bitten was such as to have allowed the lice to be ready for a second meal after having fed to temporary repletion when they were first released.

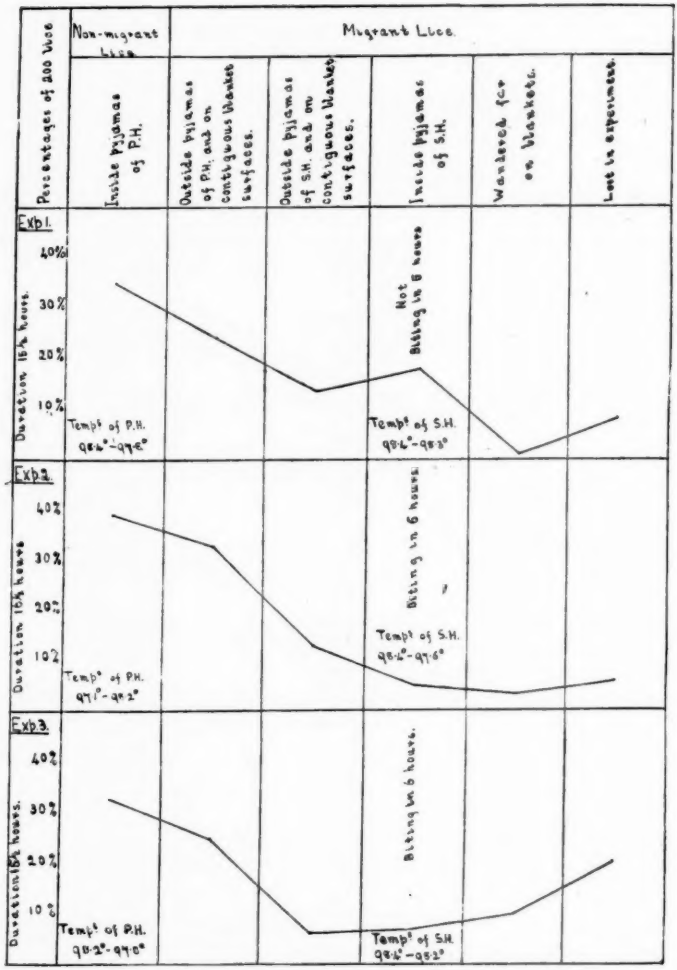


CHART I.

Illustrating the migration of lice from an afebrile man (P. H.) to an afebrile bedfellow (S. H.).[1]

[1] The charts, tables, and illustrations accompanying this paper are reproduced by permission of Messrs. Henry Frowde, Hodder and Stoughton.

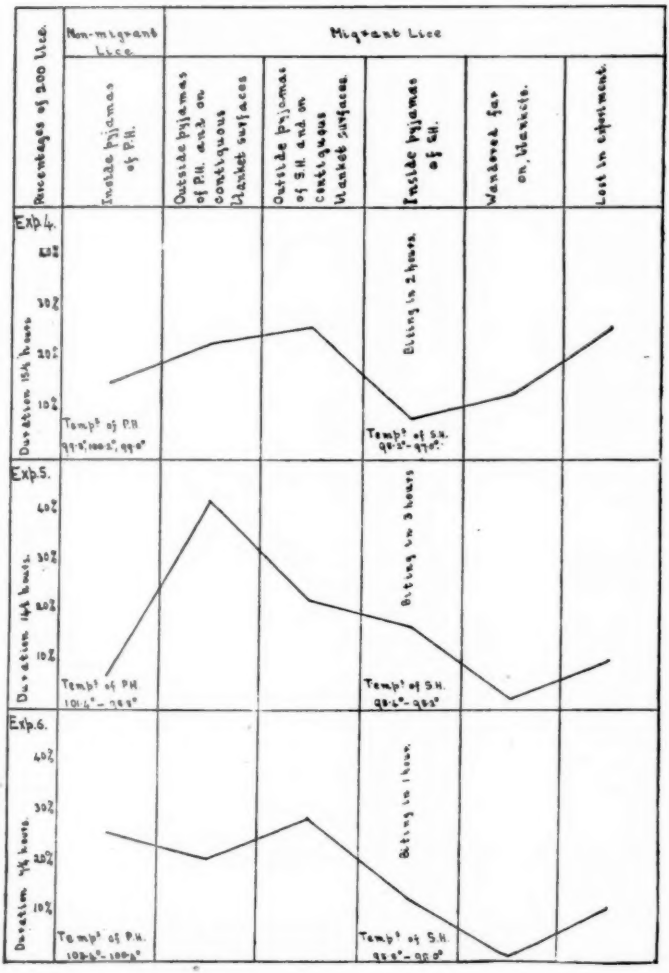


CHART II.

Illustrating the migration of lice from a febrile man (P. H. to an afebrile bedfellow (S. H.).

TABLE SHOWING THAT THE MIGRATION OF BODY-LICE IS INCREASED BY FEVER.

Number of experiment	Date	Condition and temperature of primary host	Condition and temperature of secondary host	Duration of experiment	Number of the 200 lice recovered	Percentage on or about primary host	Percentage on or about secondary host	Percentage inside pyjamas of primary host	Percentage inside pyjamas of secondary host	Time when secondary host was first bitten
A 1	February 6-7	Normal; 98·4°—97·8° F.	Normal; 98·3°—98·4° F.	15½ hours	184	65·7	34·2	38·0	19·5	Unknown, but not in first five hours
A 2	,, 12-13	Normal; sweated a little; 97·1°—98·2° F.	Normal; 98·4°—97·6° F.	16½ ,,	188	78·7	21·2	42·0	5·3	In sixth hour
A 3	,, 14-15	Normal; 99·0°—97·8° F.[1]	Normal; 98·4°—96·2° F.	15¼ ,,	159	80·5	19·5	40·9	8·2	In sixth hour
B 4	,, -4	Febrile; 100·2°—99·0° F.	Normal; 98·2°—97·0° F.	15½ ,,	167	54·0	46·0	17·3	9·6	In second hour
B 5	,, 9-10	Febrile; 101·4°—98·8° F.	Normal; 98·4°—98·2° F.	14½ ,,	179	56·4	43·5	7·8	18·4	In third hour
B 6	,, 10	Febrile; 103·4°—100·4° F.	Normal; 98·8°—98·0° F.	7¼ ,,	179	51·9	48·0	29·0	14·5	In first hour

[1] Temporary rise.

The temperatures attained by the sufferers from the three diseases carried by lice and characterized by a febrile condition are: in the case of typhus fever 103° to 104° F., sometimes 105° F.; in that of relapsing fever 104° to 105° F. usually; and in that of trench fever commonly 103° F., often 104° F. It may therefore be taken as proven that the fevers of these maladies tend greatly to increase the shedding of the lice from the patients, quite apart from their deaths, and that this phenomenon is partly accountable for the rapidity with which louse-borne epidemics spread.

The conditions of war or famine greatly favour the multiplication of lice, and at such times epidemics of trench fever are likely to occur; but over-crowding, dirt, and other depressing circumstances all reduce the inclination and opportunities for disinfestation, and provide conditions suitable to the transmission of the disease. It is easy, therefore, to see how "louse-borne" diseases have usually been regarded as "dirt-borne" in the past.

During the louse-borne epidemics in Roumania, which occurred in 1916 and 1917, it was noted that the assemblage of large numbers of persons at the centres established for vaccination and prophylactic inoculation led to an increase in the number of cases of typhus and relapsing fever, and there is every reason to suppose that there might be a like increase in cases of trench fever under similar circumstances.

A large number of cases of trench fever resulted among troops in the field from the use of bedding and clothing discarded by the sick and issued to healthy men before being thoroughly disinfested and disinfected. Even when measures to destroy the lice have been thoroughly carried out, such articles may still remain a source of danger. The excreta of the lice are infective and remain so after exposure to dry heat at 80° C. for twenty minutes—moist heat at considerably lower temperatures (60° C. for twenty minutes) is capable of sterilizing them. In addition, sunlight, prolonged keeping (four months and possibly longer), and exposure to soap and hot water, as used when clothes are washed by hand, have been proved, experimentally, to have no effect on the infectivity of the virus as contained in louse excreta. Such droppings of lice adhere to fabrics, and it is easy to understand how the blankets and garments of the sick are capable of transmitting trench fever when issued to other men. The excreta become detached when the article is again taken into use, and may enter cuts, scratches, open wounds, or even the conjunctival sac, and infection is the result.

The contamination of food by louse excreta is not a source of danger, as infection in man cannot be produced by the mouth; neither can infection take place by inhalation—a point of importance when we realize the ease with which the light dust, derived from louse excreta is blown about.

The prevalence of scabies may be an epidemiological factor of importance, for lesions of the skin result from this disease, and constant scratching ensures inoculation if infective louse excreta are present.

Now to turn to "seasonal incidence." Cold weather, in that it leads to the wearing of underclothing for long periods, and keeps people indoors and in close contact when fuel is scarce, is an epidemiological factor under the conditions already discussed. Atmospheric conditions, *per se*, do not appear to have any influence on the spread of the disease. The effects of atmospheric conditions on the prevalence of lice is too big a subject to consider here.



Fragment of sewing-cotton fouled by louse excreta. (× 30.)

The chief epidemiological factors are therefore: (*a*) human carriers of trench fever; (*b*) lice; (*c*) close association between the sick and the healthy; and (*d*) garments and bedding which have been discarded by the sick and then taken into use before being properly treated. The endemiological factors cannot be said to be known with certainty. Hereditary transmission of the disease in lice does not occur. The excreta of lice, though infective for four months after being passed, and possibly somewhat longer, have failed to produce infection when kept in the dry state for nine months or more. The human carrier case alone remains.

It having become evident that a large proportion of the cases of trench fever invalided home showed a tendency to pass through a subacute stage into a chronic condition, with symptoms of disordered action of the heart, and also, in some cases, of neurasthenia, two series of experiments were performed by us for the War Office Trench Fever Investigation Committee in order to obtain some further information as to the duration of these infections. The results, we think, establish the

fact that the sequelæ of trench fever are but a continuance of the disease and evidence of a persistence of infection—a finding that must have a far wider bearing on the field of medicine generally than on that of trench fever alone when other neurasthenic conditions are considered—and a finding that explains how an epidemic may be started anew.

Series I.—Chart III is that of a patient said to be suffering from neurasthenia, who complained of indefinite pains in the limbs, a tendency to excessive sweating, the symptoms of disordered action of the heart, and who was nervous, depressed, and much below his usual weight. Lice were fed on him from the two hundred and ninety-eighth to the three hundred and thirty-ninth day from the onset of his trench fever attack—that is to say, nine months from the commencement of

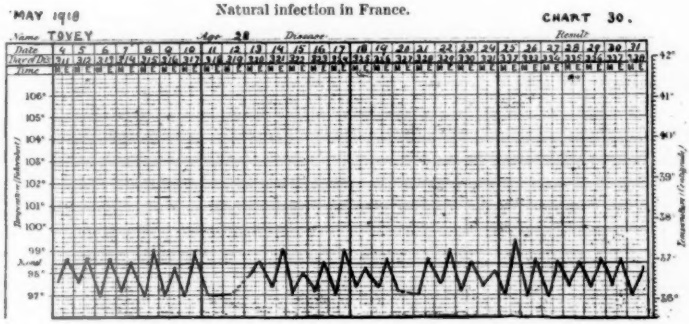


CHART III.

Patient said to be a neurasthenic. Lice were fed upon him from the two hundred and ninety-eighth to the three hundred and thirty-ninth day from the onset of his trench fever attack. During this period he was only febrile as shown. The excreta from these lice produced the infection shown on Chart IV.

his illness, the patient having been in England and under our observation from the early febrile stage of his disease, which was contracted under natural conditions in France. During the period the lice continued to feed he was only febrile as shown. The excreta collected from these lice, on inoculation into a healthy volunteer, produced an infection, the fever of which is shown on Chart IV. During this fever period, lice were fed on the volunteer and excreta from them inoculated into a second healthy man: Chart V shows the result. It can only be supposed, therefore, that this case of neurasthenia was really one of trench fever, in a form capable of infecting lice, and a danger to those

around him. The fact that the lice themselves were non-infective before being used in the experiment was proved by inoculation of their excreta into two healthy volunteers, who showed no apparent change as the result of this inoculation.

Series II.—A case similar to the one first described had been infected with trench fever in France, under natural conditions, fifteen months previously, and had been under observation in England for thirteen months when the experiment began. Two hundred and fifty lice, obtained by the kindness of Mr. Bacot, from the same clean stock as that used for the experiments in Series I, were fed on this man twice daily from the four hundred and forty-third day of his disease onwards, for a period of twenty-eight days. The excreta passed by these lice

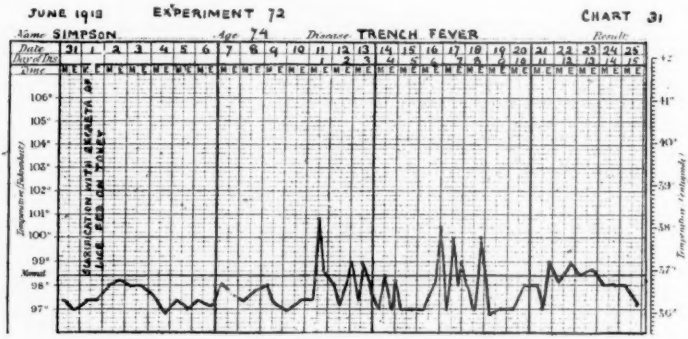


CHART IV.

The result of infection with excreta from lice fed on the neurasthenic patient whose chart (III) is shown above.

were collected and 40 mgm. were emulsified in normal saline solution, and injected subcutaneously into a healthy volunteer. Fifteen days later an acute febrile illness (temperature 103° F.) began, with frontal headache, pains in the loins, and pains in the knees. On the following day the eyes were pink, the tongue slightly furred, and rose spots appeared on the trunk—chiefly the abdomen. There was tenderness of the knee cartilages and of the lower portion of the left shin. Pain in the head and in the calves and lumbar region became so severe that morphia had to be administered. The area over the spleen was so tender that it was impossible to determine whether this organ was enlarged. Four days from the onset of illness, the volunteer was free

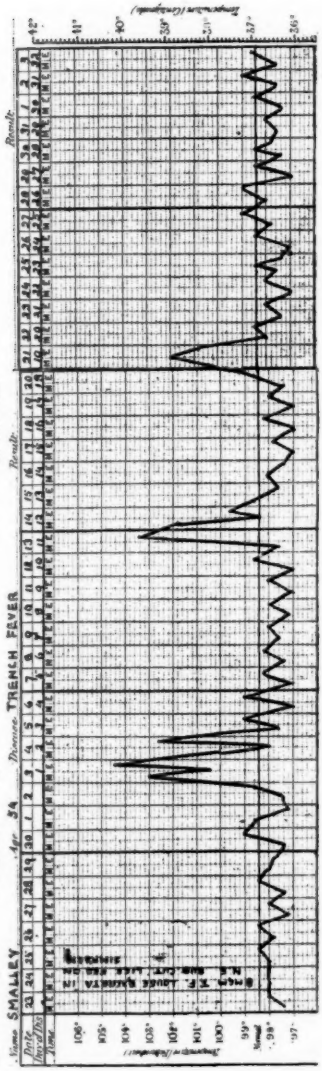


CHART V.

The result of infection with excreta from lice fed on the man infected from the neurasthenic case.

The three charts seem to indicate that the virulence of the infection has been enhanced by a series of passages through man and louse, and that even old chronic cases of trench fever are a danger to their associates if they are lousy.

of all symptoms, and no enlargement of the spleen could then be detected. On the following day severe frontal headache returned, there was extensive herpes labialis, and the temperature rose rapidly to 102° F. No further febrile relapses occurred, but convalescence was prolonged. Lice were fed on this volunteer from the fourth to the thirty-third day of his febrile attack, and their excreta, in turn, when inoculated into the skin of a healthy man, caused an acute attack of trench fever after an incubation period of seven days.

It is obvious, therefore, that the correct diagnosis of these late and persistent cases is of the greatest importance, both for the sake of the patients themselves, whose sufferings at present are little understood, and for the sake of those with whom they come in contact and who are, therefore, liable to be infected should lice be present. And we may say, when considering the endemiology of trench fever, that chronic carrier cases are the most important factor.

Finally, it may be well to summarize our present knowledge of the mode of transmission of the disease. Trench fever is conveyed from man to man by means of the louse (*Pediculus humanus*), both the body-louse and the head-louse being capable of transmitting infection. Such experiments as have been carried out suggest that fleas, bed-bugs, ticks, and mosquitoes are incapable of passing on the disease from man to man, and as the usual laboratory animals, including birds, have proved immune to infection, it is probable that no other intermediate host than the louse plays any part in the spread of trench fever.

The exact part that lice play in the transmission of the disease has been the subject of much discussion, but the main facts to be deduced from our experimental work and that published by other observers may be summarized as follows :—

(1) Lice that have fed on a trench fever patient become infective to healthy men after a period varying from five to eight days from the first infecting blood feed.

(2) Conversely, lice that have fed only on healthy men are incapable of causing trench fever.

(3) Lice once infected probably remain so for the rest of their lives, and certainly up to the twenty-third day of their infection. Such lice, however, do not pass on their infection hereditarily.

(4) When infected lice feed on healthy men, a certain number of infections will be produced; though the time that such louse feeding requires to be continued before infection results is often as much as thirty days or longer. Young men react more readily to louse bites

than do old men, and old men, though equally susceptible to infection when their skin is artificially broken, rarely contract trench fever when infected lice are fed on them.

In this connexion it is well to note that lice are constantly contaminated with their own excreta, masses of which may be seen, by means of a hand lens, adhering to their surface, and that, no matter what position lice are fed in, their excreta will generally reach the skin on which they are feeding, and often in a semi-fluid condition as the result of solution in the sweat.

(5) The excreta passed by lice, which have been fed on trench fever patients several days previously, are capable of producing trench fever in healthy men. The minimum interval varies in different instances from five to eight days.

(6) Such infective excreta may enter the body through abrasions of the skin, particularly those resulting from scratching induced by the irritation of lice, through gunshot and other wounds, and through the healthy conjunctival sac.

(7) A very high percentage of lice eventually become infected while continuing to feed on a trench fever patient. In this connexion an experiment we carried out is of interest. Lice, from a non-infective stock, were obtained from Mr. Bacot at the Lister Institute and fed for five days on an active case of trench fever, being placed on the patient for half an hour every morning and evening. Twelve lice were then selected at random, and isolated in twelve glass tubes, which were numbered 1 to 12. Thereafter the lice were fed separately, twice daily, on *healthy men*, and transferred to fresh sterile tubes each morning. The excreta passed each day were collected separately and examined for *Rickettsia* bodies. No *Rickettsia* bodies were seen in the excreta of any of the lice passed during the first twenty-four hours after segregation. By the twenty-sixth day from the first infecting blood feed, lice Nos. 7 and 9 were dead, and No. 12 had escaped. The excreta from all these lice, however, had been demonstrated to contain *Rickettsia* bodies before this. Of the remaining nine lice, No. 6 had never shown *Rickettsia* bodies in its excreta, while the other eight lice had done so—*Rickettsia* being intermittently present from the seventh day of the experiment, and consistently, and in increasing numbers, from the seventeenth day onwards. On the twenty-seventh day of the experiment, louse No. 6 (*Rickettsia*-negative) was crushed and emulsified in two drops of normal saline solution. One minute later, this emulsion was rubbed into a scarified skin area of a healthy volunteer.

TABLE SHOWING DAY AFTER INFECTING FEED ON WHICH LICE FIRST PASSED INFECTIVE EXCRETA.

Series	SOURCE OF INFECTION		DAY AFTER INFECTION OF LICE ON WHICH EXCRETA WERE PASSED															
	Name	Day of disease	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	23
1	Gatehouse ...	79	−		−		−			−				+ 8				+ 9
2	Jones	2	−					−	+ 13	−	+ 9	+ 7	+ 8		*a* + 13			
							+ 16	−	+ 13	−								

The figures below + results indicate incubation periods. *a* = Reinfection.

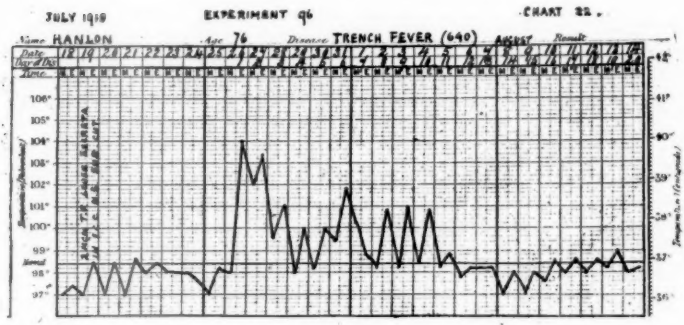


CHART VI.

The result of the subcutaneous injection of 2 mgm. of louse excreta.

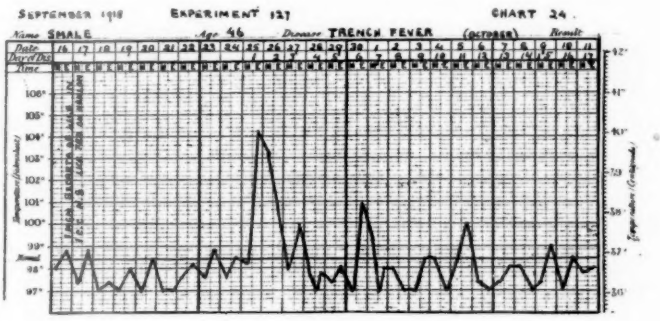


CHART VII.

The result of the subcutaneous injection of 1 mgm. of louse excreta.

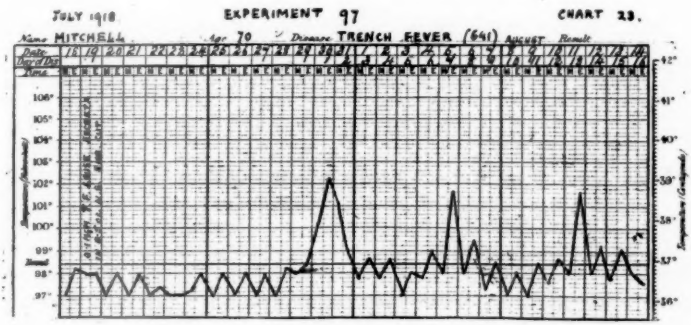


CHART VIII.

The result of the subcutaneous injection of 0·1 mgm. of louse excreta. 0·05 mgm. of this same excreta failed to produce infection. Note the increasing incubation periods as the dose was reduced.

Immediately afterwards, louse No. 8 (*Rickettsia*-positive from the eighth day) was treated in the same way, and inoculated similarly into another healthy man. The man who received the louse which never showed *Rickettsia* bodies, remained healthy during two months' observation, while the volunteer inoculated with the *Rickettsia*-positive louse, developed a typical attack of trench fever after seven days' incubation.

This result strongly supports Arkwright's findings with regard to *Rickettsia*, and suggests that eleven out of twelve lice became infected with trench fever as the result of ten feeds on a trench fever patient. Other experiments similar to the above, though modified in various ways, have provided much confirmatory evidence.

(8) The excreta from a single louse or its gut contents, are capable of producing trench fever.

(9) 0·1 mgm. of infective louse excreta has produced typical trench fever by inoculation subcutaneously.

(10) Lice can be infected by being fed when on an afebrile trench fever patient while the disease is still active, as indicated by pains in the limbs, and similar symptoms. Sometimes patients who have infected lice in this way have suffered subsequently from febrile relapses, while others have not.

(11) Even as late as the four hundred and forty-third day of disease a patient's blood may remain infective and be capable of infecting lice fed on such a patient while slightly febrile. A patient showing such persistent symptoms is by no means rare, though we have not been uniformly successful in transmitting the disease from such cases.

(12) Infection probably does not take place by the mouth or by inhalation, attempts to transmit the disease by means of infective louse excreta, in such ways, having failed.

(13) The evidence against transmission by mechanical transference of blood by lice is extremely strong; and from what has been said it is obvious that a healthy person may contract trench fever without ever having had a louse upon him, being infected by louse excreta dislodged from garments or blankets, or by louse excreta conveyed as dust through the air.

We see, therefore, that trench fever is conveyed by the excreta or crushed bodies of infected lice; that the virus may enter through the broken skin or unbroken conjunctiva; that rubbing and scratching promote infection, but that the bites of lice may cause a sufficient lesion to enable infective material to enter the body.

DISCUSSION.

Dr. BUCHANAN: I admire the greatness of the results which have been achieved by Major Byam and his colleagues by the experimental methods which have been so amply set out in the paper. I think it important clearly to distinguish between three conditions: (1) Those which favour the transmission of lice from man to man; (2) those which, given the presence of lice, favour the spread of typhus and relapsing fever from man to man; and (3) those which, also given the presence of lice, favour the transmission of trench fever from man to man. Notwithstanding all the brilliant investigations, our understanding of the epidemiology of trench fever is incomplete until satisfactory explanation is forthcoming of the different behaviour of trench fever epidemics on the one hand and those of typhus and relapsing fever on the other. I may refer to the evidence of the habitual association of typhus and relapsing fever in epidemics, laying special stress on this occurrence in the epidemics which are now prevalent in the Balkans and in Poland and Galicia. The general epidemic of trench fever on so many fronts in Europe in 1916 affected Poland, Austria and the Balkans, and *a priori*, there was as much reason to anticipate the continuance of chronic carriers of trench fever and local centres of trench fever infection in these regions as there was in the case of typhus and relapsing fever. It does not seem a sufficient explanation of the non-appearance of trench fever during the last year or two in these typhus and relapsing fever-ridden countries to suggest that the infection of trench fever has not been introduced or that trench fever might have been present but was overlooked. I have made inquiries in Poland and elsewhere of many doctors who were fully acquainted with trench fever as seen in 1916, and they had no suspicion that any trench fever was associated with the other louse-borne diseases with which they were constantly dealing. I may refer also to the practical absence of trench fever in Egypt and Mesopotamia where typhus and relapsing fever occurred, and where apparently all the conditions existed for the propagation of trench fever also, including the presence of men liable to relapses, who, on the experimental evidence given by Major Byam, might be supposed to remain active carriers of the infection for long periods. Since the regulations, made early in 1919, by the Local Government Board, trench fever has been notifiable in this country, but, with one exception, the notifications appear to have related to ex-soldiers who have contracted the disease in France. It is, of course, possible that cases of trench fever occur which are contracted in this country but are not diagnosed. One frequently hears of specialists in trench fever who discover the disease where it has been previously unsuspected. I hope that whenever this is done in a case which has apparently contracted the infection in England, the specialist concerned will keep in mind the requirements of notification imposed by the Ministry of Health regulations so that the facts of transmission can be fully investigated.

Mr. A. BACOT: As a member of the War Office Trench Fever Committee I am in too close agreement with my colleague, Major Byam, to be able usefully to discuss the lecture as a whole, but I will refer to a few points that have been mentioned this evening. With regard to the very valuable and interesting experiments conducted by Major Byam and Captain Lloyd concerning the migration of lice from patients when in a febrile condition: I would point out that in normal circumstances the lice are less likely to hurry away to a new host before infecting themselves than under the conditions of the experiment. During war both the patient and his sleeping companion would probably rest in more or less complete uniform in place of wearing pyjamas, while the lice instead of being suddenly released on a fevered skin would be distributed among the patient's garments, having had time gradually to dispose of themselves to their own comfort in accordance with the increase of his body temperature. Lice migrate to the outer garments when they find the heat or moisture between skin and underclothing uncomfortable or avail themselves of the better ventilated situations afforded at wrist, knee, fork or neck. Under these circumstances the insects would probably indulge their normal appetites for blood to satiety before they wandered on to a new host. The importance of the factors of humidity or drought in regard to the distribution of body lice, and as a consequence of the diseases they convey, was not sufficiently emphasized by Major Byam. Body lice appear to have a great dislike to sweat or sweat-moistened garments. Cold climates, even if humid, are not unfavourable, because of the dryness of the atmosphere between clothing and skin consequent upon the wide difference between the temperature of the host's body and that of the surrounding air. Dry hot climates, especially if the range between night and day temperature is great may also prove favourable to the insects, but hot humid countries are unsuitable. The danger of the spread of the disease by the excreta of infected lice in discarded clothing seems to me to be a relatively unimportant risk *provided that the active lice themselves are destroyed*, so that irritation due to their bites is avoided. Dr. Buchanan has referred to the scarcity of evidence of any spread of the disease having been caused in this country owing to the return of trench fever patients who might reasonably be suspected of acting as carriers. I would suggest that the reason is that, in the great majority of cases, upon the man's return home, even if his clothes do harbour a few lice, they are very speedily disposed of by the normal routine of the washing of underclothing and the use of separate night garments by both the man and his wife. The man's family may harbour head lice but these are unlikely to infest him owing to the shortness of his hair, and it follows that a link is wanting in the sequence of the cycle.

The PRESIDENT (in calling upon Dr. Arkwright to continue the discussion): Will Dr. Arkwright be good enough to explain the nature of the *Rickettsia* bodies? These bodies were first described in connexion with typhus fever, later they were found also in trench fever. Have they, therefore, any special significance in respect of either of these two diseases, and are there any

differences in the *Rickettsia* bodies of typhus and trench fever? I draw attention to a fact that has been established by certain of the observations made by Major Byam and others in trench fever—namely, that a very small amount of infecting agent is sufficient to infect a person. The virus contained in one louse is apparently ample. The same fact has been observed in typhus.

Dr. J. A. ARKWRIGHT: In reply to the President's question as to what *Rickettsia* bodies are, I am unable to do more than state that they have a definite appearance and are fairly easily recognized in films stained with Giemsa's stain, when they are present in large numbers. They resemble round or oblong bacteria and are very small, about 0·3 by 0·3-0·5 micron in diameter. Their relationship to bacteria, spirochætes or protozoa is undefined, and failure to obtain cultures on artificial media retards advance in our knowledge of them. They are found in enormous numbers in the stomach or mid-gut of lice which have fed on patients, when five to ten days have elapsed since the first infecting feed. It is not necessary that the lice should have fed on the patient during the whole of this time, so long as they have had a good meal of blood twice daily and have been kept at a temperature of about 30° C. between the feeds. The excreta of such lice also show very large numbers of *Rickettsia* bodies after about a week or ten days from the first infecting feed. This period corresponds very well with the time after the first feed at which the lice become infective for man. The variety of *Rickettsia* associated with trench fever was first described by Toepfer. Rocha Lima has also published observations on the subject in a paper written with Munk. We have done some work with typhus fever *Rickettsia* and virus obtained by feeding lice on human cases of typhus, and we have reproduced the disease in monkeys and guinea-pigs by inoculating them with the infected lice and passing on the disease by the blood to other animals. Our work on typhus has not been very extensive, but we have been able to satisfy ourselves as to the occurrence of a form of *Rickettsia* in very large numbers in infected lice, and we believe that we can distinguish the typhus organism by its staining properties, its slightly larger size and its more bacillary shape. These differences on the whole agree with those described by other workers, especially the fact that the typhus *Rickettsia* stains redder and the trench fever form a more purple colour. Since the papers on *Rickettsia* by Arkwright, Bacot and Duncan were published,[1] we have continued our observations in connexion with the clinical work of the Trench Fever Committee which has been carried on by Major Byam and his colleagues at the New End Military Hospital. We have had the advantage of the special knowledge and skilled help of Captain Lloyd, the entomologist to the hospital. We have now examined 108 boxes of lice which had been fed on sixty-four cases of trench fever, and from all these boxes we have obtained films definitely showing large numbers of *Rickettsia* bodies. Two boxes of lice fed on recent cases of trench fever did not show *Rickettsia*, but the reason for these two failures was probably that these two boxes were insufficiently examined; films were made

[1] *Journ. Hyg.*, 1919, xviii, p. 76, and *Trans. Soc. Trop. Med. and Hyg.*, 1919, xii, p. 61.

on two days only from one box (thirteenth and fourteenth day from the first feed), and one day only (seventeenth) in the other case. A number of old trench fever cases on whom lice were fed gave negative results as regards *Rickettsia*, showing either that these lice did not acquire *Rickettsia* from these cases or only in numbers too small for recognition. Six boxes of lice were fed on experimental cases of trench fever on the first day of the disease only and none of these showed *Rickettsia;* these were the only instances in which it was attempted to infect lice on the first day only of the disease. Two boxes of lice which were fed on two cases during the first and second day only, and on the third day only of the disease respectively, did not show *Rickettsia*, though lice fed later on the same cases yielded large numbers. It appears, therefore, that lice fed on one day only, and especially if that is the first day of the disease, may not show *Rickettsia:* this may be because they only become slightly infected, the septicæmia existing in the patient at this stage of the disease being of a low grade. *Rickettsia* can only be recognized when present in large amounts. On the other hand septicæmia may usually be absent at this early period of the disease. A very large number of examinations of boxes of lice fed on healthy men have been made, with negative results. Twenty-five boxes of normal lice fed on twelve healthy men have been examined repeatedly; some over long periods with negative results, and lice taken from six other healthy men, have also been found negative. Only on one occasion has a box of lice fed on a man not under suspicion of trench fever given a film strongly suggesting trench fever *Rickettsia*, but even this did not show definitely typical appearances. The typhus fever lice have also yielded uniformly negative results as regards trench fever *Rickettsia*, and the trench fever lice have never shown forms like the typhus fever organism. Lice fed on five other febrile men (phthisis, cerebro-spinal meningitis, &c.), have also served as negative controls. The association in lice of trench fever virus and *Rickettsia* is thus extraordinarily close and the control observations on lice from healthy men in London have been quite satisfactory. It is difficult to disbelieve in a causal relationship between the *Rickettsia* and trench fever. However, as no one has yet obtained a culture, and as the attempt to recognize definitely so small and simple an object as *Rickettsia* in small numbers in a blood film from a patient is too difficult a task, certainty has not yet been reached. It is interesting and important from the epidemiological point of view (1) that lice which have become infected with trench fever *Rickettsia* appear to remain infected till they die, and that their life is not apparently shortened by the infection; (2) that trench fever patients who are no longer febrile will often infect lice with *Rickettsia*. Lice coming into both these categories have been shown not only to contain *Rickettsia* but also to be infective for man.

Surgeon-Captain P. W. BASSETT-SMITH, R.N.: Cases of trench fever have been very few in the Naval Service. I have been through all our hospital returns from 1914-1919 and find only one case recorded. In the Gallipoli campaign we had a large force of the Royal Naval Division employed when

diseases were rampant. In France there were many Naval and Marine units but most of the sick were treated in military hospitals. Anyway the conditions on board naval ships do not favour the spread of the disease. The ætiology of the disease is still unknown but probably it is allied to the typhus and relapsing fever group. The clinical and epidemiological characters of that type known as "febris quintana" strongly suggest a spirochætal origin, and in view of the work of Leishman and Balfour on the granule-forming properties of spirochætes, can the minute *Rickettsia* bodies found in lice be distinguished from the spirochætal granules? At any rate much further investigation is required before it can be definitely stated that trench fever is not a spirochætosis. When we bear in mind the long period that was taken to demonstrate the very prevalent *Treponema* of syphilis, negative results count for little as proof against the theory.

Major-General Sir W. G. MACPHERSON: Major Byam's experiments refer only to the louse as the transmitting agent of trench fever. But, if I remember rightly, the first experiments of the American Committee in France, of whose work I saw a good deal, showed that direct inoculation of the blood of a trench fever patient produced the disease; so that there is a possibility of trench fever being contracted without the agency of the louse or its excreta. May I ask Major Byam to state how far this point was taken up in his experiments?

Dr. CARMALT JONES: I will comment on two points in Major Byam's and Captain Lloyd's paper. The first point is epidemiological, the second clinical. In describing the epidemiological distribution of trench fever Major Byam remarked on its occurrence in Egypt: this is quite contrary to my experience. I spent six months in Egypt at the end of the war, where I had access to all medical units outside the Army areas, but I never saw any cases of this disease, for which I was on the look-out, as I had become interested in it in France. I think Dr. Butler will bear me out that it was never mentioned at this time in the returns of diseases occurring among the troops, and when I discussed it with medical officers who had served only in Egypt they always told me that they were unfamiliar with it. Its absence was certainly not due to any lack of lice, which are ubiquitous in those parts. I recollect finding lice in my clothes after merely walking through the streets of Damascus, without even sitting down, an event which had considerable interest for me as there was a great deal of typhus fever in the place. Will Major Byam tell us at what time trench fever was prevalent in the Egyptian theatre of war? The second point is this: Major Byam has remarked on the difficulty of making a laboratory diagnosis of trench fever in the absence of any demonstrable parasite. There is, however, a clinical method of diagnosis which has a certain value; this is the presence of areas of hyperalgesia of the skin, corresponding to the distribution of the eighth cervical and first dorsal, the seventh dorsal and the lumbar segments. These areas, or some of them, are to be found in nearly all cases of trench fever, and in no other fever, I believe, with the exception of malaria, which is capable of accurate laboratory diagnosis. I came across

these areas in the following way: I was investigating segmental hyperalgesia in visceral lesions in general, and in heart disease in particular, for which I used to visit a "heart centre." In the course of this work I found the seventh dorsal segment frequently hyperalgesic, and I was unable to understand the connexion between this and any cardiac lesion. I referred the matter to Major H. F. Marris, R.A.M.C., an expert cardiologist, who asked me if these patients had had trench fever, and showed me a case of trench fever in an acute stage, in which it was easy to demonstrate this hyperalgesic area. I asked him if the characteristic shin-pains were segmental in origin, and he replied that he thought this was probable, and on examination the lumbar hyperalgesia was found. I made a large number of investigations on these cases after this, and bearing in mind that the patients often complain of ulnar pains, I looked for and found the same condition in the eighth cervical and first dorsal areas.[1] The presence of these areas will, I think, be found a very good rough guide as to whether any case of fever, other than malaria, is trench fever or not.

Major BYAM (in reply to Dr. Buchanan): Trench fever was well recognized by the Germans as occurring in the Eastern European war zones. When introduced into our Army on the Salonika front, by men from France, the disease spread till steps were taken to reduce the prevalence of lice. In France trench fever was rampant, a high percentage of our men were infected by lice, and yet the other louse-borne diseases were absent. In Germany, during the war, all forms of louse-borne infection were common. In Egypt and Palestine relapsing fever was the only louse-borne disease that assumed epidemic proportions in the Army, and the troops chiefly affected were the men of the Egyptian labour corps and those coming in contact with them. From all of which, I think, we may conclude that the spread of louse-borne disease into new areas is a slow process, requiring intimate contact between the sick and the healthy. That trench-fever-infected lice do not pass on their infection hereditarily must also do much to limit the endemic area of the disease. It is more than likely that trench fever will be passed unrecognized during an outbreak of the more serious louse-borne infections.

With regard to Mr. Bacot's remarks, our experimental subjects were clothed in pyjamas to facilitate the removal of the lice, but we always failed to retrieve a certain percentage of them. The fact that in both series of experiments the conditions were identical, with the exception that in one series the primary host was febrile and in the other he was not, would, we believe, justify us in attributing the marked difference in the wandering proclivities of the lice to the one difference in the conditions—namely, the presence or absence of fever. That trench fever often spreads in the absence of lice is unlikely, but the infectivity of the adherent louse excreta becomes of great importance when hospital bedding or clothing is under consideration, as the sick and wounded

[1] See *Lancet*, 1918, ii, pp. 443, 444.

present many portals of entry for the virus that are absent in the healthy. At the same time, it is likely that infective garments were often issued to louse-infested men whose lice were non-infected but whose skin was broken as the result of scratching.

Sir William Macpherson has raised the question of transmission by other means than the louse or its excreta. The blood of trench fever patients, as he says, is infective both during fever and in the afebrile intervals. Direct blood transmission was carried out successfully by us with blood obtained from patients on various days of the disease up to and including the fifty-seventh day. Three attempts to infect by applying the urinary sediment of trench fever patients to the scarified skin of human volunteers gave negative results, the urine being collected from patients at all periods from the first to the twenty-first days of illness. The fact that infection cannot be produced by the ingestion of known infective material robs such experiments of much of their importance.

That trench fever was ever prevalent in Egypt, I think, is more than unlikely. Having worked in that country for nearly twelve years, and having only left it at the end of 1916, I can say that I never saw a case there. During 1918, however, full records of cases in Egypt were sent to me by competent observers, and I am inclined to believe that sporadic infections did occur among those coming in contact with men from France. One case was that of an English hospital nurse. The Egyptians did not suffer from the disease either before or during the war, and yet they are particularly liable to contract louse-borne infections. Their time is probably yet to come. The areas of skin hyperalgesia alluded to by Dr. Carmalt Jones are well known to us, and I can corroborate his statements as to their value for diagnostic purposes.

Section of Epidemiology and State Medicine.

President—Dr. E. W. Goodall, O.B.E.

The Serbian Epidemics of Typhus and Relapsing Fever in 1915: Their Origin, Course, and Preventive Measures employed for their Arrest.[1]

(An Ætiological and Preventive Study based on Records of British Military Sanitary Mission to Serbia, 1915.)

(WITH MAPS AND CHARTS.)

By William Hunter, C.B., Colonel, A.M.S.

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[1] At a meeting of the Section, held November 28, 1919.

MAP OF SERBIA, SHOWING RAILWAYS AND FORTY-TWO CHIEF TOWNS AND HOSPITAL CENTRES.

ARMY AREAS: A—First Army.
" " B—Second, Third, and Belgrade Armies.
" " C—Chief Railway Towns.
" " D—Zeitjer Army.
" " E—Ujitze Army.

CIVILIAN AREAS: F.

THE CHIEF SEATS OF HOSPITALS WERE:—

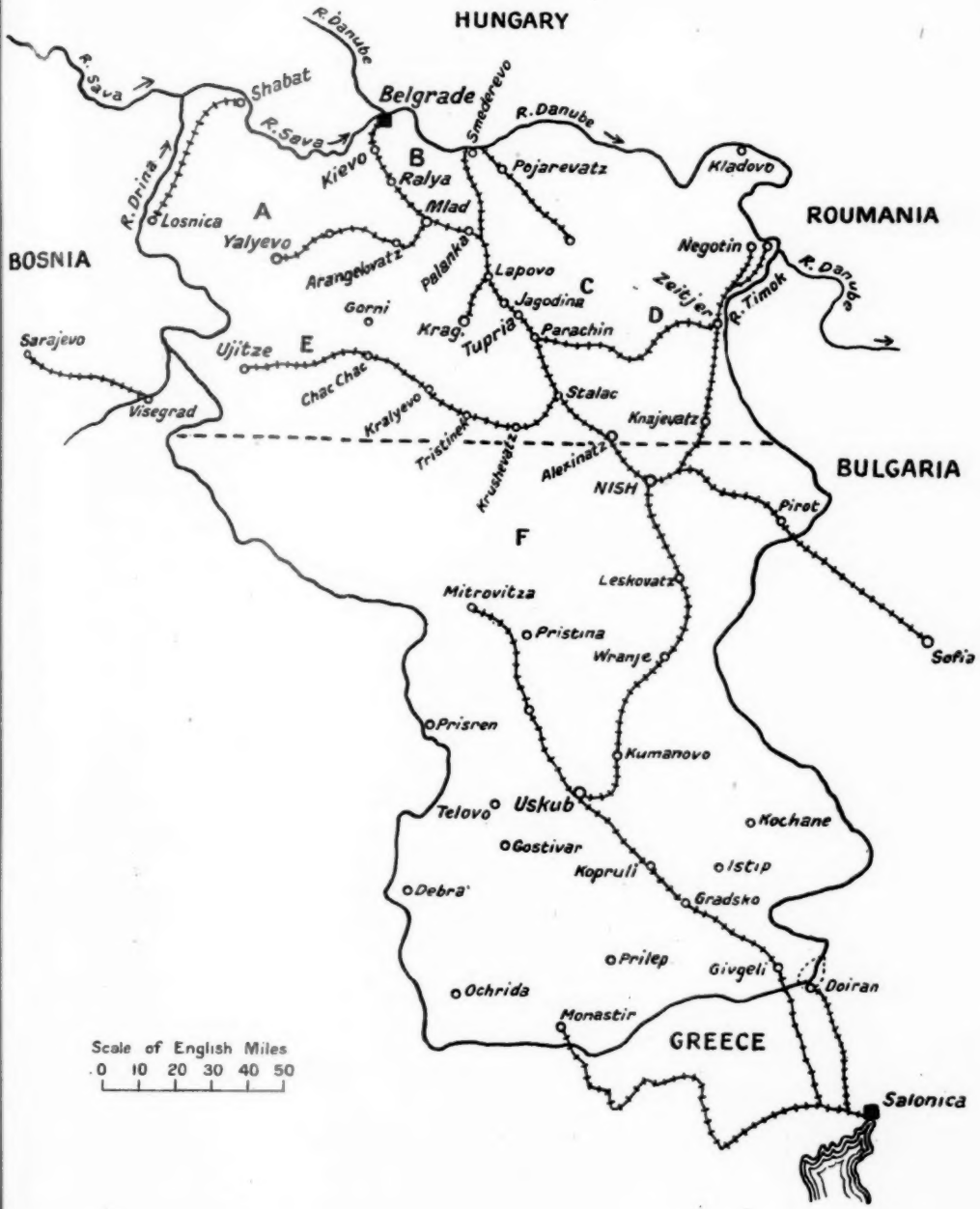
Area A ...	Valyevo.	
Area B ...	Belgrade. Palanka. Svilinatz. Kragujevatz.	
Area D ...	Zeitjer. Negotin. Kladovo.	
Area E ...	Ujitze. Chac-Chac. Kralyevo. Krushevatz.	
Area F ...	Nish. Uskub. Monastir. Givgeli.	

The Headquarters of the Mission were at *Kragujevatz*, G.H.Q.

The chief seat of its work was in Area B.

The seat chosen by it for its Quarantine and Disinfection Station to control the spread of infection from the Valyevo area A—the worst infected—to the rest of Serbia was *Mladenovac*, the junction between Areas A and B.

The English sanitary train for inoculation and disinfection operated from *Kievo* and *Ralya*.


HUNGARY
R. Danube
R. Sava
Shabat
Belgrade
Smederevo
R. Danube
R. Sava
R. Drina
Losnica
A
Kievo
B
Ralya
Mlad
Pejarevatz
Kladovo
ROUMANIA
BOSNIA
Yalyevo
Arangelovatz
Palanka
Lapovo
C
Jagodina
Zeitjer
R. Timok
R. Danube
Negotin
Gorni
Krag.
Tupria
Parachin
D
Sarajevo
Ujitze
E
Chac Chac
Visegrad
Kralyevo
Tristinek
Stalac
Knajevatz
Krushevatz
Alexinatz
BULGARIA
NISH
Pirot
F
Mitrovitza
Leskovatz
Pristina
Wranje
Sofia
Prisren
Kumanovo
Telovo
Uskub
Kochane
Gostivar
Kopruli
Istip
Debra
Gradsko
Prilep
Givgeli
Doiran
Ochrida
Monastir
GREECE
Salonica
Scale of English Miles
0 10 20 30 40 50

PART I.

Section I.—Character of Data.

The data on which this account of the great Serbian epidemics of typhus and relapsing fever in 1915 is based were furnished me and all official communications were made to me in Serbian (Cyrillic) characters, the latter so unintelligible that one could not even recognize the name, Kragujevatz, or the names of stations through which one passed. The data were furnished me every night, and their study and interpretation formed my continual occupation for three or four hours each night, on the conclusion of each day's work (involving visits, journeys, interviews, and continuous official work in headquarters) of the English Sanitary Mission to Serbia, sent out on February 15, 1915, by the War Office, under my charge. The data were of two classes:—

(1) The first of these related to the numbers of cases in hospitals in forty-two towns of Serbia, arranged alphabetically, and without, at first, any indication as to their position in relation to each other or to the various armies in the field. This relation I only ascertained as time went on, and eventually made clear to myself by the arrangement adopted in the analyses I afterwards made, and now show in the map and tables and charts now submitted.

(2) The second class of data, also furnished me nightly, related to the numbers and characters of the sick, wounded and infectious cases, and their distribution in the central military hospital and seven reserve hospitals in Kragujevatz. The latter proved to be of much interest and value, for the number of cases in hospitals in Kragujevatz were so large—constituting about one-tenth of the whole in Serbia—that their close study gave me a very clear picture and accurate sample of what was happening in all Serbia.

(3) As time went on, other data which I was constantly applying for were furnished me, so that eventually my records before I left Serbia about the middle of June, 1915, included the following. Many of them were so involved and so difficult to analyse that owing to my departure to Eastern war areas a week after my return from Serbia in June, 1915, my absence there for two more years as President of the Advisory Committee for Prevention of Disease in these areas, and my subsequent official duties in the Eastern Command which commenced two weeks after my return in 1917, it has only been in the past year, since the conclusion of the war, that I have found it possible to make the

detailed analysis of them now embodied in this account. The analysis referred to has involved a close study of the daily numbers of cases in hospitals, admissions, discharges and deaths in forty-two places in Serbia; the numbers of cases in war territories and field ambulances, the relation of the varying numbers at different periods to the preventive measures adopted.

The following are the data referred to:—

(A) WHOLE OF SERBIA.

General Return A.—(1) Daily numbers of cases of typhus, relapsing and enteric fevers in hospitals in forty-two towns in Serbia (January 1 to June 4, 1915). (Charts, pp. 44, 45, 95, 102; Tables, pp. 58, 76.)

General Return B.—(2) Daily numbers of admissions, discharges and deaths in above hospitals (April 14 to June 4, 1915). (Charts, *ibid.*; Tables, *ibid.*)

General Return C.—(3) Daily numbers of wounded and sick and of deaths; of empty beds; of the total deaths in hospitals (April 27 to June 4). (Table, p. 92.)

(B) WAR AREAS.

Special Return D.—(1) Daily number of typhus cases in war territories: (*a*) in hospitals; (*b*) in field ambulances of armies (March 28 to June 4).

Analysis D.—(2) Daily admissions and deaths in different war areas and civilian areas (April 26 to June 4). (Charts, pp. 66, 67, 96, 97; Tables, pp. 62-65, 78.)

(C) KRAGUJEVATZ DATA.

General Return E.—(1) Daily returns of numbers of wounded, sick, infectious cases, and total deaths in each of the eight hospitals (March 28 to June 4). (Chart, p. 87; Table, p. 85.)

General Return F.—(2) Daily numbers and character of cases of infectious disease in all Kragujevatz hospitals (March 14 to June 4). (Charts, pp. 86, 87.)

General Return G.—(3) Daily distribution of these cases among the various hospitals (March 28 to June 4).

Special Return H.—(4) Daily admissions and deaths of typhus cases in 1st Reserve Hospital (December 14, 1914, to May 14, 1915). (Charts, pp. 71, 72; Tables, pp. 70, 80.)

Special Return K.—(5) Mortality data for Kragujevatz military area (August 14, 1914, to May 13, 1915: (*a*) General mortality; (*b*) Mortality from typhus. (Chart, p. 86; Table, p. 82.)

Special Analysis L.—(6) Daily distribution of cases of typhus and relapsing fever in hospitals in forty-two places in Serbia. (Charts, pp. 46, 47.) (*a*) Daily numbers of cases in hospitals (March 27 to June 4): (*b*) Number of admissions and deaths (April 26 to June 4). In the different war areas and in civilian areas. (Charts, pp. 66, 67, 96-98; Tables, pp. 62-65, 78.)

This analysis was the most complicated of all, and has involved some five months' work. The results are shown in Charts, pp. 66, 67, 96-98.

Section II.—Application for help from the Serbian to the English Government.

It was under the circumstances (hereinafter detailed)—the full nature of which was unknown to anyone but themselves—that on February 9, 1915, the Serbian Government appealed to the English Foreign Office (under Sir Edward Grey) for the help of a mission of doctors—if possible about 100 in number. The response was immediate. The request was handed on the same day to the War Office (Lord Kitchener), and immediately submitted by him to the Director-General (Sir Alfred Keogh).

On the following day, Wednesday, February 10, Sir Alfred telephoned to me as senior physician to the London Fever Hospital, to confer with him as to the measures to be taken in the selection and command of a corps of twenty-five R.A.M.C. officers, which he had decided to send out to be attached to the Serbian Armies. This was the greatest number which it was possible for him to send, in view of the great calls which he had to meet for the British Armies in the field; and I was authorized to invite the help of any physicians or doctors willing to accept the duty. I attempted that evening to obtain the services of five physicians known to me, but without success; their official or other engagements preventing them giving up their duties at so short a notice.

On the following day (Thursday, February 11) I therefore accepted the duty and was appointed Colonel-in-charge of the Mission myself. The nature or extent of the chief epidemic diseases was not known at the time. It was reported to be "typhus." Whether this was "typhus abdominalis" (typhoid), or "typhus exanthematicus" (typhus), or "typhus recurrens" (relapsing fever) was not known, and I asked that a telegram be sent asking for information.

On the following Friday morning (February 11) I was informed by telephone that a reply had been received. The prevalent fever was "typhus exanthematicus." I was asked whether I could leave the following day (Saturday). I said I could, and I spent the day in providing the necessary equipment for Field Service.

The following day (Saturday), Major (afterwards Lieutenant-Colonel) G. E. F. Stammers, R.A.M.C.—an officer with wide sanitary experience

in India and South Africa—was appointed second officer in charge; Captain W. W. C. Topley, R.A.M.C., as bacteriologist, and twenty-two Lieutenants of the R.A.M.C., as members of the Mission, were directed to meet me at Victoria Station on Monday morning, February 15, "for foreign service." The place of service was not known to them; they joined me under the happy impression that they were on their way to France.

The title given to the Mission—thus rapidly formed and despatched—was that of "The British Military Hospital attached to the Serbian Armies." The instructions given to me were, that the Mission should not be employed in hospital clinical work, but that after ascertaining the actual prevailing conditions and characters of the epidemic, we should form for the Mission as a whole some definite programme of prevention work that would serve to check the epidemics in the Serbian Armies, and throughout the country.

The first and main object to be kept in view was the extreme necessity of the moment of restoring the health of the Serbian armies for fighting purposes, and freeing the land as quickly as possible from infection, in view of future possible military contingencies that might necessitate the operations of foreign military help to that country.

Provisional arrangements were made for a large supply of anti-cholera and anti-typhoid vaccine as required. The quantity of the former subsequently requisitioned by telegram on the day of our arrival and duly received and used by us was 300,000 complete sets of doses; each dose sufficient for two inoculations. At a later period 200,000 more were requisitioned and duly sent off.

The Mission was to be officially attached to the Serbian Armies, to whom all arrangements regarding provisioning, supply of interpreters and orderlies were left. Under normal circumstances the Serbian authorities might have been able to meet these requirements. Under the abnormal conditions we found on arrival this was not possible, and the absence of interpreters in particular formed from the first one of the greatest of the many difficulties I had to contend with.

Thus constituted and equipped, the Mission left London on Monday, February 15, Marseilles on February 22, and arrived in Salonica on Tuesday, March 2, and reached Nish on Thursday morning, March 4.

Sudden and hasty as our departure from London had been, equally sudden and unexpected was our arrival in Salonika on Tuesday. Our departure from England had not been announced to the Serbian Government, and the first information they received was my telegram

which I sent off according to my instructions, on our arrival at the Piraeus on Monday, March 1, the day before, to the English Military Attaché (Colonel Harrison) at G.H.Q., Kragujevatz. This only reached him on the Tuesday afternoon, twelve hours or more after our arrival in Salonica, at 6 a.m., on Tuesday, March 2. It was immediately communicated to the Prime Minister, who immediately wired to the Serbian Consul-General at Salonica (received and conveyed to me at 10 p.m., on board) and sent off a special officer, Lieutenant Petronevitch, to meet the Mission on its journey north to Nish. The latter had just time to catch the train from Nish at 8 p.m., on the Tuesday night, and meet our train next day shortly after we crossed the Serbian-Greek frontier on our journey to Nish.

On our arrival at Nish, about 8 a.m., on the Thursday morning (March 4) we were met by the chief officers representing the various departments of the Serbian Government—namely, by the British Minister (Sir Charles des Graz); the Assistant Secretary for Foreign Affairs (M. de Grouitch), representing the Prime Minister and Foreign Secretary (M. Paschitch); the Chief of the Sanitary Department (Colonel Karanovitch); the Chief Inspector of Military Hospitals (Colonel Sondermayer), and other Serbian officers.

Two hours later, accompanied by Lieutenant-Colonel Stammers, I reported to the Secretary for War, who extended a warm welcome, and then commenced our work by a long conference with the chief officers of the Sanitary Department. Within two hours we had ascertained all the available facts regarding the epidemic conditions, the origin of the outbreak, and the numbers of sick and wounded, and the conditions of pressure and difficulty which the country had to face from the insufficiency or absence of the equipment necessary to deal with those actually sick. Still more, the entire absence of any measures for the protection of those who were well.

On the following morning we were presented by the English Minister to the Prime Minister (M. Paschitch) who, speaking in German, explained to me very fully the position of matters in the country, and their difficulties in getting all the supplies they had ordered. In the afternoon we had a long conference with the Parliamentary Sanitary Commission formed specially to co-ordinate and support the efforts of the Military and Civil authorities; and during each day we visited and inspected the hospitals in Nish—a town normally with 20,000 inhabitants—now crowded to a degree with 100,000 refugees and others driven out of the Northern war areas.

SECTION III.—MILITARY CIRCUMSTANCES PRECEDING THE EPIDEMICS.

The military circumstances under which the outbreak occurred in December, 1914, were interesting and dramatic enough. Amid the crowded events in the French and Russian war areas at that period, they drew but little attention. They can now be seen in their true perspective, and they exercised a profound influence on the course of subsequent events in all the Eastern war areas.

The Austrian declaration of war against Serbia on July 28, 1914, found Serbia exhausted by two years' fighting in the first and second Balkan wars in 1912 and 1913—first against the Turks, and secondly against the Bulgarians in 1913. Belgrade, her capital, on the opposite side of the Danube from Hungary, could be, and was at once bombarded and had to be evacuated. The Government retired to Nish; one division of troops being left behind to prevent the Austrians crossing the Danube, or the Sava, as long as possible. The defence was a very remarkable one. All the Austrian attempts to cross in this neighbourhood were driven back with heavy losses; but it was fully recognized that they were only in the nature of feints. The real Austrian attack was arranged to come from the Bosnian frontier, in the direction of Valyevo and Ujitze (*see* Map, p. 31), and the main armies of defence were grouped on the line Valyevo to Palanka.

It came in force in August from the direction of Losnica over a front of a hundred miles. They forced their way across the Sava and the Drina (at Shabatz, Leshnitza and Losnitza), along the valley of the Yadar, converging on Valyevo, with the object of capturing that place, and driving a wedge between the first Serbian Army at Valyevo and the second and third armies operating south of the Danube and Sava. The fortunes of war swayed for five days in the Battle of the Yadar, August 18, and turned eventually in favour of the Serbians. The Austrians were repulsed and had to recross the Drina, followed by the Serbians who then penetrated into Bosnia. The Battle of the Yadar was the first great battle won by the Allies on the European fronts. The Austrian losses were 8,000 dead, 30,000 wounded and 4,000 prisoners; the Serbians lost 3,000 dead and 15,000 wounded. The advance and retirement of the Austrians were attended by the greatest atrocities to the population. "Towards such a population"—so ran the official Austrian order—"there is no room for feelings of humanity or generosity." The campaign was stated to be not a war but an execution.

The second campaign of the Austrians followed in September on much the same lines—namely, across the Drina from the Bosnian frontier, but further south. For six weeks the Serbians once more held them back over a line 100 miles in length in the mountains near the frontier, but the small Serbian army was holding too long a line with inadequate forces and supplies. They had to withdraw to a shorter line (the Kolubara River). In the beginning of November the Austrians advanced in great force: the Battle of Rozhan gave them the key to Valyevo, which was evacuated by the Serbians, and the Kolubara mountains—the key to the whole district. Belgrade had to be evacuated, and by the end of November the Austrians occupied a line reaching from Chak-Chak to Belgrade. The position of the Serbian army was precarious in the extreme, with only one narrow-gauged single line railway for their supply, utmost lack of ammunition, and unsupported by any big guns. They defended as best they could the heights of Rudnik, but they had to yield them, leaving the road open to Kragujevatz, the seat of their only arsenal. With Austrian armies advancing from Belgrade, in the north, and from Valyevo in the west, there was immediate danger that the whole Serbian army would be surrounded and captured. Everything depended on the nerve and patriotism of of the Serbian armies at the critical moment, and the story is told of how their old king (King Peter) strove to encourage them at this moment. He went forward to them in their trenches, and addressed them:—

"My dear Brothers,—You have taken two oaths: one to me, your king, and one to your country. From the first, I release you; from the second no man can release you. If you decide to return to your home, and if we should be victorious, you shall not be made to suffer. As for me and my sons, we remain here."

Not a man left his post, and King Peter kept his word and stayed with them until they entered Belgrade again. Towards the end of November new guns and fresh ammunition arrived from England. General Mischitch attacked again, stormed the Suvobor mountains on December 4, taking the Austrians completely by surprise, drove their right wing in headlong flight along the road to Valyevo, and later on once more across the Drina; drove their left wing across the Sava over the mountains near Mladenovac, and finally entered Belgrade on December 13—thirteen days after it had been captured by the Austrians.

Their two invasions, it is estimated, cost the Austrians 330,000 men

(of whom 40,000 were prisoners) with enormous loss of guns, ammunition, transport and stores. The victory was so great and complete as to be almost incredible to the Serbians themselves. It was regarded by the Serbians as a "crowning mercy." The extent of their victory, little known at that time, may be judged from the reference to it by General Ludendorff in his subsequent "Memories," 1919, i, p. 111 :—

"A shadow fell on the proud satisfaction with which we contemplated the development of events on the Eastern front (1914). The Austrian army had suffered a heavy reverse in Serbia. At the end of November it had penetrated far into that country. Belgrade had been taken on December 2, 1914. Austria-Hungary rejoiced greatly. Yet, as early as the days of the capture of Lodz and the Battle of Limanova (in Poland) the Austrians were retiring from Serbia defeated. They were no longer an effective fighting instrument. At first they had underestimated their opponents; now they went to the opposite extreme and overestimated them. The enemy's numbers alone terrified them. This feeling, and a certain feeling of impotence in the face of the Russians, have proved too much for the courage of the Austrian Army."

Outbreak and Spread of Epidemics of Typhus and Relapsing Fever.

But the cup of Serbian misfortune was not yet full. The Austrians in their retirement from Valyevo left behind them a trail of disease of typhus and relapsing fever that eventually spread to the whole country, and cost the Serbians suffering, loss, and misery exceeding, if possible, even that brought on them by the losses and atrocities of the war itself. The exact extent of their losses will probably never be known. But from data at my disposal, it is within the mark to estimate that in the course of the next three months—January to March, 1915—the number of typhus and relapsing fever cases in Serbia, in a population of about 3,000,000, fell little short of 500,000 cases, and the number of deaths to at least 120,000.

Origin and Spread of the Epidemics.

The general health conditions of the Serbian army and throughout Serbia as regards prevalence of infectious fevers were fairly good up to the middle of December, 1914.

This general information, conveyed to me on my arrival on March 3, 1915, was afterwards confirmed by later data obtained by me relating to typhus mortality in the wide Kragujevatz army area from August, 1914, onwards. These showed that no deaths from typhus occurred during August, September, and for three weeks of October, 1914. In

the last ten days of October, six cases of death were recorded, followed by nineteen in November, and by fourteen cases in the first half of December (*see* Table, p. 82; Chart, p. 86).

Early in December the Serbians began to take Austrian prisoners-of-war and shortly afterwards illnesses began to appear in the following order: Relapsing fever, typhus exanthematicus, enteric (but not so badly as the above), small-pox (a few cases). The latter did not give them much trouble, as vaccination is compulsory in Serbia, both in the army and among the people. But the conditions as regards the former steadily grew worse from that time onwards up to the time of the arrival of the English Sanitary Mission on March 3, 1915.

(The Kragujevatz data above referred to showed that the number of deaths from typhus, which had been 19 in November, rose to 30 in December, 126 in January, 554 in February, and 581 in March.)

Seat of Outbreak.

The first and worst seat of outbreak was Valyevo, a small town of about 10,000 inhabitants, situated in the north-west of Serbia, near the Bosnian frontier at the end of a single narrow-gauged railway line, and about 80 to 90 miles south-west from Belgrade. When the Serbians retired from this town in the second invasion of Serbia by the Austrians there were no cases of typhus among the troops, and they left there about 200 Serbian wounded with medical officers in charge.

When they again advanced, early in December, they took more and more prisoners, up to the number of 40,000, and when they returned to Valyevo they found more than 3,000 Austrian wounded and sick with typhus and relapsing fever under the worst possible conditions. The sanitary conditions were filthy. In one building, quite a new school, they found 150 dead Austrians in the cellars; and in the courtyards adjacent both men and cattle had been buried, many of them only partially, under a foot of soil.

Spread of Disease from Valyevo.

The conditions were so bad that the Serbians were obliged to evacuate the foremost places, and to establish concentration camps along their lines; and their 40,000 prisoners were, most unfortunately, distributed throughout the country to supply the labour so much needed everywhere. Typhus and relapsing fever rapidly followed this concentration and distribution, and were distributed along the whole line of railways to all the chief towns of Serbia, as far down as Monastir and Givgeli close to the Greek frontier.

The extent of infection and rapidity of its spread were shown by the figures of typhus and relapsing fever cases in hospitals during January and February, which I afterwards obtained. These rose from 2,400 on January 2 to 6,500 on February 1, and 11,500 on March 3 —the figures at the time of our arrival; subsequently to the maximum of nearly 17,000 cases by the end of March. These totals were made up as follows:—

	Typhus	Relapsing	Total	
January 2 ...	200 ...	2,200 ...	2,400 ...	
February 1 ...	1,200 ...	5,300 ...	6,500 ...	
March 3 ...	4,100 ...	7,400 ...	11,500 ...	Arrival of English Mission
April 2 ...	8,200 ...	8,500 ...	16,700 ...	

These figures were cases in military hospitals—all men. The number of cases in the civil population was not known. From data I subsequently obtained the number was at least five times greater.

Conditions in the Serbian Medical Corps.

The position in which this great outbreak found the Serbian Medical Corps was an exceedingly difficult one. It had just gone through two great wars—first against Turkey, and immediately followed by one against Bulgaria, and it had entered on this far greater World War without having had time to recover and re-organize itself, and utilize the experience gained by its previous two wars. On account of the unceasing and uninterrupted strain put upon it, its members became, as they officially reported to me, finally, utterly insufficient to deal with this new and strenuous work placed on them by so great an epidemic. They had successfully coped with, and been able to deal with all their wounded, but when, after the last defeat of their enemy, they had to re-enter the territories recently in the possession of their enemy and found them so filthy and so infected, they felt overwhelmed by the new tasks with which they were faced.

The Serbian doctors—whose total number was always small (not more than 450 in the whole country before the war)—had bravely and courageously accepted this great new fight against epidemic disease, and they soon began to take ill themselves and die in great numbers. By the end of February, when we arrived, sixty-two had already died, 200 had been ill and were not fit for duty, and less than 200 doctors were available for the whole country. In the first army in the Valyevo area—first affected—out of sixty doctors originally available, only fifteen remained fit for duty; ten to twelve doctors had already died

there. In one of its six hospitals there had originally been five doctors, three were down with typhus and only two remained for duty. The Serbian Medical Corps had endeavoured to concentrate its forces on this place, but had not sufficient reserves for that purpose, for the epidemic which had originally started had rapidly spread to all parts of Serbia. They had made an endeavour to clear out one of its towns of all its inhabitants, and turn it into a concentration hospital camp for isolation of its epidemic cases, but this measure could not be carried out for various reasons—military and others.

Section IV.—Conditions on Arrival in Serbia, March 4, 1915.

Our departure from London on February 15—after only some forty-eight hours in which to make our final preparations—had been hastened at the express wish, I was informed, of Lord Kitchener, who was extremely desirous to bring help to Serbia as speedily as possible. The fact that he was personally much interested in its success gave great support to my subsequent work, for his interest in Serbia was fully reciprocated by the intense respect and admiration which all Serbians had for his name. That interest was continued throughout our work, as I was happy to learn from the Director-General, Sir Alfred Keogh, in a letter written on April 15, received from him early in May:—

"We follow your work with the greatest interest. I hope you will continue to use all R.A.M.C. personnel for sanitation and administration, and not for hospitals. Plenty of men can be got for hospital work only. Continue to send me your reports on technical matters. Lord Kitchener is interested in them, and watches your work closely."

The largest number of typhus cases admitted into any fever hospital in England, Scotland or Ireland, in the years 1847 to 1870, had been 2,493 cases—viz., in the London Fever Hospital in 1864. The number of cases seen by Dr. Murchison in the period referred to—on which he based his classical accounts—was over 20,000. The mortality was 19 per cent., or about 1 in 5.

The experiences of the week after our arrival were to include many surprises, as will presently be seen, fully confirming, and going far beyond anything above described from the past history of the disease. I little knew then, that within a few days I was to be amidst conditions in which I was to see not 1,000 cases in a year, but 1,000 cases in a day; to find a maximum not of 2,500 cases in a year, but of over 4,000

cases in the hospitals at the time, rapidly rising to over 8,000 in the course of the next three weeks, plus an unknown number among the civilian population of Serbia, probably three or four times as many, of whom there was no information.

New cases arrived in hospitals at estimated rates as high as 1,000 to 1,500 a day.

There was a mortality not of 19 per cent. (1 in 5) but of 1 in 3—viz., 30 per cent., in January and February, and rising in a week or two to nearly 1 in 2, 40 per cent., in March.

That mortality was, as usual, highest amongst the doctors, ten to twelve doctors already dead in one hospital alone; out of an original total of 450 doctors, sixty-two dead (soon rising to over 100) and 200 unfit for duty; in the army fifteen doctors to a division instead of sixty; in one hospital out of five doctors, three down with typhus, and only two at work; in the first hospital visited in Serbia, containing 956 cases of typhus, relapsing and enteric, there were only two young doctors in charge, one of them, a handsome young Swiss, dead about ten days later; my first official duty on the day after my arrival two days later in Kragujewatz being to attend the funeral of a brave young Scottish mission worker, Miss Neill Fraser, of Edinburgh.

And lastly, to find hospital conditions appallingly overcrowded and insanitary, with 13,000 wounded, over 37,000 sick, and over 16,000 fever cases; of typhus, over 4,000, soon rising to 8,200; relapsing, 7,500, rising soon to 8,500; and enteric over 2,000.

Conditions on Arrival.

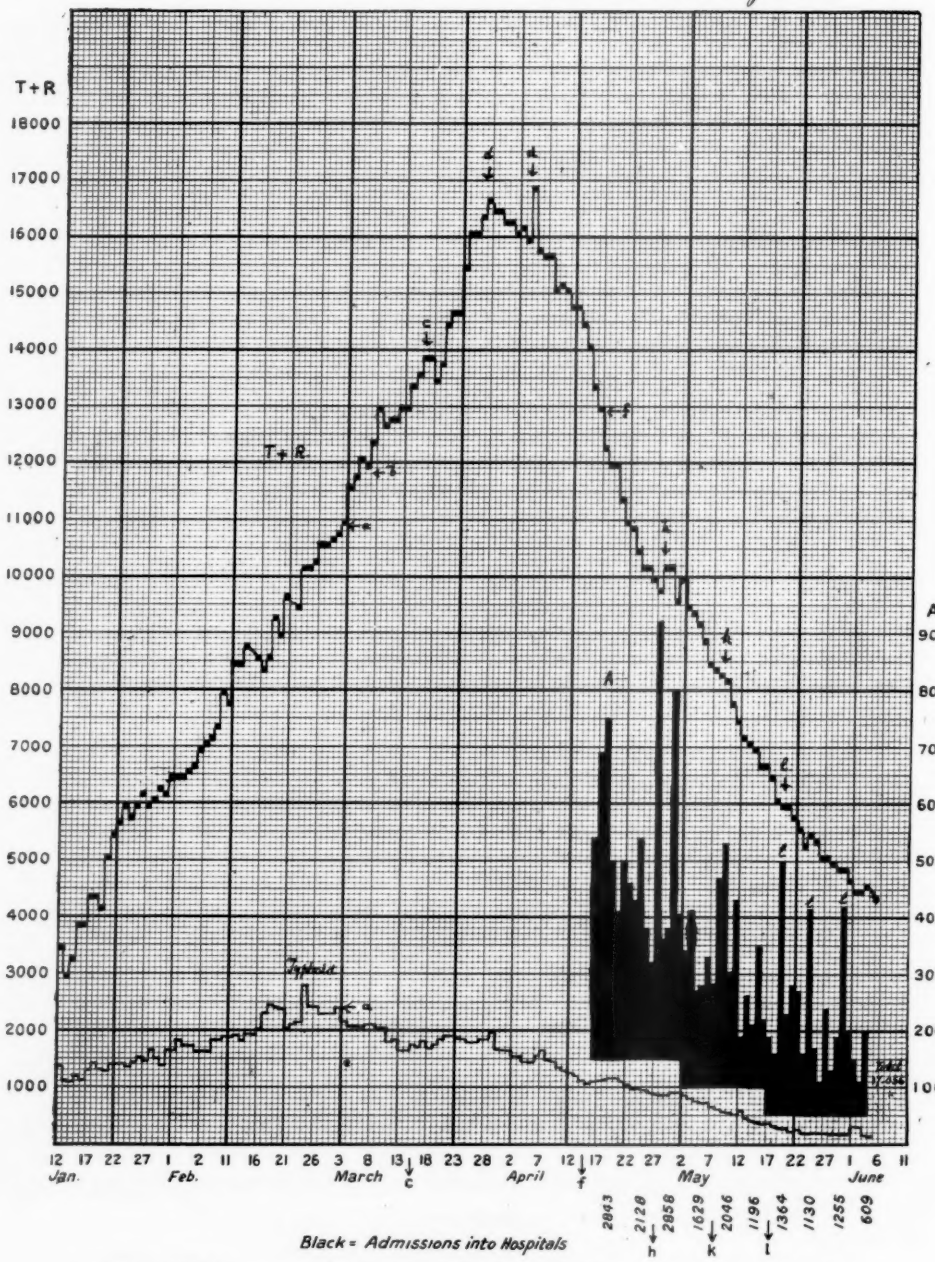
They were the following, as summarized in my official telegram and reports:—

Epidemics of typhus and relapsing fever, widespread throughout the country, in every town, village and hamlet—a raging pestilence, uncontrolled (*see* Charts, pp. 46, 47).

Cases in hospital on March 3: Relapsing fever, 7,429; typhus, 4,157; rising in the course of the next four weeks. March 31: Relapsing fever, 8,220; typhus, 8,085; estimated daily admissions of typhus, 1,000 to 1,500 cases; mortality from typhus, 40 per cent.

Conditions in hospitals, indescribably bad; overcrowded, sanitary arrangements non-existent; no disinfection arrangements. "*Urgent, urgent* need, not for surgical appliances and help and surgical measures, but for beds and blankets, bed linen, shirts and clothes for the mass of 15,000 fever patients."

Chart 1 Course of Epidemics of Typhus & Relapsing Fever in Serbia 1915.
(N°. of Cases in Hospitals). A = N°. of New Cases (April 17-June 4)

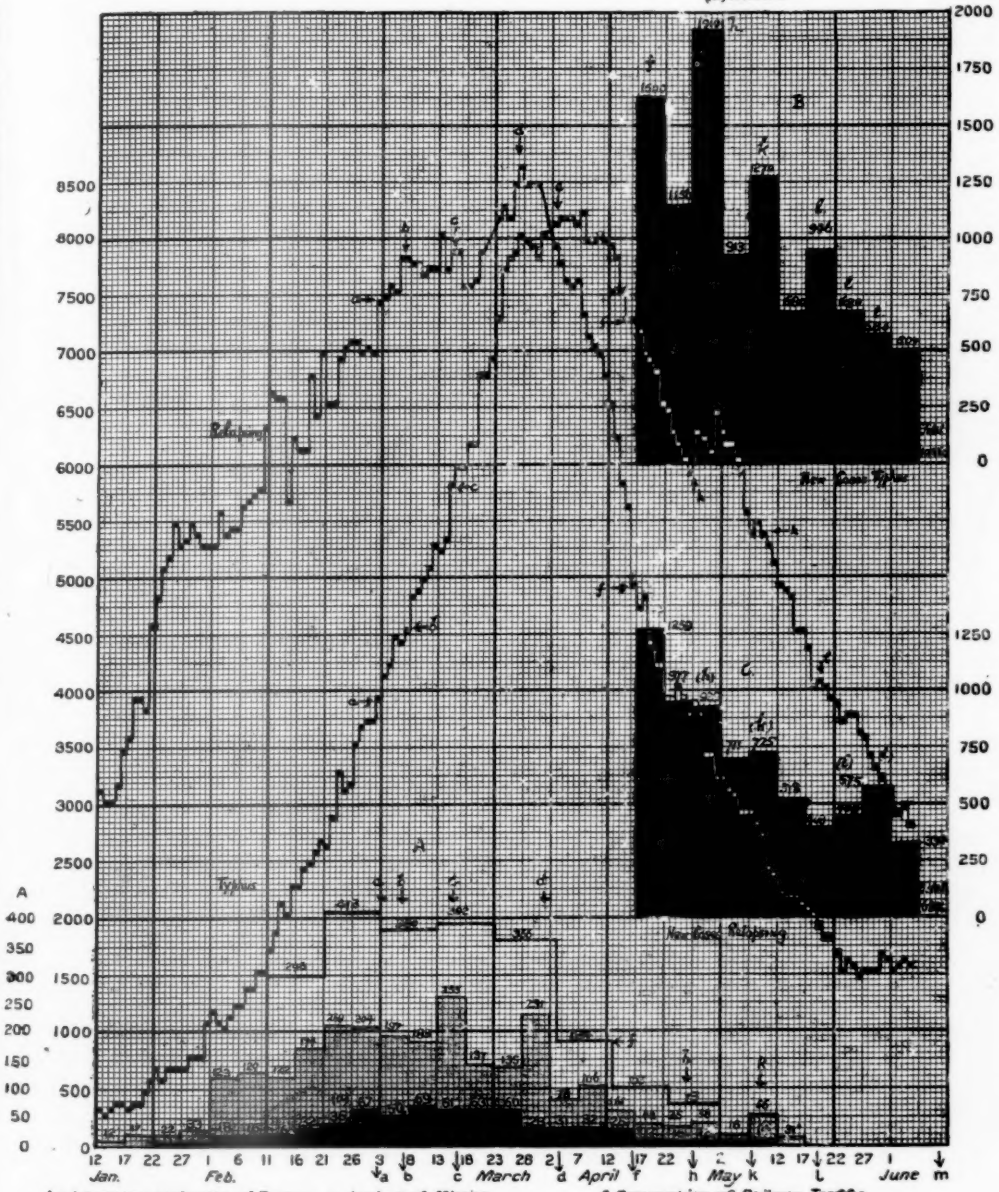


Black = Admissions into Hospitals

a = Arrival of Mission
b = Programme of Prevention
c = Suspension of Railway Traffic
d = Arrest in No: of Cases in Hospital 10 days after c.
f = Resumption of Railway Traffic
h = Increase of Cases in Hospital 10 days after f.
k = A Second increase 10 days after h.
L = A Third increase 10 days after k. due to evac. of F^d Ambul. (Chart 7,
m = Mission left Serbia.

Chart 2. Course of Epidemics of Typhus & Relapsing Fevers in Serbia as shewn by

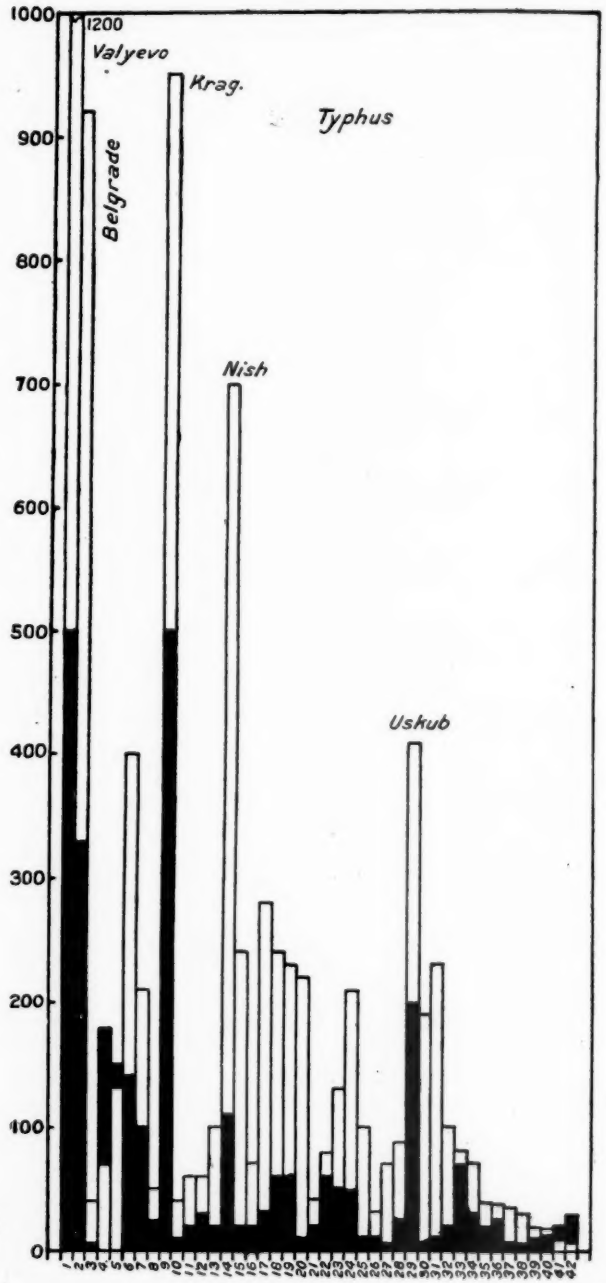
T+R. Nos of Cases in Hospital (Jan 12-June
B Typhus New Cases (April 17-June
C Relapsing " " " "
A Admissions of Typhus 1st Reserve Hos
(a) Deaths " " " " "



A = Admissions + (a) Deaths of Typhus in largest Fever Hospital in Serbia
B = Admissions into all Serbian Hospitals (Typhus)
C = Do. Do. (Relapsing)

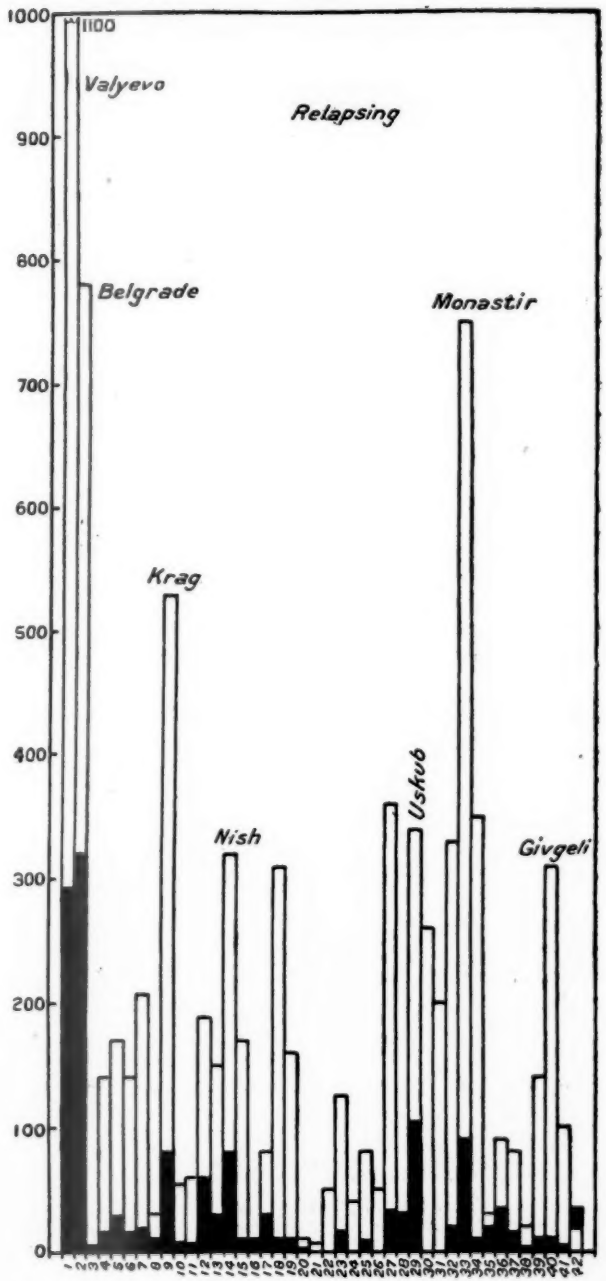
a. Arrival of Mission
b. Programme of Prevention
c. Suspension of Railway Traffic
d. Arrest in No of Cases 10-12 days after c.
f. Resumption of Railway Traffic
h. Increase of Cases 10-12 days after f.
K. A Second increase 10 days after h.
L. A Third " " " " K (See chart 7)
m. Mission left Serbia

CHART A.



This chart shows the numbers and distribution of cases in forty-two hospital centres in Serbia: (*a*) at end of March (clear block) when the numbers were at their maximum—viz., 72 per cent. in war territories and 28 per cent. in central (14-16) and Southern (27-42) civilian areas; (*b*) on May 31 (dark block)—viz., 80 per cent. in war areas and 20 per cent. in civilian areas. The total fall between April 3 and May 31 was: In war areas, from 5,810 to 2,420, or 59 per cent.; in civilian areas, from 2,400 to 670, or 76 per cent. Column 4 shows the increased number of cases held up by the Quarantine and Disinfecting Station at Mladenovac.

CHART B.



This chart shows the number and distribution of cases in hospitals in forty-two hospital centres in Serbia: (*a*) at end of March when the numbers were at their maximum—viz., 44 per cent. in war territories and 56 per cent. in central (14-16) and southern civilian areas (27-42); (*b*) on May 31 (dark block)—viz., 67 per cent. in war areas and 33 per cent. in civilian areas. The chief areas of disease were Valyevo and Belgrade (1 and 2) as the result of infection conveyed after resumption of railway traffic on April 16 by refugees returning to their homes in these areas; the proportion in these areas rose from 33 per cent. on April 26 to 49 per cent. on May 31.

Hospital needs, dictated by the conditions, and immediately telegraphed by me to the War Office: Tent accommodation for 16,000 patients, blankets and bedding for 10,000, sheets for 30,000, socks for 20,000, bed gowns for 10,000, slippers for 10,000, clinical thermometers 2,000, urinals 5,000, bed pans 1,000, baths 300.

Remedial measures being carefully considered by us, exceedingly difficult; no labour and utter lack of material to provide necessary accommodation, or to carry out the simplest sanitary measures; climatic conditions (weather very cold, wet and snowy) precluded living in the open air, and no tents available. Conditions appallingly bad, widespread and quite out of control; to check them will be extremely difficult, if at all possible. Strong measures will be necessary to get things put right, as the appalling nature of existing conditions does not seem even now to be at all fully realized.

In the whole of Serbia 450 doctors originally, of whom sixty-two are dead, soon rising to over 100, and 200 unfit for duty. The Serbian Medical Authorities, as they officially wrote, utterly exhausted and overwhelmed by this terrible epidemic coming upon them after three wars in less than two years.

The warmest welcome therefore extended to the English Mission sent out in about forty-eight hours after receipt of their appeal for help—arriving in Salonica unexpected by the Serbians, who had therefore no time to make any preparations for us, and our first accommodation in Nish in empty cavalry barracks—this sudden arrival itself creating impressions favourable to our work:—

"Just like the English; we ask for 100 doctors to help; they apologize for being able to send only twenty-five, but they send them at once—and they arrive unexpectedly, provided with all their kit and asking only for empty barracks in which to sleep."

Such was the impression and comment made by the Foreign Office representative, who met us on our arrival in Nish at 8 a.m. on March 4, as he told me some three years later. And he then described to me how he had visited our barracks and seen our surroundings—and taken photographs of them—such surroundings and conditions that two of my officers were down with high fever and septic throats, which necessitated their invaliding home four weeks later.

Conditions in Hospitals on Arrival.

The above data on arrival served generally to make clear the widespread nature of the epidemics so far as the armies at least were concerned. But they represented very imperfectly the degree of stress on the whole land, the widespread gloom that existed, the fear that permeated the population inducing them to seek safety in places other than their own homes, thus spreading the disease by travelling in crowded trains; arriving in places—e.g., Nish—where no accommodation was available, and sleeping where they could get any shelter of any sort, mostly on the floors of the eating-houses, where they congregated during the day. The full extent of this was not clear to me in the first few days, but only became manifest by my experiences on travelling to Kragujevatz the following week. What was made clear, however, by these first experiences in Nish was the severity of the conditions in the hospitals in which the sick were intensely overcrowded; the utter inadequacy to meet the pressing needs, in respect of doctors, nursing attendance, accommodation, ventilation, and worst of all, sanitation and disinfection. Thus, in the first hospital visited by me in Nish—as described in my first report on March 7—there were 700 fever cases in all, with only 200 beds amongst them; only two young doctors in charge—one of them a brave young Swiss doctor, Dr. Ernst, who took typhus a few days after I saw him and died the following week. Groups of thirty, forty, fifty and sixty cases of typhus, relapsing and typhoid patients all mixed together, lay in ill-ventilated rooms, close together on wooden boards, in some cases three or four in two or three beds, with one blanket covering the whole, many of them on the floor; and some of them under the beds and in the passages between the beds.

There were no nurses: the only attendants being Austrian prisoners; no bath rooms or arrangements for washing; no sanitary arrangements. The sewage from typhoid patients and others was discharged through the floor into an open ditch just outside the ward; no arrangements existed for disinfection, either on admission or discharge. The only thing of this kind shown me was a small brick kiln capable of disinfecting one or two articles at a time. It was explained to me that this was quite good if a piece of blotting paper were placed on each article, and if the disinfection were stopped when the paper began to turn brown! The arrangements in various inns and eating-houses used for typhus patients which I afterwards visited the same day were of a similar or even

worse character. In one small inn I found a small bedroom unoccupied, and on my asking what it was for, it was explained to me that it was reserved for visitors. An instructive experience which made clear the dangers run by anyone occupying a room even for one night in any house whose previous history or freedom from infection was not guaranteed. No such place, it might be assumed, existed. One other great source of spread of infection was also made clear by these first inspections, and fully confirmed by all my later experience. This was the absence or inadequacy of all disinfection, of arrangements for washing or bathing, supply of hospital linen, blankets and sheets for the patients either on admission or discharge. Hence they infected each other while in hospital, and spread the disease to others on their discharge.

As already stated, the patients lay on straw palliasses which naturally harboured lice, and which it was impossible to disinfect, covered by dark blankets which, owing to the needs of the ward, it was impossible to spare to be washed. Therefore fresh cases, arriving in hospital for one disease—e.g., relapsing fever—were as likely as not placed in beds which had just been vacated by a patient dead from typhus or discharged for typhus. As relapsing fever and typhus were frequently mixed up in the same ward, cross infection must have continually occurred. Under these circumstances, and in the absence of any system of disinfection on discharge, the hospitals were an actual source of danger to the patients themselves, and the patients a danger to others on their discharge. This danger, as I had occasion to learn afterwards, was fully realized by the public themselves; to be sent to hospital was a dread to them; little short of a sentence of death.

The most striking and pathetic illustration of these conditions I had occasion to observe was in one instance in a large town with many hospitals, all of them without any means of disinfection. On inquiry I was always told that disinfection was carried out in a special disinfecting station. This was situated outside the town; and not very accessible to any of them. On visiting this, I found one disinfector (a Berlin pattern) which had obviously been little used, and was at the time not working. It was in charge of a caretaker who had a small house of two rooms with a kitchen. In each of the two small rooms—sufficiently large for one bed—I found twelve typhus patients, lying on the floor. There were twelve women and young children in one—one of the children lying in its mother's arms—and twelve men and boys in the other; two of the boys standing in their shirts trying to keep themselves warm near the stove. In the room occupied by the

women there was a beautiful bed with white sheets and silk counterpane in the corner. On inquiry, I learned that it was for the wife of a well-to-do officer who was expected; and, just as I was leaving, a fiacre with the lady and her husband drove up. The picture of that gentle patient, seeking refuge in that spot—in a room crowded with typhus patients, without any nursing other than could be given by the caretaker (whose official duties included the disinfection of typhus clothing), was one which drove home to me more than any other experience I had the desperate straits to which those suffering from typhus were put in Serbia at this period.

A further fertile source of infection in hospitals was supplied by the want of adequate accommodation for attendants. In the hospitals staffed by Serbians the only attendants were Austrian prisoners (mostly convalescents and a few male nurses). These attendants and other servants slept in the wards with the patients.

The Nature of the Problems.

In all these directions, and others subsequently noted, the conditions noted in these first few days of my experience in Serbia—in respect alike to the widespread character of the epidemics, and the utter absence of adequate measures or equipment to deal with them, presented problems of extraordinary difficulty. The problems were of two kinds—the clinical one of improving the hospital accommodation, necessary to deal with so many cases in the absence or lack of all the necessary nursing, clothing, bathing, disinfecting and sanitary arrangements. Secondly: the preventive problem of checking its spread to the healthy.

The Clinical Problem.

One's first desire on seeing the conditions under which these patient sufferers lay, was that of affording whatever medical help was practicable to the patients themselves.

In the absence of any proper accommodation, equipment or nursing arrangements, the amount of help possible to afford at this period was very limited. It was confined for the most part to supplying the patients with such food as was available, or with such drugs or stimulants—also limited in supply—as might enable them to pass through the crisis of their disease—one of the worst of all diseases in its severity, but one of the most gratifying in the rapidity of its recovery

and convalescence after the crisis has been reached about the fourteenth day. A patient with typhus is like a ship in a storm; neither physician nor pilot can quell the storm, but by tact, knowledge and able assistance, they may save the ship.

The mortality at this time, as I afterwards found, was about one in two cases—viz., 40 per cent.

Mortality from Typhus in Kragujevatz.

No exact figures of mortality were available at this time. It was stated to be very high: to bury the dead was itself a difficulty in the absence of labour or material. Bodies lay unburied in groups even up to 200. The estimate of mortality ranged from 30 to 50 per cent., and figures which I got at a later period (May) in Kragujevatz fully confirmed this estimate. Figures which I afterwards obtained for the chief typhus hospital in Kragujevatz (the First Reserve) and the largest one in Serbia, showed that up to this period, March 2, out of 700 completed cases (admitted between December 19 and March 2) the number of deaths had been 207—a case fatality of 30 per cent. In March the fatality in 829 completed cases (i.e., discharged or dying) was even higher—viz., 331 cases, or 40 per cent.

These data were naturally not known to me at the time now referred to—viz., early in March. They were only obtained by me piecemeal and analysed later. They are introduced at this place to indicate by actual figures the severity of the conditions presented at the time of my arrival—the arrest or amelioration of which was the problem to be faced. They indicated clearly the extreme gravity of the clinical problem of the amount of sickness and fatality of the cases and the hospital problems which had to be faced in each hospital and without the immediate resources at hand by which these needs could be met. But bad as these conditions were, contributing as they did both to the increase, spread and mortality of the disease, they were not the causes of the wide outbreak.

Prevention Problem for the Whole Country.

But urgent and pressing as these needs were, and strongly as they appealed not only to myself but to my officers to give clinical help, my instructions were not to disperse my officers amongst the hospitals for clinical work, but to form a programme of prevention which would check the epidemics in the armies and affect all parts of the country—

not merely those already stricken with disease lying in hospitals. How that problem was to be solved was the subject of intense perplexity and thought, and gave us great concern during the first days of our experience in Serbia.

Section V.—Problems and Measures of Prevention.

The preventive problems were of two classes—viz., (*a*) those relating to lice-borne diseases (typhus and relapsing fevers); (*b*) those relating to water or sewage-borne diseases (typhoid, now prevalent; and contingently, cholera, the outbreak of which, early in the summer, was anticipated by all).

The most pressing of these was that connected with lice-borne diseases. It was of two kinds. First and foremost, there was the immediate urgent need of devising some simple method of disinfection with the materials at hand capable of being easily improvised by every one, and put into use at once both in hospitals and the houses of the people. The necessity was urgent and pressing, and never was the old adage, "necessity is the mother of invention," better illustrated than by the "barrel disinfector" contrived within forty-eight hours by the capable sanitary officer of the Mission—Lieutenant-Colonel Stammers.

The second necessity was to drive home to the people themselves the facts regarding the lice-borne mode of spread of these diseases, and the urgent need for them to combat these by their own individual efforts. This was equally well met by the communication No. 5 (of March 7, 1915) and by the suggested pamphlets and posters (in communication No. 6) of the same date.

The Serbian authorities, essentially practical in their outlook, and made keenly alive to the necessities of the situation, quickly appreciated the practical nature of these measures and recommendations, and put them into effect as speedily as possible. They recognized that they changed the entire aspect of the problems with which they were faced. It was made clear to me that the chief problem which they had put before themselves up to the time of our arrival was to provide hospital accommodation and equipment for the patients—their appeal had been for doctors. The other aspect now put before them by the arrival of our Mission and our recommendations, was the even more urgent necessity of preventing the spread of the diseases by concerted measures of prevention fitted by their simplicity and their wide applicability to

check that spread, and thereby relieve the pressure on the hospitals by diminishing the number of new cases.

It was, then, by preventive measures—disinfection against typhus and relapsing fevers, improved sanitation and water control against the existing typhoid, and preventive inoculation against the expected cholera, or against typhoid—that these epidemics could best be checked or controlled, or in the case of cholera altogether prevented.

Therefore these three classes of measures were recommended in the seven communications sent in on Monday, March 8, within three days of our arrival, as our first endeavour to meet the needs of prevention, and brought prominently to the notice of the Serbian authorities and of the Parliamentary Sanitary Commission formed specially to meet the needs of the situation. I met this Commission in a long interview on the day after our arrival, and the communications referred embodied our chief recommendations and many others of a practical nature which we had placed before them on that occasion. These related first and foremost to the lice-borne nature of the diseases—typhus and relapsing fever—and a strong appeal called on every man and woman in Serbia to help to the utmost in the war against disease just as they had fought successfully against their human foes.—"Death to all vermin; how to wage this new war; how to kill lice, bugs and vermin by simple practical domestic measures."

The Measures that should be adopted by Government, Town and Hospital Authorities to prevent the spread of Typhus and Relapsing Fevers.

(1) By compulsory notification, isolation, and disinfection, in the case of hospitals.

(2) System of notification in towns and villages—and by householders.

(3) Personal cleanliness of patients and of destruction of vermin in all bedding, clothing, bedsteads.

(4) Removal to hospital where possible.

(5) Personal interest of each individual in combating these diseases.

(6) Public interest to be aroused by pamphlets and newspapers.

(7) Supplies of disinfectants and arrangements for disinfection by public authorities.

(8) Formation of public disinfecting stations; and lastly,

(9) *The formation of the newly improvised steam or "barrel disinfector"* devised by Lieutenant-Colonel Stammers, so simple in

character, so inexpensive, and so easily made, that they could be formed in any number required in every village, and even in every household in the country.

Sequel to the Preventive Measures taken.

How far that decision was wise, the sequel will show. Following the measures advised and taken on March 16 the numbers of new cases of typhus contracting the disease, which prior to March 16 had been 1,000 to 1,500 a day, were reduced in the course of ten to fourteen days by March 29 to one half, and in the course of one month, by April 16, to 230 daily, and by May 15 to 115 a day. The hospital staffs thereby obtained what they most needed at this trying period—viz., some time and opportunity to put the hospitals into better order, and infinite credit is due to the officers in charge of such hospitals for the improvements, amounting in some cases to transformation, which when thus relieved they carried out without additional medical help in the course of four to six weeks.

But of even more importance was the fact that the measures we left in force ("barrel disinfectors," railway van disinfectors, a disinfecting station at Mladenovac, cleansing and bathing stations, hospital accommodation at Mladenovac) on the departure of the Mission from Serbia in June were of such a nature as to be permanently applicable to the prevention and control of the recurrence of the epidemics—should such occur (see *postea*). These measures dealt with in the first seven communications related specially to the civil population and civil authorities. The response to them on the part of the Parliamentary Sanitary Commission was immediate and practical. Within a week they came up to Kragujevatz (G.H.Q.) to which we had gone forward on the following day (Monday, March 8) and they immediately formed a "State Sanitary Commission for Prevention of Disease" to act as their executive and give effect by financial and other means to the recommendations and measures.

With this body I was afterwards in close relation in connexion with the hospital and disinfecting arrangements we recommended in Kragujevatz, and equally applicable to all other towns in Serbia. The result was that, although our duty lay specially with the armies (not with the civil population), the conferences and discussions held with the Commission were rendered of advantage to the whole country by the application to it of the measures employed in Kragujevatz.

Propaganda.

An extensive propaganda was undertaken to make these barrel disinfectors known throughout the army and the country generally, urging their production and use. "The 'Columbus Egg' of simplicity and efficiency" they were enthusiastically described by the director of the railway works at Nish, where I had taken one and demonstrated it in the Parliament House, and also in the Prefecture before the Medical Society of Nish. It was also seen by M. Paschitch, the Prime Minister, who gave orders that a hundred should be made at once and sent out as samples to the prefects in all towns, with instructions to get others made in each place.

In the Arsenal, at Kragujevatz, I urged their formation in as great a number and as quickly as was possible. I sent out some to each of the English Missions at work, and in season and out of season I continually came into conversation with all officials with whom I could converse in German; I spoke of them in strongest terms of recommendation.

A course of instruction was formed by Major Protitch, the Director of the Central Military Hospital at Kragujevatz and P.M.O. of the Schumadia Division of the Army, ten to fifteen non-commissioned officers from the different regiments being brought at a time to Kragujevatz for three or four days, receiving for eight to ten hours daily practical instruction in their use. The course was a most successful one, as shown by the following report furnished to me by Major Protitch on April 14, a week or two after the course was started:—

"At your personal request, I have the honour of informing you that I have terminated the teaching of forty-nine men how to disinfect. These were sent to me from corresponding units of Kragujevatz Garrison. I have further terminated the teaching of 117 persons about disinfection. These were sent to me by several communities of the Kragujevatz district: for the Kragujevatz community, seven persons; to each other commune, one to two persons. Besides this I have proposed to the chief of the Kragujevatz district:—

"(1) That each commune and village should be provided with one improvised 'barrel disinfector' (barrel and boiler), a Vermorel spray, and a hair-cutting machine.

"(2) That each village council shall at any time have a large store of lime for distribution for disinfecting purposes.

"(3) That men who have been instructed in disinfection shall be freed from any other work, and be used only for carrying and disinfection.

"(4) That the chief of each district shall be supplied with a larger quantity of carbolic acid (about 1,000 kilos) or creolin to each council.

"(5) To every one employed in disinfecting, small booklets will be sent ('Instructions for Disinfection') which will be printed in a few days and edited by the Sanitary Commission."

In acknowledging this and congratulating him on his work, I expressed my warm appreciation of the good work he was doing in regard to disinfection: the teaching measures he had taken and was carrying out were precisely those that would be most helpful in preventing the spread of infective disease.

Programme for the Army.

The practical measures for cleanliness and disinfection, especially the provision of "barrel disinfectors," applied equally to the army—the disinfectors, indeed, specially to the army, since one barrel disinfector to each company of 250 men sufficed amply and efficiently for their complete disinfection every two weeks. But these measures were of a character which would necessarily take some little time to be put in force: and the immediate need of the moment was for some more general measures —applicable at once—that would serve to control the raging epidemics then in force, and check their further distribution throughout the country.

During our first few days in Nish our observations related to conditions, causes and factors which we had been able to observe in visiting its hospitals, and our recommendations, as above summarized, related to their prevention. The factors responsible for the wide spreading of infection throughout the whole country were made clear to myself and my colleague Lieutenant-Colonel Stammers on our journey north to Kragujevatz the next day (Monday, March 8). This was the first journey we had made in daylight since our arrival in the North of Serbia. The conditions observed on our journey, in the carriages of the train and in the stations through which we passed, were those of extreme pressure and overcrowding, crowds of peasants in their sheep skin coats—the friendly homes of countless lice—being seen everywhere trying to get seats in the train. The carriages of the train reeked with the smell of naphthalin and camphor used in a vain attempt to prevent infection. The conditions were clearly such as to favour the spread of infection to an extreme degree. The whole railway system was acting —as I described it in my report—as "rivers of infection conveying infection up and down the country"—from the army areas to the civilian areas, and from the latter back to the army.

TABLE I.

Course of the Epidemic of Typhus, Relapsing Fever, and Enteric in Serbia, January to June, 1915, in relation to the work of the British R.A.M.C. Sanitary Mission, as shown by numbers of Cases in Hospitals on the dates specified.

Date	Typhus	Relapsing	Enteric
January 1	113	1,776	496
,, 6	124	2,184	753
,, 11	243	2,981	1,246
,, 16	378	3,532	1,116
,, 21	622	4,506	1,260
,, 26	730	5,233	1,491
,, 31	1,107	5,122	1,677
February 5	1,256	5,458	1,658
,, 10	1,557	6,347	1,880
,, 15	1,782	6,715	1,851
,, 20	2,587	6,449	2,411
,, 25	3,125	7,072	2,405
March 2 (*a*) ...	3,964	6,997	2,383
,, 7 (*b*) ...	4,569	7,874	2,099
,, 17 (*c*) ...	5,997	7,894	1,818
,, 22	6,797	7,904	1,878
,, 27 (*d*) ...	8,040 (*e*)	8,661 (*e*)	1,856
April 1	7,952	8,114	1,641
,, 6 (*e*) ...	8,024	7,550	1,527
,, 11	7,984	6,791	1,295
,, 15	7,300	5,663	1,075
,, 16 (*f*) ...	7,324	4,960	1,100
,, 17	7,219	4,748	1,115
,, 18	6,984	4,861	1,126
,, 19	6,947	4,451	1,160
,, 20	6,779	4,230	1,120
,, 21	6,604	4,253	1,114
,, 22	6,555	3,930	1,041
,, 23	6,317	3,918	979
,, 24	6,209	4,039	985
,, 25	6,054	3,967	937
,, 26 (*g*) ...	5,979 (*g*)	3,808	858
,, 27 (*h*) ...	6,314 (*h*)	3,904	878
,, 28	6,249	3,815	849
,, 29	6,158	3,455	846
,, 30	6,475	3,463	882
May 1 (*i*) ...	6,343 (*h*)	3,265	865
,, 2	6,260	3,250	857
,, 3	6,263	3,166	797
,, 4	6,107	3,110	751
,, 5	5,938	2,941	722
,, 6 (*l*) ...	5,598	2,943	694
,, 11	5,138	2,398	505
,, 16	4,529	2,149	381
,, 21 (*m*) ...	3,957	1,870	221
,, 26	3,635	1,516	198
,, 31	3,098	1,642	188
June 4 (*n*) ...	2,860	1,609 (*n*)	160
	(Patients in Hospitals mostly convalescent.)		
,, 10	Mission left Serbia for Salonika.		
,, 14	Mission left Salonika for Malta.		

TABLE I—(*continued*).

(*a*) Arrival of Mission.

(*b*) Programme of prevention and disinfection. "Barrel Disinfector" devised.

(*c*) Railway traffic suspended and leave from army stopped. "Railway Van Disinfector" devised (*d*).

(*d*) Mladenovac Disinfecting Station sanctioned and formation commenced. "Railway Van Bath" devised (*e*).

(*e*) Number of cases in hospitals at their maximum twelve days after (*c*); (= incubation period of cases infected before March 17).

(*f*) Railway traffic resumed and leave from army permitted. Inoculation and Disinfecting Train completed.

(*g*) Steady fall in number of cases since April 3: Typhus, 27 per cent.; relapsing, 52 per cent.

(*h*) Recrudescence of typhus twelve days after (*f*), (= incubation period).

(*i*) Mladenovac Disinfecting Station developed to meet this recrudescence.

(*l*) In full action (1,700 men in one day).

(*m*) Two base hospitals transferred to Mladenovac.

(*n*) Mladenovac as a great disinfecting station with two base hospitals connected with it.

These observations and conclusions pointed to the most urgent chief preventive measures called for by the occasion:—

(1) The temporary suspension of all railway passenger traffic, and, above all, in the case of the army, the stoppage of all leave.

(2) The formation of some quarantine and disinfecting station on the railway close behind the chief armies, that would serve to check the spread of infection between the army and the civil areas.

Such then were the measures and the scheme of prevention which we deemed applicable to the situation, and which we submitted in our "Scheme of Work" sent in to G.H.Q. two days later (March 10).

The measures of suspension of railway traffic and stoppage of leave —not a light decision in the condition of public alarm throughout the country—were decreed to take effect on Tuesday, March 16, and were put in force on that date, the duration to be for fifteen days—i.e., until March 31. As will presently be seen, the end of the month was a very critical one as regards the course of the epidemic—calling, in our opinion, for a continuance of these measures. They were absolutely necessary, not only in the state of the epidemics at that time, but also to give time for the formation and completion of our disinfecting station at Mladenovac and the English sanitary train for inoculation and disinfection. At a critical moment these measures were therefore continued for another fifteen days until April 15 (*see* p. 140).

The results, as will afterwards be seen, were most striking—viz., sudden arrest of the epidemics on March 30. The results of the resumption of traffic on April 16 were no less striking, and even more instructive as regards the effect of railway traffic in spreading the

disease. For a sharp rise and spread of the disease followed on April 27, and but for the establishment of our cleansing station at Mladenovac the original conditions favouring the spread might have been largely reproduced. The nature of this recurrence and spread of the disease will be dealt with in a later section.

Section VI.—Summary of Nature, Object and Results of Chief Measures of Prevention.

(*a*) Suspension of railway traffic.
(*b*) Stoppage of leave from the army.
(*c*) Mladenovac quarantine and cleansing station.
(*d*) Formation of "English sanitary disinfecting train."
(*e*) "Barrel disinfector" devised.
(*f*) Railway van disinfectors devised.
(*g*) Railway van douche baths devised.
(*h*) Cleansing of railway stations and rolling stock, disinfecting and bathing stations.

The first wide measures of prevention placed in the foreground of our programme submitted March 10, and put in force at once, March 16, were suspension of railway traffic and stoppage of leave from the army. Their object was to check the spread of infection by the railway all over the country, particularly from the army areas, A, B, C, D, E, to the middle and southern civilian areas F (*see* Map, p. 31), or from these latter areas back to the armies.

The particular area which we had selected as the sphere of our work (B) was the largest area infected, with, as I afterwards found, its total of 4,166 cases of typhus and relapsing fever on April 3. Its particular importance was that it lay between the first army heavily infected area (A) with its total of 2,264 cases, and the rest of Serbia (*see* Table, pp. 62-65).

The particular object we had in view in the establishing of a big quarantine and disinfecting station at Mladenovac, the junction of the first army area (A) with the army areas (B) was to shut off army area A from B, and to hold up all infection coming from the two great army areas (A and B), and to prevent its spread southwards to areas F, even after the railway traffic was resumed.

If our disinfecting station and sanitary disinfecting train could not be formed and got into action before the railway traffic was resumed,

the position of affairs would be largely the *status quo antea*—viz., infection would once more spread southwards. Hence our great anxiety to get the station in hand, and the regret to us occasioned by the delay of two or three weeks from March 10 to 27 in getting the formation started.

The sequel was to show that the events above anticipated were precisely those that happened. It was a neck to neck race between the completion of our disinfecting station at Mladenovac on April 21 and the resumption of railway traffic on April 16. The latter won by five days on the start, and owing to difficulties met with by my officers at Mladenovac—which were only overcome by my arrival at that place on May 1, as will be afterwards described—it may be said to have got the advantage of another seven days on the course up to May 3. But it was overtaken by a great spurt which the disinfecting station was able to make on May 3, two days after my arrival, with the result that the Mladenovac disinfecting station came fully into action just at the time (May 2-6) it was urgently needed, and most effectively arrested or controlled the spread of infection which had again set in after the resumption of railway traffic on April 16. But for the existence of this station the original conditions would have been reproduced—i.e., infection would have passed unchecked from the armies to the civilian areas.

The data furnished by my analyses (Tables, pp. 62-65) and represented graphically in the accompanying Charts, pp. 66, 67, show that, immediately following the resumption of railway traffic two streams of infection began to flow—(*a*) one of typhus conveyed from the army areas, especially army area A, to other areas—principally B and civilian areas F, by troops going and returning on leave; (*b*) one of relapsing fever conveyed from the civilian areas into the army areas—especially area A—by refugees returning north to the areas A and B, whence they had been driven on the invasion of the Austrians.

To meet these conditions the troops and refugees were detrained and temporarily detained for disinfection at our station in Mladenovac. The numbers dealt with rose rapidly from 50 up to as many as 1,700 daily. (*See postea*, p. 127.)

TABLE H (CHART, p. 66).

Course of Epidemic of Typhus in Serbia as shown by number of Admissions, Deaths, and Cases in Hospitals, April 14 to May 31, in (1) War areas A, B, C, D, E; (2) Civilian areas, F; (3) All Serbia, S. Showing increased infection conveyed by troops on leave in all areas except Area A, whence the chief infection was conveyed.

Date	War Territories						Civilian	Totals		
	A	B	C	D	E	Total	F	Admissions	Cases in hospital	Deaths
Cases in hospital April 3 (*d*)	1,221	2,789	216	1,013	571	5,810	2,403	—	8,213	—
April 14								155		50
,, 15								289		48
,, 16 (*f*)	Percentage fall in cases in hospital, April 3—26							419		80
								863	7,320	178
,, 17								464		63
,, 18								369		63
,, 19	4·0	42·0	+17·0	23·0	24·0	27·0	28·0	304		57
,, 20								256		80
,, 21								227		53
								1,620	6,600	316
,, 22								242		48
,, 23								228		58
,, 24								236		63
,, 25								217		35
,, 26 (*g*)	89	56	11	21	10	187	41	228	27·0	32
Cases in hospital	1,126	1,692	244	771	431	4,264	1,715	1,151 (*g*)	6,000	236
,, 27 (*h*)	89	114	31	43	7	284	327 (*h*)	611 (*h*)		40
,, 28	38	104	15	29	9	195	39	234		35
,, 29	32	59	19	15	12	137	107	244		54
,, 30	54	417 (*h*)	18	20	13	522 (*h*)	48	570		41
May 1	36	60	20	60	5	181	79	260		51
	249	754 (*h*)	103	167	46	1,319 (*h*)	600 (*h*)	1,919 (*h*)	6,475 (*h*)	221
,, 2	15	24	44	35	18	136	58	194		67
,, 3	48	72	30	23	5	178	82	260		51
,, 4	—	24	9	5	40	78	70	148		37
,, 5	64	32	21	8	8	133	32	165		34
,, 6	—	15	22	83	9	129	22	151		40
	127	167	126	154	80	554	264	918	5,560	235
,, 7	8	54	—	7	1	70	42	112		31
,, 8 (*k*)	18	252 (*k*)	48	15	7	340 (*k*)	28	368 (*k*)		45
,, 9	162 (*k*)	86	22	12	3	285	58	343		56
,, 10	44	44	38	43	4	173	48	221		37
,, 11	—	52	13	6	3	74	153	227		34
	232	488	121	83	18	942 (*k*)	329	1,271 (*k*)	5,140	203

TABLE H (*continued*).

Date	War Territories						Civilian	Totals		
	A	B	C	D	E	Total	F	Admissions	Cases in hospital	Deaths
May 12 ...	—	23	14	4	9	50	42	92		31
,, 13 ...	—	25	19	3	5	52	104	156		31
,, 14 ...	—	89	14	14	4	121	17	138		27
,, 15 ...	—	73	6	4	1	84	65	149		32
,, 16 ...	35	58	9	9	4	115	30	145		44
	35	268	62	34	23	422	258	680	4,530	165
,, 17 ...	9	27	7	12	8	63	52	115		36
,, 18 ...	27	18	7	14	2	68	48	116		31
,, 19 (*l*) ...	30	292 (*l*)	15	1	5	343 (*l*)	24	367 (*l*)		30
,, 20 ...	—	97	1	2	17	117	47	164		16
,, 21 ...	—	54	31	27	26	138	46	184		35
	66	488	61	56	58	729	217	946 (*l*)	3,960	148
,, 22 ...	26	40	25	2	9	102	71	173		19
,, 23 ...	—	34	8	7	2	51	43	94		23
,, 24 (*l*) ...	—	182 (*l*)	1	16	2	201 (*l*)	12	213 (*l*)		38
,, 25 ...	—	36	3	15	49	103	23	126		26
,, 26 ...	18	9	3	19	1	50	23	73		34
	44	301	40	59	63	507	172	679	3,630	140
,, 27 ...	28	78	4	9	5	124	14	138		30
,, 28 ...	—	24	1	5	1	31	24	55		9
,, 29 ...	—	2	3	13	5	23	52	75		21
,, 30 (*l*) ...	7	168 (*l*)	3	7	8	193 (*l*)	19	212 (*l*)		26
,, 31 ...	20	31	5	3	7	66	37	103		20
	55	303	16	37	26	437	146	583	3,090	106
Cases in hospital May 31	502	1,437	129	174	187	2,420	670		3,090 (*m*)	
Total percentage fall since April 3	58·0	47·0	40·0	82·0	67·0	59·0	76·0		63·0	
								10,630		1,948
Percentage distribution—										
April 3	14·0	35·0	2·0	11·0	7·0	72·0	28·0			
,, 26	18.0	28·0	4·0	13·0	7·0	72·0	28·0			
May 31	16·0	46·0	4·0	5·0	6·0	80·0	20·0			

(*d*—*g*) Fall in numbers of cases in hospitals and in admissions during suspension of railway traffic (up to *f*) and for ten days after (*f*); the greatest fall in War area B, least in War area A.

(*h*) Sudden rise in admissions twelve days after resumption of railway traffic on April 16, as the result of infection conveyed from Area A by men going on leave—shown in all areas *except Area A*.

(*k*) Second rise twelve days later, twelve days after return of men on leave to the armies. Shown in all army areas *including Area A*. (*See* Charts, pp. 66, 67.)

(*l*) Rise in admissions seen in all War areas, *not in Civilian areas F*, specially in War area B, due to evacuation of field ambulances in that area, preparatory to forward movement of troops. (*See* Charts, pp. 96, 97, 98, 101.)

(*m*) Patients in hospital mostly convalescent.

TABLE K (CHART, p. 67).

Course of Epidemic of Relapsing Fever in Serbia as shown by numbers of Admissions and Deaths in hospitals (April 14 to May 31) in (1) War areas A, B, C, D, E; (2) Civilian, F; (3) All Serbia, S. Showing increased infection conveyed by Refugees back to War area A and Civilian areas F, after resumption of railway traffic on April 16—21; most marked in War area A and Civilian areas F.

Date	War Territories.						Civilian	Totals		
	A	B	C	D	E	Total	F	Admissions	Cases in hospitals	Deaths
Cases in hospital April 3 (*d*)	1,043	1,377	407	416	237	3,480	4,330	—	7,810	—
April 14 ...								117		49
,, 15 ...								253		16
,, 16 ...	Percentage fall in cases in hospital, April 3—26							268		16
								638	4,960	81
,, 17 ...								284		26
,, 18 ...								354		13
,, 19 ...								197		11
,, 20 ...								149		12
,, 21 ...								275		12
	71·0	30·0	+ 27·0	13·0	57·0	36·0	65·0	1,259	4,250	74
,, 22 ...								217		12
,, 23 ...								206		9
,, 24 ...								304		14
,, 25 ...								161		3
,, 26 ...	—	8	3	14	14	39	50	89	52·0	13
Cases in hospital (*g*)	305	965	520	361	102	2,253	1,547	977	3,800	51
April 27 ...	150 (*h*)	6	20	—	—	176 (*h*)	128 (*h*)	304 (*h*)		11
,, 28 ...	30	11	5	12	—	58	68	126		9
,, 29 ...	34	47	6	1	4	92	41	133		4
,, 30 ...	31	80 (*h*)	1	1	1	114	117 (*h*)	231		8
May 1 ...	25	51	18	—	2	96	49	145		15
(*h*)	270	195	50	14	7	536	403	939	3,260	47
,, 2 ...	42	4	7	18	1	72	72	144		10
,, 3 ...	28	23	26	9	4	90	63	153		8
,, 4 ...	24	28	7	22	4	85	39	124		9
,, 5 ...	27	24	10	15	11	87	27	114		9
,, 6 ...	14	21	10	2	21	48	128 (*h*)	176		8
(*h*)	135	100	60	66	21	382	329	711	2,940	44
,, 7 ...	34	30	15	13	1	93	76	169		12
,, 8 ...	8	8	17	3	5	41	58	99		11
,, 9 ...	26	25	7	10	21	89	93	182		11
,, 10 ...	16	10	10	5	17	58	26	84		10
,, 11 ...	34	13	10	4	1	62	149 (*k*)	201		10
(*k*)	118	86	59	35	45	343	392	735	2,340	54

TABLE K—(*continued*).

Date	War Territories						Civilian	Totals		
	A	B	C	D	E	Total	F	Admissions	Cases in hospitals	Deaths
May 12 ...	11	4	6	2	1	24	30	54		11
,, 13 ...	—	9	21	15	1	46	57	103		4
,, 14 ...	—	11	8	9	1	23	63	76		2
,, 15 ...	—	2	5	9	2	18	193 (*k*)	211 (*k*)		15
,, 16 ...	17	11	11	7	2	48	24	72		9
	28	37	45	42	7	159	357	516	2,150	41
,, 17 ...	9	4	7	6	—	26	36	62		4
,, 18 ...	3	11	6	2	—	22	24	46		3
,, 19 ...	55 (*l*)	14	4	7	1	81	49	130		6
,, 20 ...	—	18	—	29	6	53	20	73		3
,, 21 ...	—	20	12	1	—	33	64	97		3
	67	67	29	45	7	215	193	408	1,870	19
,, 22 ...	—	4	15	3	1	23	75	98		4
,, 23 ...	—	25	2	2	6	35	32	67		4
,, 24 ...	125 (*l*)	16	2	7	—	150 (*l*)	54	204		9
,, 25 ...	—	16	5	8	—	29	14	43		9
,, 26 ...	5	13	2	13	—	33	6	39		4
	130	74	26	33	7	270	181	451	1,510	30
,, 27 ...	25	9	3	4	9	50	50	100		3
,, 28 ...	—	22	1	1	1	25	50	75		—
,, 29 ...	78	6	—	5	—	89	27	116		5
,, 30 ...	17	191 (*l*)	5	5	—	218 (*l*)	68	286		1
,, 31 ...	9	54	1	4	—	68	22	96		3
	129	282	10	19	10	450	223	673	1,640	12
Cases in hospital June 4	308	480	210	68	22	1,088	522	—	1,610	
Total percentage fall since April 3	71·0	65·0	48·0	84·0	90·0	69·0	88·0	—	80·0	
							Total ...	7,327		463
Percentage distribution—										
April 3	13·0	17 0	5·0	5·0	3·0	44·0	56·0	—		
,, 26	8·0	25·0	13·0	9·0	2·0	60·0	40·0	—		
June 4	19·0	30·0	13·0	4·0	1·0	67·0	33·0	—		

(*d—g*) Fall in numbers of cases in hospitals during suspension of railway traffic (up to *f*), and for ten days after its resumption (*g*) : this fall greatest in area A.

(*h*) Sudden rise in admissions twelve days after (*f*) ; seen in all areas, but most marked in Army area A, Civilian areas F, and continuing for ten days.

(*k*) Increase continued, but most marked in Civilian areas F.

(*l*) Rises in War areas A and B, due probably to return of civilian refugees—absent in civilian areas F.

Chart 12.

Typhus shewing Increased Admissions (h) in different Areas (Except Area A) on April 27-May 1, twelve days after resumption of traffic on April 16, conveyed by Troops going on leave (h), also on returning (k) including War Area A.

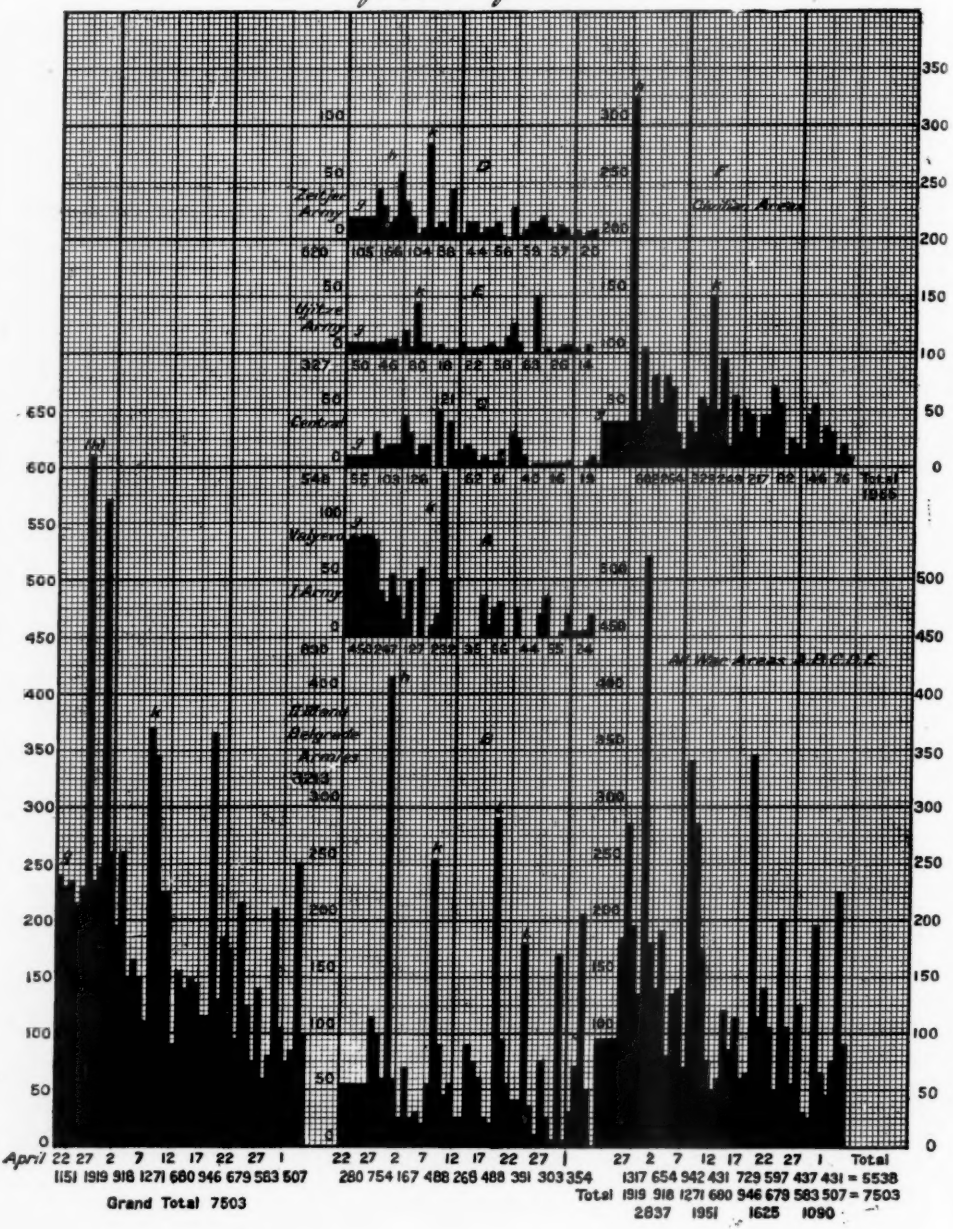
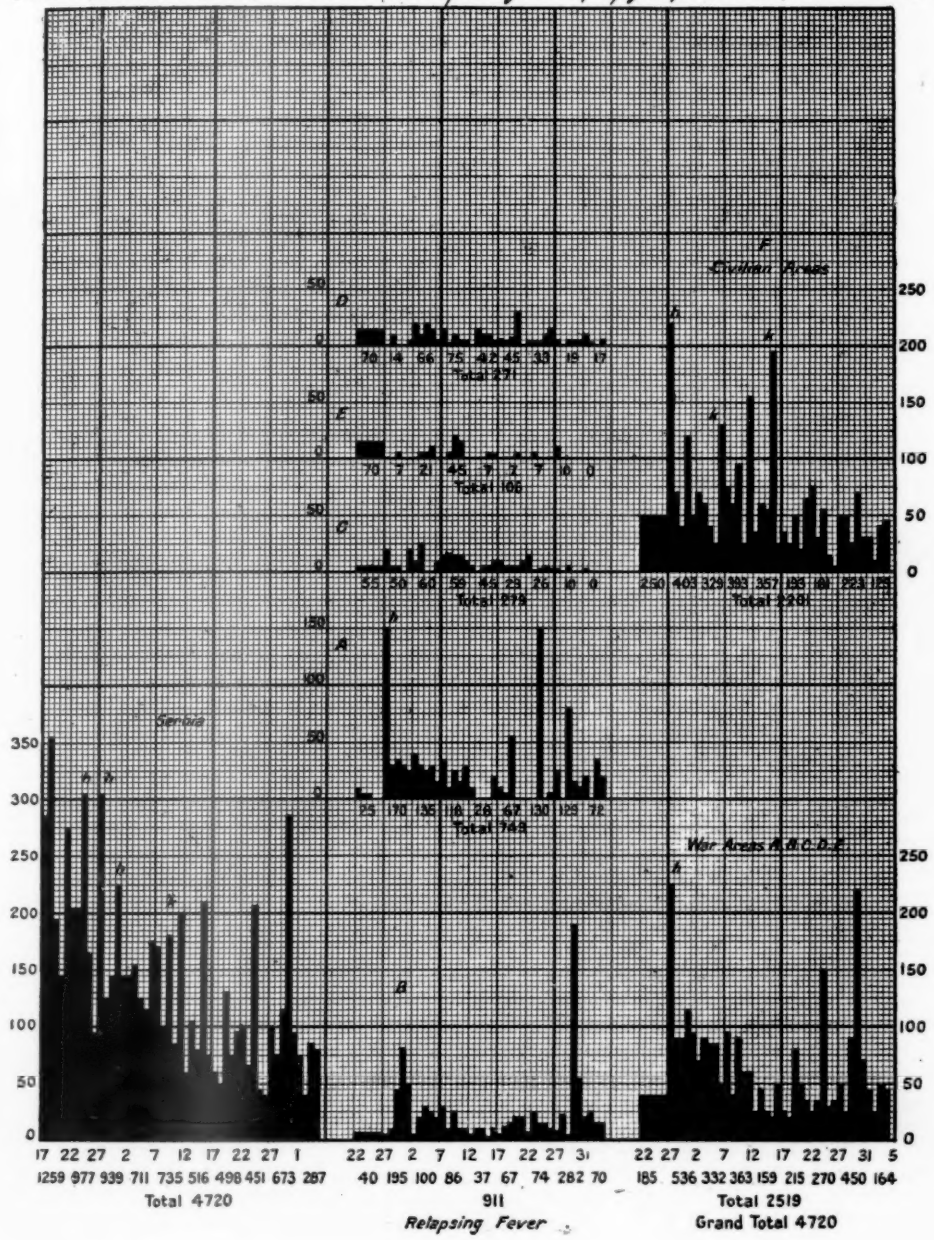


Chart 13. Increased Admissions in War Area A, also in B following return of Refugees April 16-21. also in Civilian Areas F.



Section VII.—Comparison of Course of Serbian and Other Great Typhus Epidemics.

Summary—(Charts, pp. 73, 74).

The break of the epidemic thus occurred suddenly on March 16, following the chief great administrative measures recommended and immediately put in force within ten days of our arrival in Serbia. The effect of them was immediate—and lasted four weeks—viz., for the month of actual suspension of traffic.

The effect of resumption was equally immediate, and from point of view of ætiology even more instructive, since its details could be subsequently clearly analysed. That effect was a sharp recrudescence of infection, which set in exactly ten to twelve days after the resumption; was definitely related to movements of troops and civilians, and occurred in areas and places chiefly affected by such movements.

The whole experience pointed to the importance of such definite administration measures, affecting the country as a whole, in arresting epidemics of this kind, in addition to and far more effective than the usual measures adopted locally in individual towns or hospitals for the prevention of the spread of these diseases.

The effect was out of all proportion far greater than the measures of this latter kind. Both have the same object, viz., to prevent spread of lice. But while in the latter, the object aimed at is to limit the movement or spread of infected lice in hospitals to other patients or other people outside, the object aimed at by the general administrative measures we urged in Serbia, was to limit or *prevent the movement of people*, both of the healthy and of potential carriers of infection, from one area of infection to another.

The effect of the measures adopted in suddenly arresting the epidemic, may be judged by the data of the largest fever hospital in Serbia, in Kragujevatz. The number of cases of typhus admitted into this hospital in January had been 118, in February 961, and in March 1,138; the death-rate was 30 per cent. in January and February, rising to 40 per cent. in March. (The ten day figures for February were 252, 296 and 413 cases; for March 380, 392 and 366 cases.) In April this total of 1,138 fell suddenly to 360—the ten day figures being 184, 103 and 73; and during May it fell to about 200, the figures for the first ten days being 72 cases. The daily admissions which had averaged 4 in January, 32 in February and 38 in March fell suddenly to 12 in

April and 7 in May. The number of deaths which had been 22 in January, 185 in February, and 331 in March, fell to 131 in April and 26 for the first ten days in May. (Charts, pp. 71, 72; Tables, pp. 70, 80.)

Allowing twelve days for incubation of the disease, the number of these cases contracting infection in forty days before March 18 was 1,551; the number contracting it in forty days after was 432 (*see* Table, p. 70).

Comparison of Serbian Typhus Figures with those of previous Epidemics.

This comparison is interesting, and can now be made as seen in its perspective (Charts, pp. 73 and 74).

(1) *Irish Epidemic of* 1846-48. — The best known of previous epidemics were those of the Irish famine, which, starting in 1846, went on to 1848, a period of nearly two years; the maximum lasting continuously for a year, 1847. This epidemic was one of typhus and relapsing combined, the relapsing preponderating at the outbreak of the epidemic, the typhus at the end, precisely as was the case in the Serbian epidemics. The number of cases admitted into the Glasgow Royal Infirmary in 1847 were typhus 2,399, relapsing 2,333.

(2) *English Epidemic of* 1863-65. — The second great epidemic of typhus was that which occurred in England in 1863-65, during which 5,752 cases were admitted into the London Fever Hospital. (Charts, pp. 73, 74.) Of these, 2,493 cases were admitted in 1864, the highest figure of admission of typhus into any hospital between 1848 and 1870.

(3) In the *Serbian Epidemic* of 1915, the epidemic was one of relapsing and typhus combined, breaking out in December, 1914. The first in origin was relapsing, the numbers of cases in hospitals at the end of December being 2,184, as compared with only 200 cases of typhus (but the figures at this period were very incomplete—especially those for typhus). By the end of January the numbers were 5,122 and 1,100 respectively; end of February, 7,000 and 4,000 respectively; by the end of March, 8,100 and 8,000 respectively. Thereafter the proportions altered. At the end of April, they were 3,260 and 6,300 respectively; and by the end of May 1,600 and 2,860 respectively. During this period, the number of cases of typhus admitted (January 1 to May 14), into the largest fever hospital in Serbia, in Kragujevatz, with which I was in close touch, was 2,686. (*See* Chart, p. 71.)

TABLE C.

Course of Epidemic of Typhus as shown by number of Admissions and Deaths in 1st Reserve Hospital, Kragujevatz, January 1 to May 11, 1915 (B); *and Number of Cases in Serbian Hospitals* (A).

(A) ALL SERBIA Number of cases in hospitals on date specified			(B) 1ST RESERVE HOSPITAL, KRAGUJEVATZ				(C) Infection contracted (12 days' incubation)		
			Admissions		Deaths				
Relapsing fever	Typhus		5 days	10 days	5 days	10 days	5 days ending		
2,184	200	January 5 ...	15	34	—	3	December 23 ...		
3,153	243	,, 11 ...	21		3		,, 28 ...		34
3,532	378	,, 16 ...	12	29	3	6	January 2 ...		
4,586	622	,, 21 ...	17		3		,, 7 ...		29
5,233	730	,, 26 ...	22	55	3	12	,, 12 ...		
5,122	1,107	,, 31 ...	33		9		,, 17 ...		55
		Total (30 days)	...	118	...	21	...	...	118
5,458	1,256	February 5 ...	123	252	18	34	January 22 ...		
6,347	1,557	,, 10 ...	129		16		,, 27 ...		252
6,266	2,336	,, 15 ...	122	296	16	48	February 1 ...		
6,450	2,587	,, 20 ...	174		32		,, 6 ...		296
7,072	3,125	,, 25 ...	209	413	36	103	,, 11 ...		
7,000	3,964	March 2 (*a*)	204		67		,, 16 ...		413
		Total (30 days)	...	961	...	185	...	...	961
7,874	4,569	March 7 (*b*)	197	380	50	119	February 21 ...		
7,740	5,293	,, 12 ...	183		69		,, 26 ...		380
7,900	5,997	,, 17 (*c*)	255	392	61	124	March 3 ...		
7,700	6,969	,, 22 ...	137		63		,, 8 ...		392
8,681	8,040	,, 27 (*d*)	135	366	60	88	,, 13 ...		
8,114	7,952	April 1 ...	231		28		,, 18 (*c*)		366
		Total (30 days)	...	1,138	...	331	...	...	1,138
7,550	8,245	April 6 ...	78	184	31	63	March 23 ...		
6,780	7,984	,, 11 ...	106		32		,, 28 ...		184
4,960	7,324	,, 16 (*f*)	61	103	28	45	April 2 ...		
4,253	6,604	,, 21 ...	42		17		,, 7 ...		103
3,800	5,980	,, 26 ...	35	73	11	23	,, 12 ...		
3,265	6,475	May 1 (*h*)	38		12		,, 17 (*f*)		73
		Total (30 days)	...	360	...	131	...	...	360
2,943	5,600	May 6 ...	16	72	12	26	April 22 ...		
2,400	5,138	,, 11 ...	56		14		,, 27 ...		72
2,150	4,530	,, 16							
1,870	3,957	,, 21							
1,516	3,635	,, 26							
1,642	3,089	,, 31							
1,600	2,860	June 4							

(*a*) Arrival of Mission.
(*b*) Programme of Prevention submitted.
(*c*) Suspension of railway traffic and leave from army stopped.
(*d*) Maximum numbers in hospitals.
(*f*) Resumption of railway traffic and leave from army permitted.
(*h*) Recrudescence of infection ten days after (*f*).

C = Number of cases contracting infection forty days prior to March 18 = 1,551.
,, ,, ,, ,, ,, after March 18 = 432.

Chart 10

Course of Epidemic of Typhus in Serbia as shewn by Number of Daily Admissions and Deaths, in 1st Reserve Hospital, Kragujevatz Jan. 1–May 13, 1915.

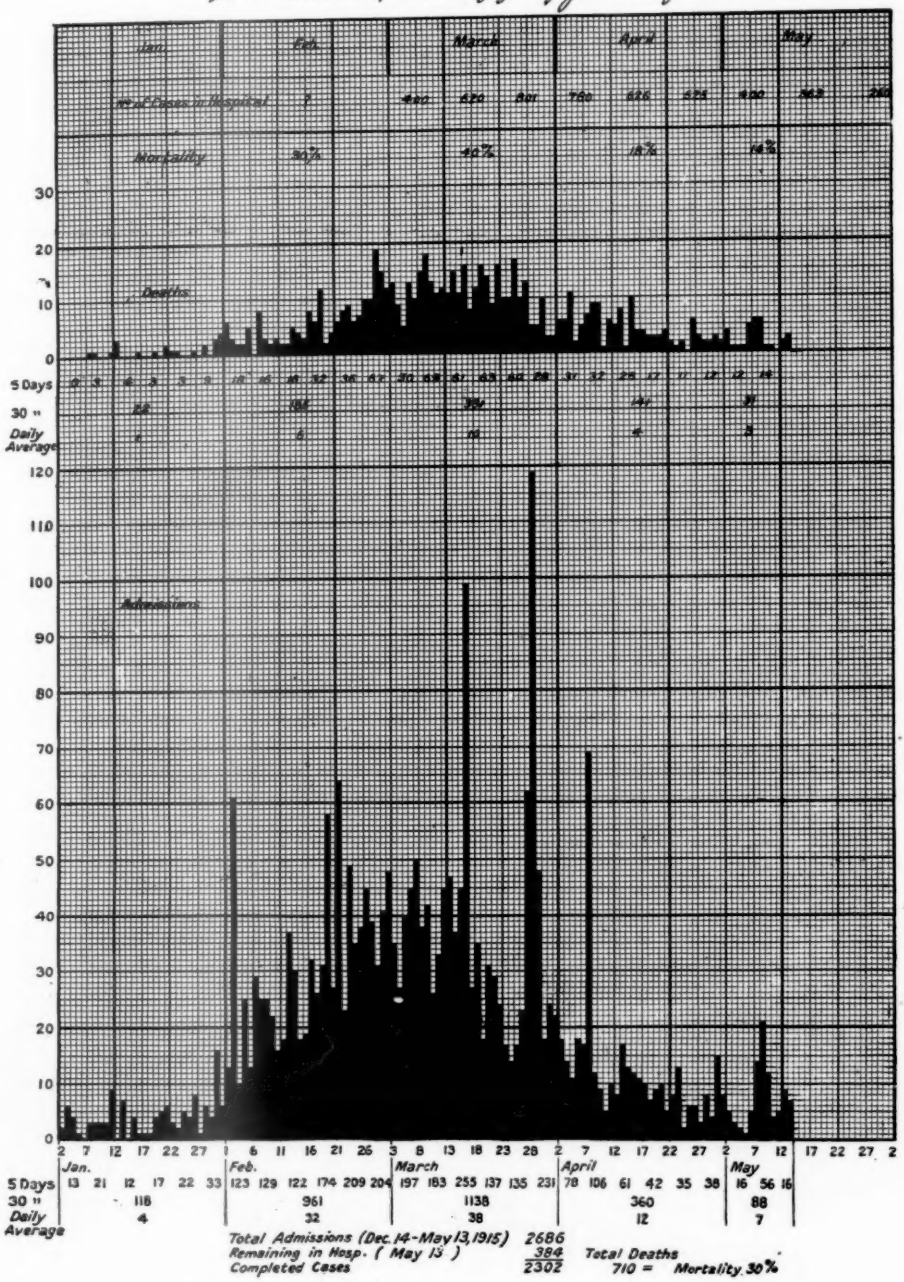
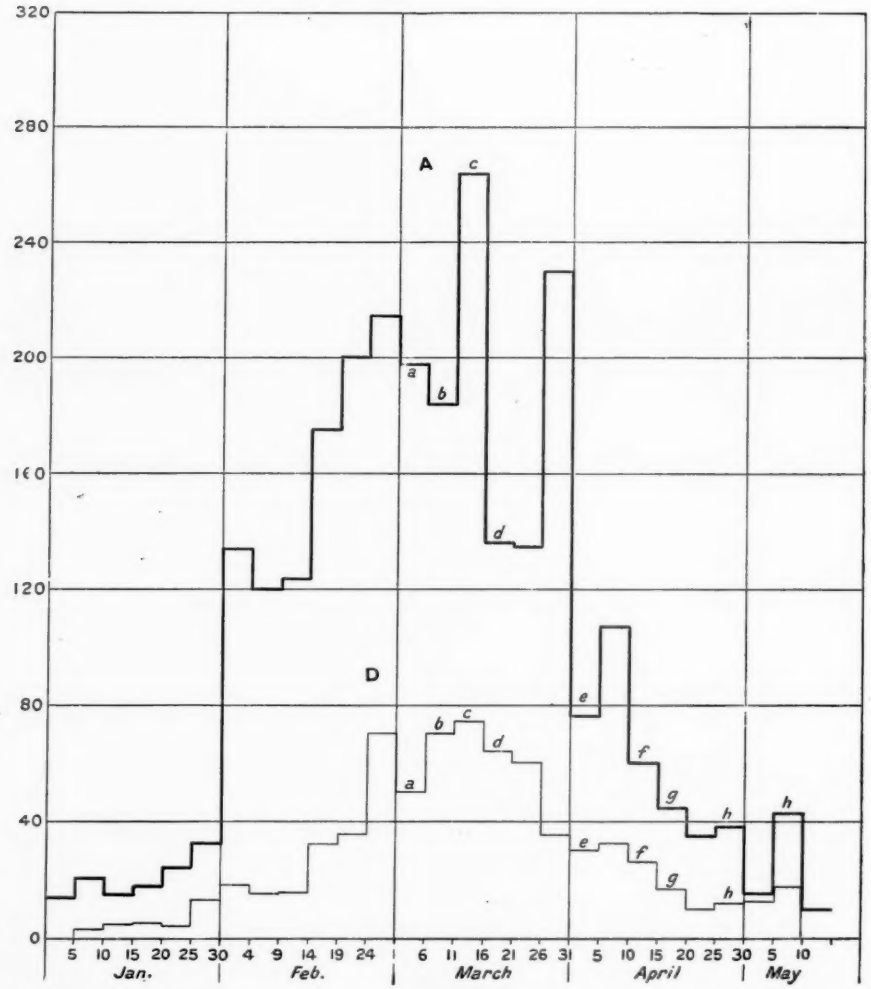


CHART 10 (*a*).

Course of Typhus Epidemic in Serbia in 1915, *as shown by Admissions and Deaths in one large Hospital.*



Number of cases in First Reserve Hospital in Kragujevatz, January to May. (Five-day totals.)

a, March 5, arrival of Mission; *b*, barrel disinfector put into use; *c*, suspension of railway traffic (March 15); *f*, resumption of railway traffic (April 15); *h*, arrest in fall, April 20-25 and May 5 (ten to fourteen days after *f*).

This group of hospitals served especially the active armies in the field, and included the largest typhus hospital in Serbia (the First Reserve). Their importance as a whole may be judged from the circumstance that at the height of the epidemic at the end of March they contained 1,000 cases of typhus (850 of them in the First Reserve Hospital), or 12 per cent. of the total cases in all the hospitals in Serbia, in addition to 500 cases of relapsing fever, or nearly 6 per cent. The chart shows the striking fall which occurred after March 30. (*See* text, p. 68.)

Chart 14.

Shewing Relative Magnitude, Course, & Duration of Three Historical Epidemics of Typhus, as shewn by Admissions into three Chief Hospitals.

1. Irish Epidemic 1846-48.
2. English " 1863-65.
3. Serbian " 1915.

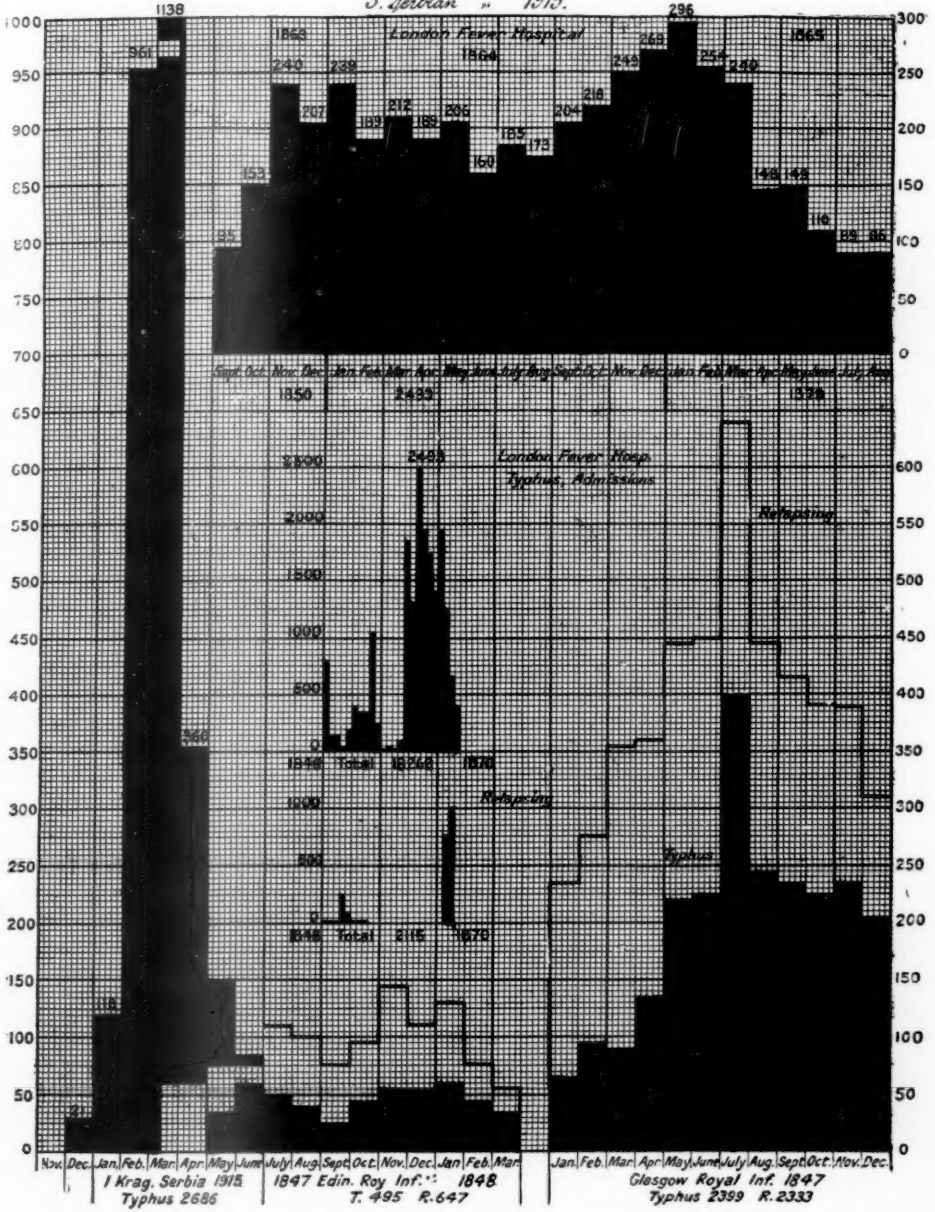
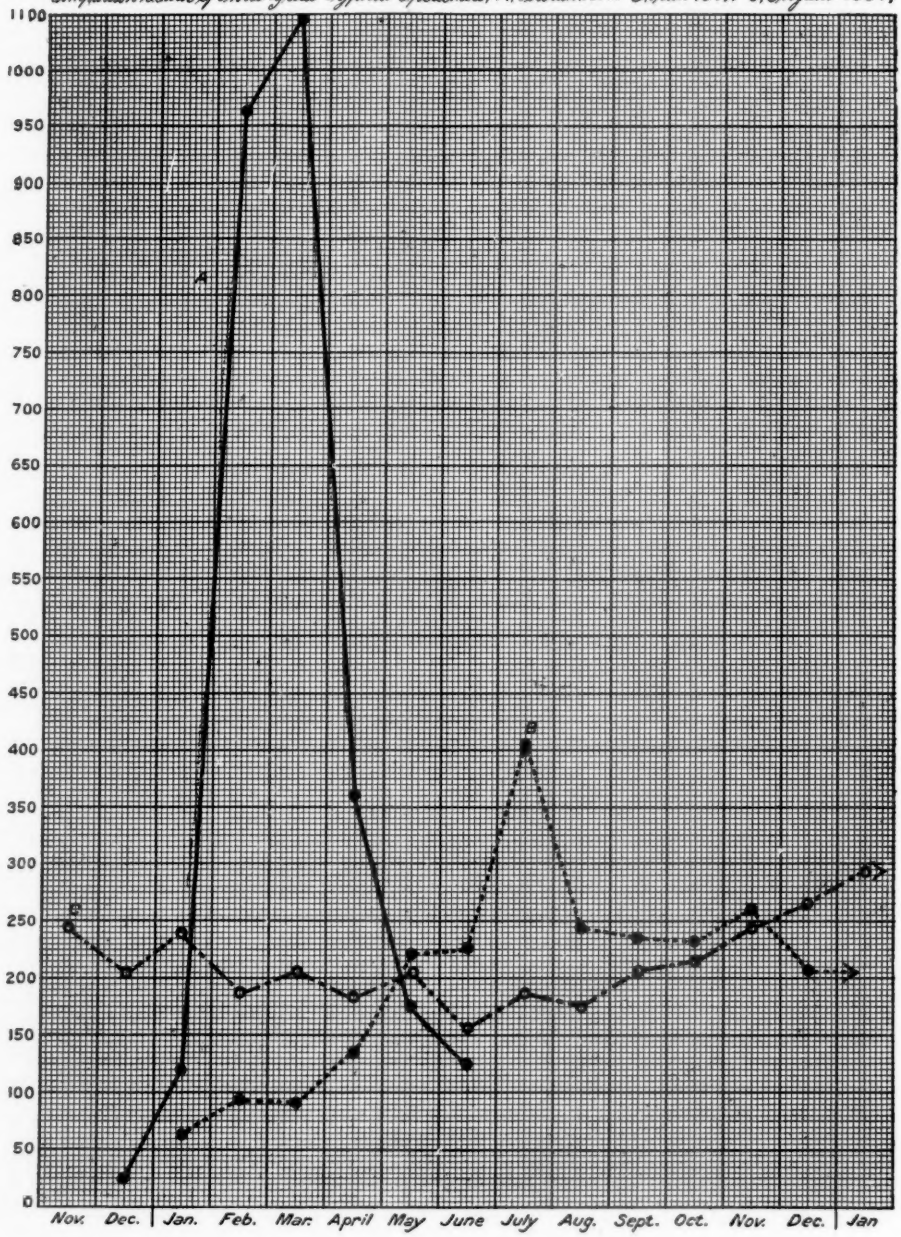


Chart 15.

Comparison Course of Three Great Typhus Epidemics, A. Serbian 1915. B. Irish 1847. C. English 1864.



The figures of typhus for comparison in the three great epidemics referred to are those of three chief representative hospitals—viz. :

Epidemic	Hospital	Number of cases of typhus	Duration of epidemic
Irish (1847) ...	Glasgow Royal Infirmary ...	2,399 ...	Two years
English (1863-5)...	London Fever Hospital ...	5,752 ...	Three years
Serbia (1915) ...	Kragujevatz Fever Hospital ...	2,686 ...	Six months

The monthly figures of admission in the three hospitals were :—

	Glasgow Royal (1847)	London Fever (1864)	Serbia (1915)
December ...	— ...	(207) ...	(18)
January ...	66 ...	239 ...	118
February ...	93 ...	189 ...	961
March	88 ...	212 ...	1,138
April	135 ...	189 ...	360
May	218 ...	206 ...	72
June	226 ...	160 ...	—
July	400 ...	185 ...	—
August	246 ...	173 ...	—
September ...	235 ...	204 ...	—
October	227 ...	218 ...	—
November ...	262 ...	249 ...	—
December ...	203 ...	269 ...	—
	2,399	2,493	2,649

The Serbian epidemic was thus the most sudden in origin, most rapid in rise, greatest in extent, and most quickly arrested of any epidemic of a like kind in history. The curve of its origin, rise and decline, differs in character from that of any previous epidemic. It lasted from start to finish about a period of six months, and maintained its maximum for only two months. Other epidemics have lasted one and a half to three years, and maintained their maximum for periods of twelve (Irish) or eighteen months (English epidemic).

The interest of this result is that it was the first great epidemic, which had arisen and been dealt with, since the lice-borne nature of typhus and relapsing fever had been established a few years previously by the researches of Nicolle, in 1911. The suddenness of its arrest —dating from a particular day—was directly related to measures for *stopping movement of people ;* its recurrence dating from a particular day was directly related to the resumption of such movements. The two events together—and all the circumstances connected with them—were too alike and too striking to be mere coincidences.

APPENDIX.

TABLE II (CHARTS, pp. 44, 45, 95, 102).

Course of Epidemics of Typhus, Relapsing, Enteric Fevers in Serbia, March 26 to June 4, as shown by numbers of cases in Hospitals and Admissions, Deaths, and Discharges (April 14 to June 4).

	TYPHUS				RELAPSING				ENTERIC			
Date	No. in Hospital	New Cases	Deaths	Discharges	No. in Hospital	New Cases	Deaths	Discharges	No. in Hospital	New Cases	Deaths	Discharges
Mar. 27	8040 *(d)*				8661*(d)*				1856			
28	7980				8504				1865			
29	7974				8532				—			
30	7814				8520				1673			
31	8085				8220				1648			
		Mortality	40%									
Apr. 1	7952				8114				1641			
(e) 2	8176				7983				1548			
3	8213				7810				1549			
4	8199				7675				1457			
5	8198				7503				1443			
6	8124				7550				1527			
7	8245				7348				1635			
8	7971				7177				1487			
9	8009				7065				1464			
10	8077				6993				1352			
11	7984				6791				1295			
12	7991				6548				1230			
13	7867				6251				1219			
14	7530	155	50	289	5867	117	49	313	1112	27	13	34
15	7300	289	48	471	5663	253	16	437	1075	71	10	95
(f) 16	7324	419	80	316	4960	268	16	655	1100	67	14	28
17	7219	464	63	506	4748	284	26	470	1115	12	9	29
18	6984	369	63	516	4861	354	13	528	1126	135	8	116
19	6947	304	57	284	4451	197	11	596	1160	54	3	17
20	6779	256	80	364	4230	149	12	348	1120	12	6	46
21	6604	227	53	357	4253	275	12	237	1114	42	7	41
22	6550	242	48	243	3930	217	12	528	1041	40	11	102
23	6311	228	58	408	3918	206	9	209	979	43	9	96
24	6209	236	63	289	4039	304*(h)*	14	169	985	51	11	37
25	6054	217	35	257	3967	161	3	230	937	82	4	126
(g) 26	5979*(g)*	228*(g)*	32	271	3808	89	13	225	858	26	14	91
(h) 27	6314*(h)*	611*(h)*	40	235	3904	304*(h)*	11	197	878	55	9	26
28	6249	234	35	264	3815	126	9	206	849	22	11	40
29	6158	244	54	281	3450	133	4	494	846	26	8	21
30	6475	570*(h)*	41	212	3463	231*(h)*	8	210	882	112	9	67

TABLE II (*continued*).

Date	Typhus				Relapsing				Enteric			
	No. in Hospital	New Cases	Deaths	Discharges	No. in Hospital	New Cases	Deaths	Discharges	No. in Hospital	New Cases	Deaths	Discharges
May 1	6343	260	51	336	3265	145	15	328	865	32	6	43
2	6260	194	67	216	3250	144	10	149	837	34	6	56
3	6263	260	57	191	3166	153	8	229	797	17	10	47
4	6107	148	37	267	3110	124	9	171	751	11	7	50
(*i*) 5	5938	165	34	300	2941	114	9	274	722	20	14	35
6	5598	151	40	451	2943	176(*k*)	8	166	694	20	7	41
7	5420	112	31	259	2955	169	12	145	613	28	3	106
8	5488	368(*k*)	45	255	2834	99	11	209	600	49	7	55
9	5416	343(*k*)	56	359	2767	182(*k*)	11	238	581	47	4	62
10	5315	221	37	285	2472	84	10	369	533	36	6	78
(*k*) 11	5133	227(*k*)	34	370	2398	201(*k*)	10	265	505	30	6	52
12	4941	92	31	258	2251	54	11	190	540	64	2	26
13	4881	156	31	185	2220	103	4	130	462	3	2	79
14	4843	138	27	149	2186	76	2	108	438	5	2	27
15	4564	149	32	396	2197	211(*k*)	15	185	391	18	1	64
16	4529	145	44	136	2149	72	9	111	381	7	2	15
17	4376	115	36	232	2120	62	4	87	341	18	4	54
18	4064	116	31	417	2072	46	3	91	299	6	2	46
(*l*) 19	4104	367(*l*)	30	297	1965	130(*l*)	6	221	280	12	2	29
20	4043	164	16	210	1862	73	3	173	257	3	1	25
21	3957	184	35	235	1870	97	3	86	221	10	—	46
22	3887	173	19	224	1685	98	4	279	242	38	1	16
23	3770	94	23	187	1566	67	4	18	216	8	2	32
24	3784	213(*l*)	38	152	1673	204(*l*)	9	88	203	5	2	16
(*m*) 25	3780	126	26	104	1609	43	9	98	205	11	2	7
26	3635	73	34	194	1516	39	4	128	198	15	2	20
27	3583	138	30	100	1535	100	3	78	206	23	3	12
28	3434	55	9	195	1558	75	—	52	196	8	1	17
29	3334	75	25	175	1558	116	5	111	184	12	—	24
30	3266	212(*l*)	26	233	1675	286(*l*)	1	148	167	17	—	34
31	3089	103	20	251	1642	96	3	126	188	35	2	12
June 1	2968	76	11	196	1578	77	2	139	316	155	1	22
2	2960	85	15	88	1581	37	—	34	311	27	—	32
3	2996	248	19	183	1621	86	2	44	164	9	2	154
4	2860	98	21	213	1609	87	4	95	160	12	—	16

(*d*) No. of cases in hospital at their maximum ten days after suspension of traffic (*c*) on March 16.
Railway Van Disinfector devised.
(*e*) Inoculation and Disinfecting Train ordered and commenced.
"Van Douche Bath" devised.
(*f*) Railway Traffic resumed.
(*g*) Continued fall in Cases for 10 days after (*f*).
(*h*) Sudden rise in Number of Cases 11 days after (*f*).
(*i*) Work of Quarantine Station greatly developed to check spread of Infection to South.
(*k*) Second spread of Infection.
Spread of Infection checked.
(*l*) Increase of cases in Hospitals due to evacuation of Field Ambulances in Northern Armies.
(*m*) Serbian Base Hospital removed to Mladenovac.
Scottish Mission Fever Hospital removed to Mladenovac (Dr. Elsie Inglis).
Order for Mission to proceed to Malta.

TABLE III (CHARTS, pp. 96, 97, 98, 101).

Course of Epidemics of Typhus in Serbia in 1915, *as shown by numbers of Cases in Hospitals.* S, *all Serbia;* A, B, C, D, E, *Army areas;* F, *Civilian areas respectively from April* 3 *to May* 31.

Date	S All Serbia		A 1st Army		B 2nd, 3rd and Belgrade Armies		C Railway Towns		D Zeitjer Army		E Ujitze Army		F Civilian Areas.	
	No. in Hosp.	New Cases	No. in Hosp.	New Cases	No. in Hosp.	New Cases	No. in Hosp.	New Cases	No. in Hosp.	New Cases	No. in Hosp.	New Cases	No. in Hosp.	New Cases
Apr. 3	8213		1221		2789		216		1013		571		2403	
8	7971													
13	7867													
14	7530	155												
15	7300	289												
(*f*) 16	7324	419												
17	7219	464												
18	6984	369												
19	6947	304												
20	6779	256												
21	6604	227												
22	6550	242												
23	6311	228	—		—		—		—		—		—	
24	6209	236	—		—		—		—		—		—	
25	6054	217	1075		1747		261		804		458		1709	
(*g*) 26	5979	224	1126	89	1692	56	244	11	771	21	431	10	1715	41
	Fall 29%		Fall 4%		Fall 42%		Rise 17%		Fall 23%		Fall 24%		Fall 28%	
27	6314	611(*h*)	1128	89	1710	114(*h*)	255	31(*h*)	813	43(*h*)	436	7	1972	327(*h*)
28	6249	234	1134	38	1757	104(*h*)	265	15	735	29	425	9	1933	39
29	6158	244	1079	32	1779	59	250	19	719	15	407	12	1924	107(*h*)
30	6475	570(*h*)	1098	54	2096	417(*h*)	240	18	713	20	405	13	1923	48
May 1	6343	260	1078	36	2058	60	227	20	656	60(*h*)	387	5	1937	79(*h*)
2	6260	194	1071	15	2280	24	251	44(*h*)	674	35	392	18	1592	55
3	6263	260	1069	48	2033	72	258	30	684	23	381	5	1838	81(*h*)
4	6107	148	987	—	1921	24	266	9	667	5	365	40	1871	68
5	5938	165	971	64	1949	32	263	21	647	8	374	8	1734	32
6	5598	151	776	—	1830	15	261	22	669	83	396	9	1666	13

7	5420	112	763	8	1817	54	228	—	615	7	329	1	1669	42
8	3488	368(*k*)	745	18	1935	252(*k*)	266	48(*k*)	614	15	324	7	1604	28
9	5416	343(*k*)	858	162(*k*)	1954	86	277	22	492	12	310	3	1525	58
10	5315	221	891	44	1871	44	306	38	506	43	299	4	1442	48
11	5138	227	776	—	1807	52	294	13	568	6	323	3	1370	153(*h*)
12	4941	92	758	—	1780	23	293	14	396	4	284	9	1430	42
13	4881	156	759	—	1803	25	292	19	381	3	282	5	1364	104(*k*)
14	4343	138	754	—	1743	89(*k*)	291	14	379	14	285	4	1391	17
15	4564	149	650	—	1774	73	271	6	314	4	256	1	1299	65
16	4529	145	775	35	1771	58	271	9	303	9	254	4	1155	30
17	4376	115	671	9	1660	27	267	7	294	12	258	8	1226	52
18	4064	116	626	27	1444	18	259	7	290	14	233	2	1212	48
19	4104	367(*l*)	636	30	1608	292(*l*)	257	15	232	1	233	5	1138	24
20	4043	164	632	—	1651	97	209	1	223	2	206	17	1122	47
21	3957	184	586	—	1636	54	230	31	245	27	227	26	1084	46
22	3887	173	624	26	1699	40	232	25	216	2	190	9	1016	70
23	3770	94	624	—	1575	34	274	8	198	7	185	2	914	43
24	3784	213(*l*)	597	—	1698	182(*l*)	205	1	197	16	181	2	906	12
25	3780	126	580	—	1687	36	202	3	203	15	218	49	890	23
26	3635	73	579	18	1604	9	196	3	218	19	205	1	833	20
27	3583	138	597	28	1590	78	189	4	215	9	205	5	768	14
28	3434	55	597	—	1575	24	126	1	201	5	196	1	739	44
29	3334	75	546	—	1513	2	136	3	201	13	191	5	756	52
30	3266	212(*l*)	515	7	1556	168(*l*)	130	3	195	7	193	8	677	19
31	3098	103	502	20	1437	31	129	5	174	3	187	7	669	37
Total Fall since Apl. 3.	63		58%		47%		40%		82%		67%		76%	

(*f*) Resumption of Railway Traffic, April 16.
(*g*) Continued Fall in Cases for 10 days till April 26.
(*h*) Sharp Rise in Number of New Cases 12 days after (*f*).
(*k*) Another Rise about 10 days later.
(*l*) An increase of New Cases in Army areas B owing to evacuation of Field Ambulances preparatory to forward movement.

TABLE IV (CHART, pp. 71, 72).

Course of Epidemic of Typhus in Serbia, 1915, *as shown by Number of Admissions and Number of Deaths in* 1*st Reserve Hospital, Kragujevatz, December* 14, 1914, *to May* 14, 1915.

Date	No. Admitted	5 days	30 days	Deaths	5 days	30 days
1914						
Dec. 14	—			—		
15	—			—		
16	—			—		
17	—			—		
18	—	—		—	—	
19	2			—		
20	2			—		
21	—			—		
22	—			—		
23	—	4		—	—	
24	—			—		
25	4			—		
26	1			—		
27	2			—		
28	1	8		—	—	
29	3			—		
30	3			—		
31	3			—		
	—			—		
	—	9	21	—	—	0
	Daily	Average 1				
1915						
Jan. 1	2			—		
2	6			—		
3	4			—		
4	1			—		
5	—	13		—	—	
6	3			1		
7	3			1		
8	3			—		
9	3			—		
10	9	21		1	3	
11	—			—		
12	—			3		
13	7			—		
14	—			—		
15	4			—		
16	1	12		1	4	
17	1			—		
18	1			—		
19	4			1		
20	5			—		
21	6	17		2	3	

Date	No. admitted	5 days	30 days	Deaths	5 days	30 days
Jan. 22	3			1		
23	2			1		
24	5			—		
25	4			—		
26	8	22		1	3	
27	1			—		
28	6			2		
29	4			—		
30	16			3		
31	6	33	118	4	9	22
	Daily	Average 4				1 –
Feb. 1	13			6		
2	62			3		
3	10			2		
4	25			2		
5	13	123		5	18	
6	29			—		
7	26			8		
8	26			3		
9	22			2		
10	16	129		3	16	
11	18			2		
12	37			2		
13	30			5		
14	18			4		
15	19	122		3	16	
16	32			8		
17	26			6		
18	31			12		
19	58			2		
20	27	174		4	32	
21	64			6		
22	23			8		
23	49			9		
24	35			6		
25	38	209		7	36	
26	45			10		
27	39			10		
28	31			19		
Mar. 1	41			15		
2	48	204	961	12	67	185
	Daily	Average 32				6
		Total	1100		Total	207

TABLE IV (*continued*).

Date	No. in Hosp.	No. Admitted	5 days	30 days	Deaths	5 days	30 days
Mission	arrived						
Mar. 3	400	35			13		
4		27			9		
5		40			5		
6		45			13		
7		50	197		10	50	
8	400	37			15		
9		42			18		
10		26			13		
11		33			11		
12		45	183		12	69	
13	580	47			11		
14		37			15		
15		45			11		
16		99			16		
17		27	255		8	61	
18	620	35			12		
19		18			16		
20		31			14		
21		29			5		
22		24	137		16	63	
23	780	17			10		
24		14			10		
25		17			17		
26		23			10		
27		62	135		13	60	
28	784	119			4		
29	801	48			5		
30	767	18			10		
31	759	24			3		
April 1	709	22	231	1138	6	28	331
		Daily	Average 38				10+
2	697	18			7		
3	683	14			6		
4	685	11			11		
5	708	18			2		
6	717	17	78		5	31	
7	779	69			7		
8	770	12			9		
9	757	9			9		
10	759	6			1		
11	719	10	106		6	32	

Date	No. in Hosp.	No. admitted	5 days	30 days	Deaths	5 days	30 days
April 12	618	8			5		
13	682	17			8		
14	694	13			1		
15	667	12			10		
16	640	11	61		4	28	
17	626	10			4		
18	619	7			3		
19	607	9			3		
20	593	10			3		
21	580	6	42		4	17	
22	583	8			2		
23	595	13			1		
24	571	2			2		
25	563	6			—		
26	548	6	35		6	11	
27	525	3			3		
28	508	8			2		
29	475	4			2		
30	427	15			3		
May 1	428	8	38	360	2	12	141
		Daily	Average 12				4
2	424	5					
3	421	3			4		
4	402	2			1		
5	395	1			1		
6	394	5	16		1	12	
7	400	14			5		
8	391	21			6		
9	401	12			6		
10	399	4			1		
11	390	5	56		1	14	
12	384	9			—		
13	384	7	16	88	2	5	31
14	385				3		
15	364						
16	365						
		Daily	Average 7				3–
17	363						
18	359						
19	359						
20	357						
21	353						
			Total	1586		Total	503

Total Admissions Dec. 14, 1914, to May 13, 1915 2686 Total Deaths 710
Remaining in Hospital, May 13, 1915 384

Total completed cases ... 2302 Total Deaths 710
Mortality = 30 per cent

TABLE V (CHART, p. 86).

Course of Epidemic of Typhus in Serbia, 1914-15, as shown by Table of Mortality from Typhus in Kragujevatz Garrison, August 14, 1914, to May 13, 1915.

Date		5 Days	30 Days
1914.			
Aug. 14-31			
Sept. 1-30			
Oct. 1-21			
22	2		
23	—		
24	—		
25	—		
26	1	3	
27	—		
28	1		
29	—		
30	2		
31	—	3	6
Nov. 1	—		
2	1		
3	—		
4	—		
5	1	2	
6	—		
7	—		
8	—		
9	—		
10	—		
11	1		
12	2		
13	1		
14	2		
15	—	6	
16	—		
17	—		
18	1		
19	—		
20	—	1	
21	—		
22	2		
23	2		
24	—		
25	—	5	
26	1		
27	1		
28	—		
29	1		
30	2	5	19
	Daily	Average	1—

Date		5 days	30 days
Dec. 1	—		
2	1		
3	2		
4	—		
5	2	5	
6	—		
7	—		
8	1		
9	—		
10	—	1	
11	1		
12	1		
13	1		
14	2		
15	3	8	
16	2		
17	—		
18	—		
19	3		
20	—	5	
21	2		
22	2		
23	1		
24	—		
25	—	5	
26	1		
27	1		
28	2		
29	1		
30	1	6	30
	Daily	Average	1

Date		5 Days	30 Days
Dec. 31	3		
1915.			
Jan. 1	—		
2	4		
3	3		
4	3	13	
5	3		
6	3		
7	1		
8	1		
9	—	8	
10	—		
11	10		
12	8		
13	3		
14	—	21	
15	—		
16	7		
17	4		
18	4		
19	6	21	
20	3		
21	9		
22	6		
23	5		
24	3	26	
25	7		
26	3		
27	8		
28	8		
29	11	37	126
	Daily	Average	4
30	14		
31	13		
Feb. 1	8		
2	13		
3	16	64	
4	18		
5	31		
6	21		
7	23		
8	19	112	
9	13		
10	17		
11	12		
12	28		
13	11	81	

TABLE V (*continued*).

Date		5 days	30 days	Date		5 Days	Days	Date		5 days	30 days
Feb. 14	21			Mar. 31	14			1914	SUMMARY.		
15	22			Apr. 1	14			Aug.			—
16	11			2	20			Sept.			—
17	25			3	6			Oct.			6
18	12	91		4	14	68		Nov.			19
								Dec.			30
19	16			5	5			1915			
20	18			6	9			Jan.			126
21	16			7	10			Feb.			554
22	16			8	18			Mar.			581
23	27	93		9	11	48		April			218
								May			
24	20			10	4			(13 days)			46
25	18			11	6						
26	21			12	6					Total	1580
27	23			13	10						
28	31	113	554	14	1	27					
	Daily	Average	18	15	11						
				16	6						
Mar. 1	21			17	8						
2	25			18	5						
3	29			19	7	37					
4	20										
5	16	111		20	7						
				21	5						
6	19			22	3						
7	16			23	1						
8	26			24	4	20					
9	25										
10	21	107		25	1						
				26	8						
11	21			27	3						
12	20			28	4						
13	23			29	2	18	218				
14	22										
15	20	106			Daily	Average	7				
16	20			30	4						
17	11			May 1	4						
18	15			2	6						
19	19			3	—						
20	21	86		4	2	16					
21	17			5	1						
22	21			6	4						
23	21			7	1						
24	19			8	5						
25	14	92		9	4	15					
26	15			10	2						
27	20			11	2						
28	16			12	4						
29	15			13	5						
30	13	79	581	14	—	15	46				
	Daily	Average	20		Daily	Average	3				

PART II.

Section VIII.—State of Epidemic Conditions at end of March.

During March the numbers in hospital increased by leaps and bounds—typhus from 3,743 up to 8,040 cases in hospitals—an increase of 4,297 cases; relapsing from 7,009 to 8,661 cases—an increase of 1,658 cases. Enteric alone had fallen from 2,314 to 1,856—a fall of 458 cases. In typhus and relapsing combined there had been an increase of, roughly, 7,000 cases (*see* Chart, p. 44, Table, p. 58).

The first measure of prevention—suspension of railway traffic—had been put in force on March 16 for a fortnight. In the *fifteen* days before suspension the numbers of typhus had increased by 2,447 cases (Chart, p. 45), of relapsing by 874 cases (Chart, p. 45), a total increase of 3,121 cases (Chart, p. 44). In the next *ten* days after the suspension the numbers had increased—typhus by 2,050 cases (Chart, p. 45), relapsing by 778 cases (Chart, p. 45), a total of 2,828 cases (Chart, p. 44).

Up to this time (March 27) the epidemic appeared to have been in no way affected by the suspension of railway traffic. This had been put into force on March 16, and was due to expire on March 30. I had by this time also got the figures of cases in hospitals, so far as this could be obtained, from the commencement of the epidemic in December, 1914, up to the time of our arrival in March. They were the following, the figures in the earlier period being only approximate as were indeed the later figures (Table, p. 58).

Course of Epidemic, December, 1914, to March 27, 1915, as shown by Numbers of Cases in Hospitals on date specified.

Position of Epidemic.—The data as to numbers of cases in hospitals were the only data available at the time; they related to forty-two chief places in Serbia, and were supplied and analysed by me each night, separate data of a similar kind relating to Kragujevatz being also received.

The degree of increase of the *typhus epidemic* was well shown in the Kragujevatz data (Table, p. 85, Chart, p. 86). Between March 3 to 28 the number of typhus cases in the eight hospitals had risen from about 450 cases up to 1,000 cases (Chart, p. 86); in the largest hospital (first Reserve) the number had increased from about 400 up to 780 cases (Table, p. 80).

TABLE B (CHARTS, pp. 86, 87).

Course of Epidemics as shown by General Hospital figures of Sick and Wounded in Eight Kragujevatz Hospitals on dates specified, March 14 to June 5, 1915. (Chart 9, p. 87.)

	March 14	March 27	April 3	April 26	April 27	May 3	May 17	May 31	June 5
A									
Number in hospital	4,052	3,376	2,865	2,902	2,862	2,360	2,615	2,262	1,989
Wounded ...	1,391	974	743	725	683	586	645	596	576
Sick ...	2,661	2,402	2,122	2,177	2,179	1,774	1,970	1,666	1,413
Serbian sick ...	1,790	1,483	1,162	1,079	1,066	973	1,187	1,005	888
Prisoners sick ...	871	919	960	1,098	1,113	801	783	661	525
B									
Fever cases ...	1,477	1,523	1,209	849	830	751	710	592	474
Typhus ...	607	973	854	690	668	572	596	484	387
Relapsing ...	668	497	321	114	117	139	100	78	52
Enteric ...	132	16	6	8	8	6	2	2	1
Dysentery ...	27	7	5	21	21	22	2	14	14
Other fevers ...	43	30	23	16	16	12	10	14	20

	October	November	December	January	February	March	April	May 1-14	Total
C									
Total deaths from typhus in Kragujevatz area	6	19	30	126	554	581	218	46	1,580
Daily average ...	1	1	1	4	18	20	7	3	—
D									
Admissions ...	—	—	21	118	961	1,138	360	88	2,686
Deaths ...	—	—	—	22	185	331	141	31	710
Case mortality from typhus in 1st Reserve Hospital	1	—	—	30 per cent.	30 per cent.	40 per cent.	18 per cent.	14 per cent.	30 per cent.

This table shows the striking fall in numbers, admissions and deaths, of typhus in hospitals, which set in at the end of March, exactly twelve days after suspension of railway traffic and stoppage of leave from army.

A = Total number of cases and deaths in hospitals from all causes (*see* Chart 9, p. 87).

B = Number of fever cases, especially typhus and relapsing (*see* Chart 9).

C = Total deaths from typhus in Kragujevatz area (*see* Chart 11).

D = Admissions, deaths, and case mortality of typhus in 1st Reserve Fever Hospital, the largest fever hospital in Serbia (*see* Table IV and Chart 10).

Chart 8.

Course of Epidemics of Typhus & Relapsing Fever as shewn in Kragujevatz
A+B = N°. of Cases in Hospitals; C+D = Admissions; E = Deaths from Typhus.

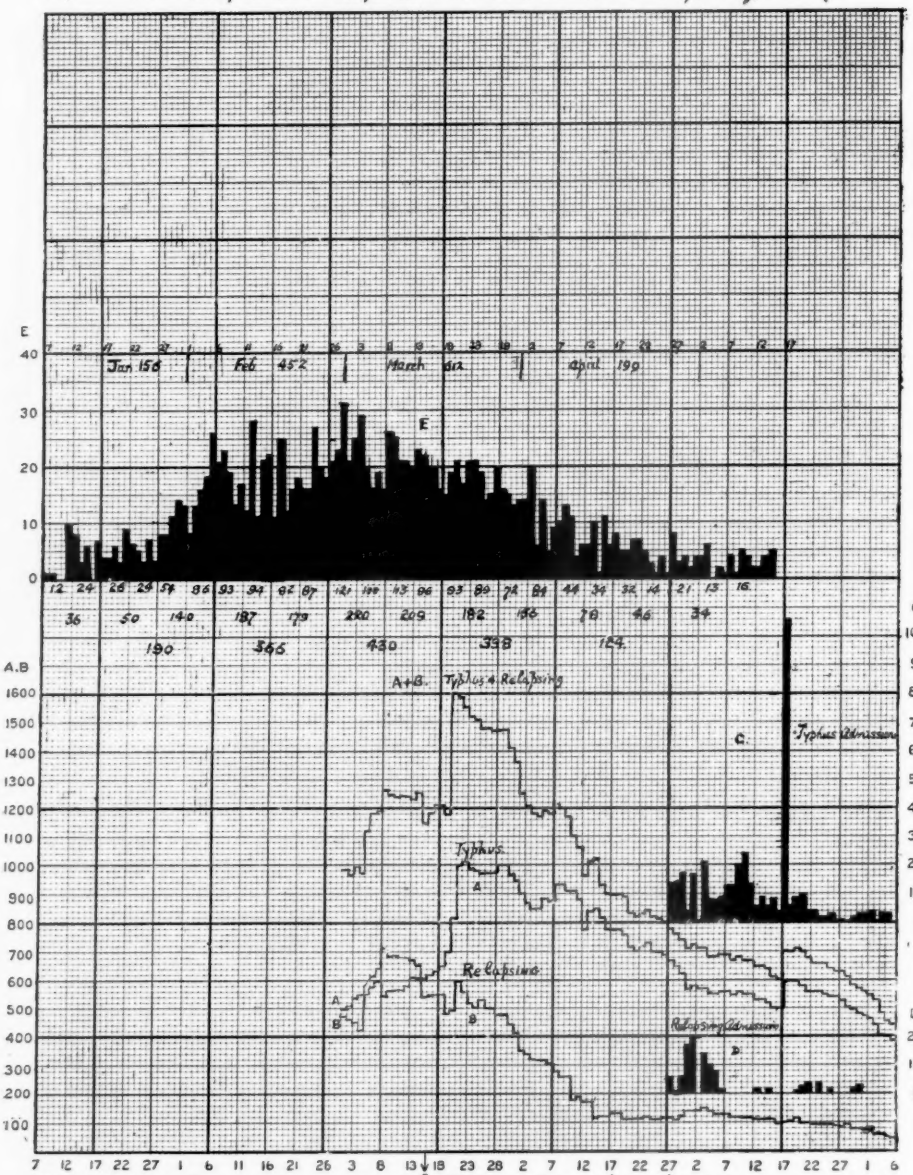
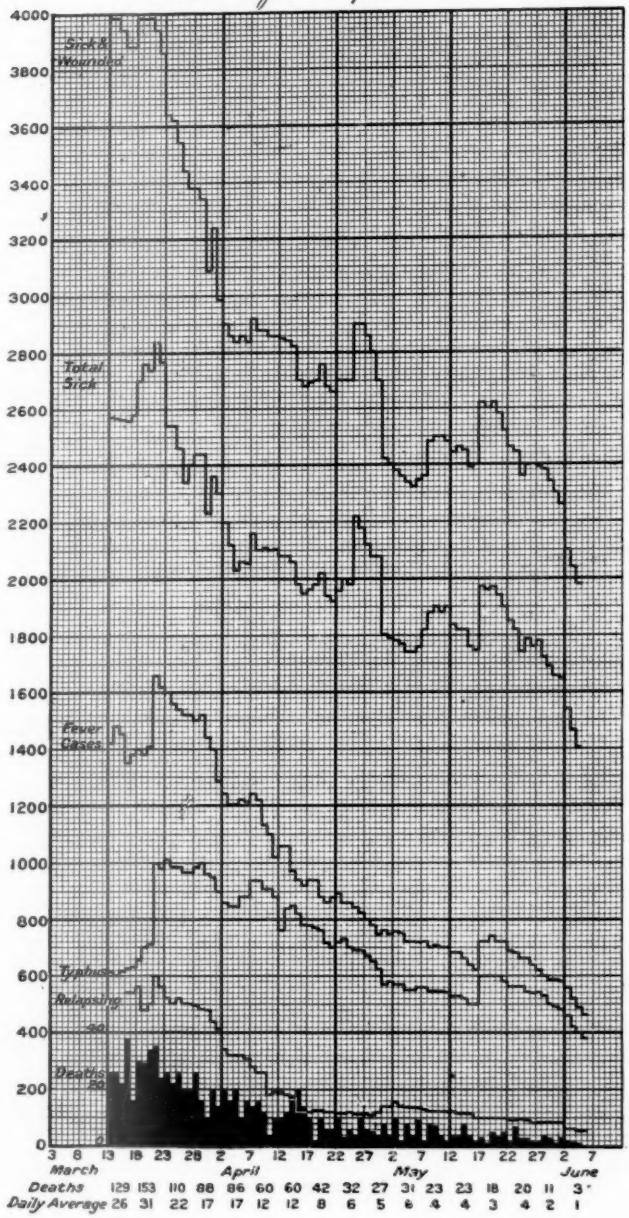


Chart E Deaths Oct. 6 cases (1st Case Oct. 22)
Nov. 15
Dec. 32
Jan 156
Feb. 452
March 612
April 199

Chart 9

Numbers of Sick & Wounded, & Total Sick, Fever Cases, Typhus & Relapsing, & Deaths in Kragujevatz Hospitals March 13 – June 3, 1915.



Deaths	129	153	110	88	86	60	60	42	32	27	31	23	23	18	20	11	3
Daily Average	26	31	22	17	17	12	12	8	6	5	6	4	4	3	4	2	1

Figures of admission or mortality were not, at this time, obtainable. When at a much later period (in May) I obtained these for this hospital, they showed (Table, p. 80; Chart, p. 71) that in the fifteen days before suspension (March 3 to 17) the number admitted had been 635 cases, number of deaths 180; while in the fifteen days of the suspension period (March 18 to 31) the number admitted had been 503 cases, the number of deaths 151—a total of 1,138 new cases in thirty days; with 331 deaths (Table, p. 85).

The *mortality* was also very high. The total of cases completed (by discharge or death) during the thirty days, March 3 to April 1, had been 829 cases, and the number of deaths among these had been 331 cases or 40 per cent. (Table, p. 70; Chart, p. 71). The corresponding admissions figures (December 19 to March 2) had been 1,100 new cases and 207 deaths; the number of completed cases had been 700 cases with 207 deaths—a mortality of 30 per cent.

Both the incidence of the disease and its mortality had therefore greatly increased. Its mortality was very high, from a proportion of one in three cases, it had risen, at this period (March 28) to nearly one in two cases (p. 139).

Distribution of Typhus and Relapsing in Northern and Southern Serbia on March 27 (Charts A and B, pp. 46, 47).

The distribution at end of March showed how extensively the army area was infected, both with typhus and relapsing fever, viz., a total of over 10,000 cases, as compared with a little over 6,000 cases in the rest of Serbia (Tables, pp. 62-64). It showed also how deeply infected the Valyevo area (first army) was; the original source of all the trouble with its total of 2,356 cases (Area A); how wide the epidemics also were in the second and third and Belgrade army areas, with their total of 4,978 cases (Area B); as, also, the Western (Ujitze), and Eastern (Zeitjer) war areas (D and E) with their totals of 2,381 cases. A grand total of 10,280 cases in the war areas, as compared with 6,329 cases in civilian areas. (Tables, pp. 62-64.)

This latter figure, it was to be borne in mind, did not in any way include the incidence of the diseases in the civilian population, men, women and children, for whom no kind of hospital isolation or even notification was possible. That incidence was widespread. From data relating to mortality from typhus in Kragujevatz area, October 22, 1914, to May 15, 1915, which I afterwards obtained (Chart, p. 86), the number of deaths

from typhus in that area during January, February and March had been 1,220, as compared with 538 cases in the chief Fever Hospital (first Reserve) which contained four-fifths of all the cases in hospitals. During January the number was 156 and 78 respectively. During February the numbers were 452 and 185 respectively; during March 612 and 331 respectively. There was a large mortality from typhus in cases outside the hospitals.

At a later period (May 1 to 14) when the epidemic was completely broken, out of a total of 937 cases treated in the Roadside Dispensary of Mrs. Stobart's mission in Kragujevatz, 319 were surgical cases and 618 medical cases. These 618 medical cases were constituted: digestive, 205; respiratory, 109; circulatory, 26; renal, 22; nervous, 20; anæmia, 46; skin diseases, 26; general diseases, 83; and infectious diseases, 118 (about 20 per cent.). This latter included typhus 18 and post-typhus complications 12 (5 per cent.); relapsing, 11; a total of 41 —about 7 per cent.

Period 2 (*April* 1 *to* 26).—*Progress of Typhus Epidemic.*

The course of the epidemic after the end of March was a most interesting one. The "black" period as it appeared at the end of March proved to be the darkness before the dawn. It was never blacker than on the date April 3. The numbers in hospitals were then at their very highest, viz., 8,213 typhus, and the pressure was greatest. Nevertheless, I reported to the War Office in my telegram of that day, that I estimated that the typhus outbreak had reached its maximum about two weeks before, i.e., about March 20. This opinion was based on the fact that the net increase of typhus cases in the hospitals for the past week (March 28 to April 3) had been only 233 as compared with 1,183 one week before.

Between March 1 and 7 the numbers in hospital had risen from 3,743 to 4,569—an increase of 826.

Between March 7 and 14 they had risen from 4,569 to 5,361, an increase of 792.

Between March 14 and 21 they had risen from 5,361 to 6,797—a net increase of 436.

Between March 21 and 28 they had risen from 6,797 to 7,980—a net increase of 1,183.

Between March 28 and April 3 they had risen from 7,980 to 8,213 —a net increase of 233.

The admissions of cases into hospitals had thus reached their maximum on March 21 to 28. Allowing an incubation period of twelve days this represented a maximum period of contraction of infection about twelve days before, viz., March 16. Suspension of railway traffic had commenced on March 16. During the week before, contraction of infection had been at its very highest.

The conclusion I expressed in my telegram of April 3 was that I attributed this lessening largely to the suspension of railway traffic and stoppage of all leave from the army, so strongly urged by us on our arrival, and put into force on March 16. And in spite of the large numbers of cases in hospitals, I stated that I was glad to report improvement in the general conditions.

In my telegram a week later (April 11) I stated that my opinion expressed in my previous telegram had been confirmed by what I had seen of the conditions of hospitals visited by me in Uskub (April 6 to 8) and Nish during the past week—most notably in the fever hospital in Nish in which the conditions were so terribly bad four weeks before at the date of our arrival. A great transformation had been effected there by the energy of the Serbian officer in charge, everything being now clean and well ventilated and in surprisingly good order—the most striking change noted by me in any hospital in Serbia.

April 3 to 10.—The actual position of the epidemic at this date (April 11) was that the number in hospitals had fallen from 8,213 on April 3 to 8,077 on April 10, a net fall of 136 cases instead of a net rise of 233 cases the preceding week. This denoted that the numbers admitted were now for the first time being exceeded by those discharged or dying.

April 10 to 18.—The position a week later was still better. The number had further fallen from 8,077 on April 10 to 6,984 on April 18 —a further net fall of 1,093 cases in hospitals.

April 18 to 26.—And lastly, a week later, the numbers had still further fallen from 6,984 on April 18 to 5,979 on April 26, a net fall of 1,005 cases. Altogether since April 3 there had been a net fall of 2,234 cases of typhus in hospital—*a fall of 27 per cent.*; and a still greater fall of 4,002 cases of relapsing fever in hospitals—*a fall of 52 per cent.* (*see* Tables H and K, pp. 62-64).

The new data (of cases admitted) which I had asked for on April 2 were furnished in the daily reports from April 14 onwards (*see* Tables H and K). These showed that in the five days, April 22 to 26, the number of new cases admitted daily had been 1,151 cases.

Allowing for an incubation period of twelve days, this meant that this was the number of cases contracting infection during the five days, April 10 to 15, immediately preceding the resumption of railway traffic on the 27th. On April 16, exactly ten days after resumption of railway traffic, the number of admissions suddenly rose to 611 cases, and in the following five days, April 27 to May 1, it totalled 1,919 cases, (average 400 daily). The infection of these cases must have been contracted at least ten days before—i.e., in the first five days, April 17 to 22, after railway traffic was resumed.

COURSE OF EPIDEMICS AS SHOWN BY NUMBERS OF CASES IN HOSPITAL AND NUMBERS OF DEATHS AT DIFFERENT DATES AT VARIOUS PERIODS (JANUARY 1 TO JUNE 4).

(*A*) All Serbian hospitals (Table, p. 92).

(*B*) Kragujevatz hospitals (Chart, p. 87).

(*C*) Kragujevatz First Reserve Fever Hospital, the largest in Serbia.

The data here given are the official figures of the returns in forty-two hospital centres in Serbia which I received nightly. They are incomplete, especially for the earlier periods, January and February, and by no means represent the prevalence of cases of typhus and relapsing in the country—probably five times greater than the figures here given. They accurately represent, however, the relative prevalence in the different periods.

(*A*) *All Serbia.* (Table, p. 92.)

The total number of cases in hospital on March 1 at time of our arrival stood at 46,000. By the end of April it had fallen to 31,000, and June 5 to 18,000. Every available place was used to accommodate patients. There was need of beds for 16,000 patients. By April 27 there were over 6,000 empty beds available, and by June 4 over 13,000.

Number of wounded: On March 7 there was over 11,000; by the end of April this had fallen to 5,700, and by June 4 to 4,200.

Total sick: On March 7 this was 36,300; by the end of April this had fallen to 25,800, and by June 4 to 13,000.

Number of typhus cases: On January 1 this was stated to be about 113, but figures at this period were very incomplete. By February 1 it had risen to 1,200; by March 1 to 3,700; by April 2 to 8,200 (maximum). It then fell to 6,000 on April 26, rose sharply to 6,300 on April 27, remained for a week, then fell to 2,800 on June 4.

Numbers of relapsing fever: On January 1 this stood at over 1,700. By February 1 it had risen to 5,300; by March 1 to 7,000; by March 27 to 8,600 (maximum). It fell to 5,000 by April 16, and 3,800 by April 26, and thereafter to 1,600 cases on January 4.

Enteric: The number of cases in hospitals on January 1 was about 900; it rose to 1,800 by February 1; to 2,380 on March 1 (maximum). It then fell to 1,500 on April 3, and thereafter to 160 on June 4.

TABLE A.

General Hospital figures of Sick and Wounded in Serbian Hospitals, March 1 to June 5.

	March 1	March 7	March 27	April 3	April 16	April 26	April 27	May 3	May 17	May 31	June 5
Number in hospital	—	46,000	—	—	—	—	31,000	29,000	23,000	19,000	18,000
Wounded ...	—	11,037	—	—	—	—	5,747	3,175	4,575	4,208	4,277
Sick ...	—	35,351	—	—	—	—	25,895	23,818	18,407	14,703	13,781
Fever cases	—	15,000	— (*d*)	17,916 (*d*)	— (*f*)	10,888 (*g*)	11,268 (*h*)	10,528 (*h*)	7,153	5,088	4,788
Typhus ...	3,750	4,570	8,050	8,213	7,324	5,979	6,314	6,263	4,376	3,092	2,860
Relapsing ...	7,050	7,850	8,650	7,810	4,960	3,808	3,904	3,166	2,120	1,643	1,609
Enteric ...	2,300	2,174	1,900	1,549	1,100	900	878	797	341	188	160
Dysentery ...	400	375	—	221	—	201	193	178	124	100	84
Other fevers	—	147	—	123	—	—	69	124	192	60	76
Number of deaths (all causes) on day specified	—	—	—	—	—	115	117	132	65	50	48
Empty beds	—	—	—	—	—	(7,000)	6,243	7,265	9,329	11,870	13,261

(*d*—*g*) This table shows the fall in numbers of cases of typhus and relapsing fever in hospitals, which occurred from the end of March (*d*), when they were at a maximum, up to April 26 (*g*).

(*h*) The increase of cases in hospitals which suddenly occurred on April 27, exactly twelve days after resumption of railway traffic on April 16.

Dysentery: The number of cases on March 1 was stated to be 400, and thereafter fell to 84 on January 4.

Other fevers: The number of these in hospitals was very small, but they do not represent the prevalence of other fevers, especially diphtheria, in the civil population.

(*B*) *Kragujevatz Hospitals.*

The numbers in Kragujevatz hospitals at different periods are shown in Table, p. 85, Chart, p. 87. They were on an average about one-tenth of the whole cases in Serbia. The total number fell from over 4,000 on March 14 to 2,000 on June 4. The number of wounded from 1,400 to 580. The number of sick from 2,600 to 1,400—made up of Serbians and prisoners, viz., 1,800 Serbians and 800 prisoners on March 14; 900 Serbians and 500 prisoners on June 4. The number of deaths daily fell from twenty-six on April 8 to eighteen on April 3; seven on April 26, and one on June 4. The total fever cases rose from 1,400 on March 14 to 1,600 on March 27, and then fell to 470 by June 4. The number of typhus cases on March 14 stood at 600 cases. It rose sharply to 1,000 cases on March 27 (maximum), fell sharply to 660 on April 27, and then afterwards to 370 by June 4. The number of relapsing fever cases on March 14 was 670, fell to 600 on March 27, and then rapidly to about 120 by the end of April. They showed a slight rise about May 3 and afterwards fell to fifty cases on June 2.

(*C*) *Course of Typhus Epidemic in Kragujevatz.*

The figures in Table, p. 85, Chart, p. 86, give very interesting information regarding the course of the typhus epidemic in Kragujevatz area. The first deaths were recorded on October 22, 1914, and the numbers thereafter were 6 in October, 19 in November, and 30 in December. They then rose to 126 in January, 554 in February and 581 in March. Then a marked fall occurred, viz., to 218 in April and 46 in the first two weeks of May—a total altogether of 1,580 cases between October 22 and May 14, of which 1,135 were in February and March.

The course is shown (D) by number of admissions and deaths in the First Reserve Hospital, the largest fever hospital in Serbia, which throughout the epidemic had about one-twelfth of all the cases in Serbian Hospitals (Table, p. 70; Chart, pp. 71, 72). The admissions in December were 21; they then rose to 118 in January, 961 in February, and 1,138 in March. A sharp break then occurred, viz., to 360 in April, and 88 in the first two weeks of May—a total altogether of 2,686 admissions between December 19 and May 14, of which 2,100 were in February and March. The deaths were: *Nil* in December, 22 in January, 185 in February, and 331 in March. They then fell to 141

in April and 31 for the first two weeks of May—a total of 710 cases between January 6 and May 14, of which 516 were in February and March. The case mortality in January and February was 30 per cent., in March 40 per cent. In April it fell to 18 per cent., in May to 14 per cent.; the average mortality of 2,302 completed cases for the whole period being 30 per cent.

(*D*) *Course of Typhus Epidemic in all Serbia.*

This is shown in Charts, pp. 45, 95, and Tables, pp. 58, 76. The epidemic began in December. By the end of January, the number of cases in hospitals was about 1,100. By the end of February it had risen to over 3,700 cases, and by March 28 to over 8,000 cases. In the first fifteen days of March the numbers rose from 3,700 to 5,800 cases—an increase of nearly 2,000 cases. In the second fifteen days they rose to 8,000, a further increase of 2,200 cases.

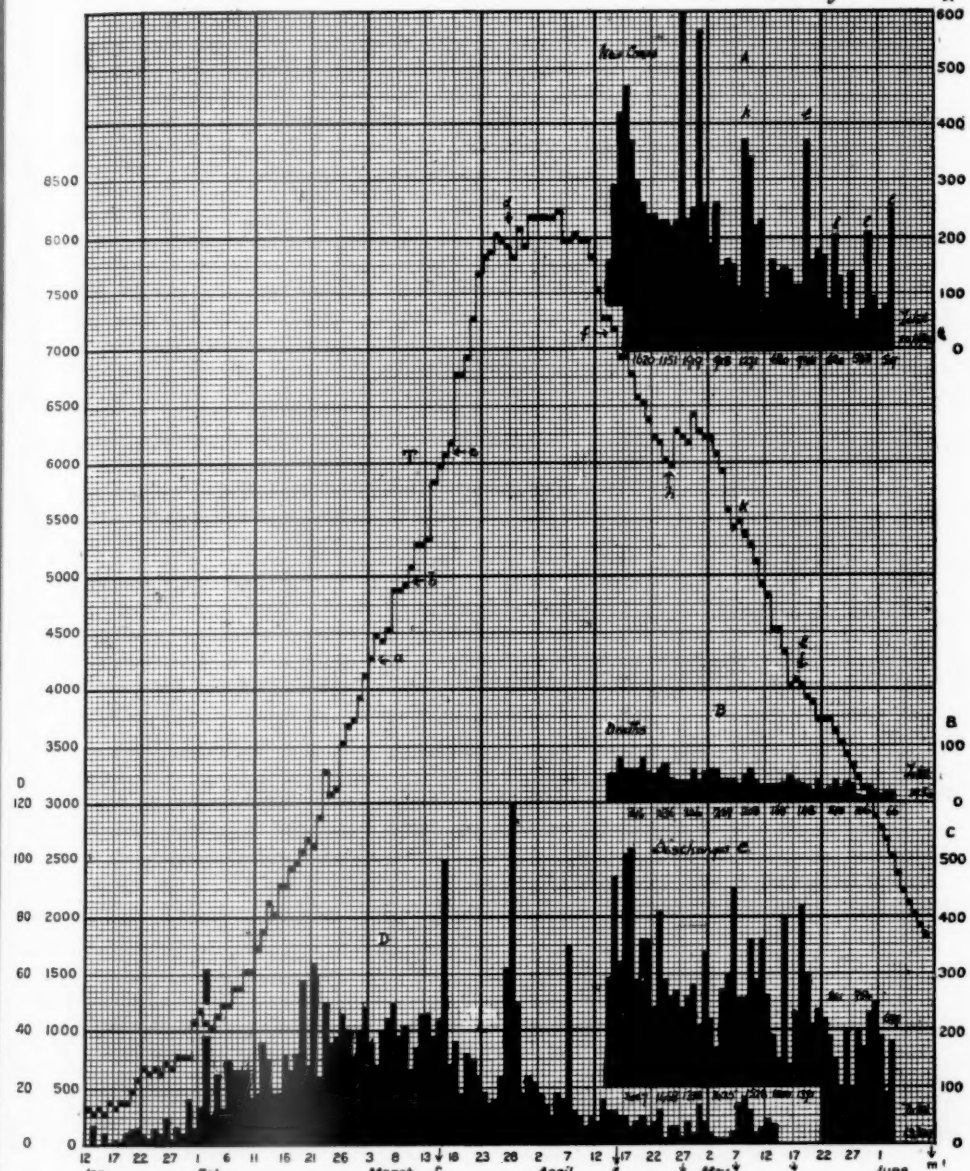
April.—They remained at a level of 8,000 to 8,200 cases for a period of fifteen days (March 28 to April 12), indicating that the numbers of admissions had greatly fallen off, only equalling the discharges and deaths instead of largely exceeding them. A most rapid fall then occurred from 8,000 on April 12 to 6,000 on April 26. The fall in admissions during the period April 17 to 26 was from 464 cases to 228 cases. The percentage fall in number of cases in hospitals from April 3 to 26 was 27 per cent. both for the army and the civilian areas (Table, p. 62). But in the army areas the percentage fall in army area A was only 4 per cent., whereas in area B it was 42 per cent. (Chart, p. 96.)

May.—Then followed a sudden sharp rise (*h*) between April 27 and May 1, exactly ten to twelve days (incubation period) after resumption of railway traffic and leave from the army on April 16 to 20. The admissions rose from 228 on April 26 to 611 on April 27 and 570 on April 30—a total of 1,919 cases in five days, as compared with 1,151 cases in the preceding five days. (*See* Chart, p. 66.)

After the first rise there followed a fall from 6,580 on May 1 to 5,500 on May 6, when another rise occurred (*k*) due to increased admissions of 368 and 343 on May 8 and 9; then a further more rapid fall to 4,000 cases on May 16 and finally to below 2,800 cases on June 4. The total fall since April 3 was 63 per cent. (Table, p. 62).

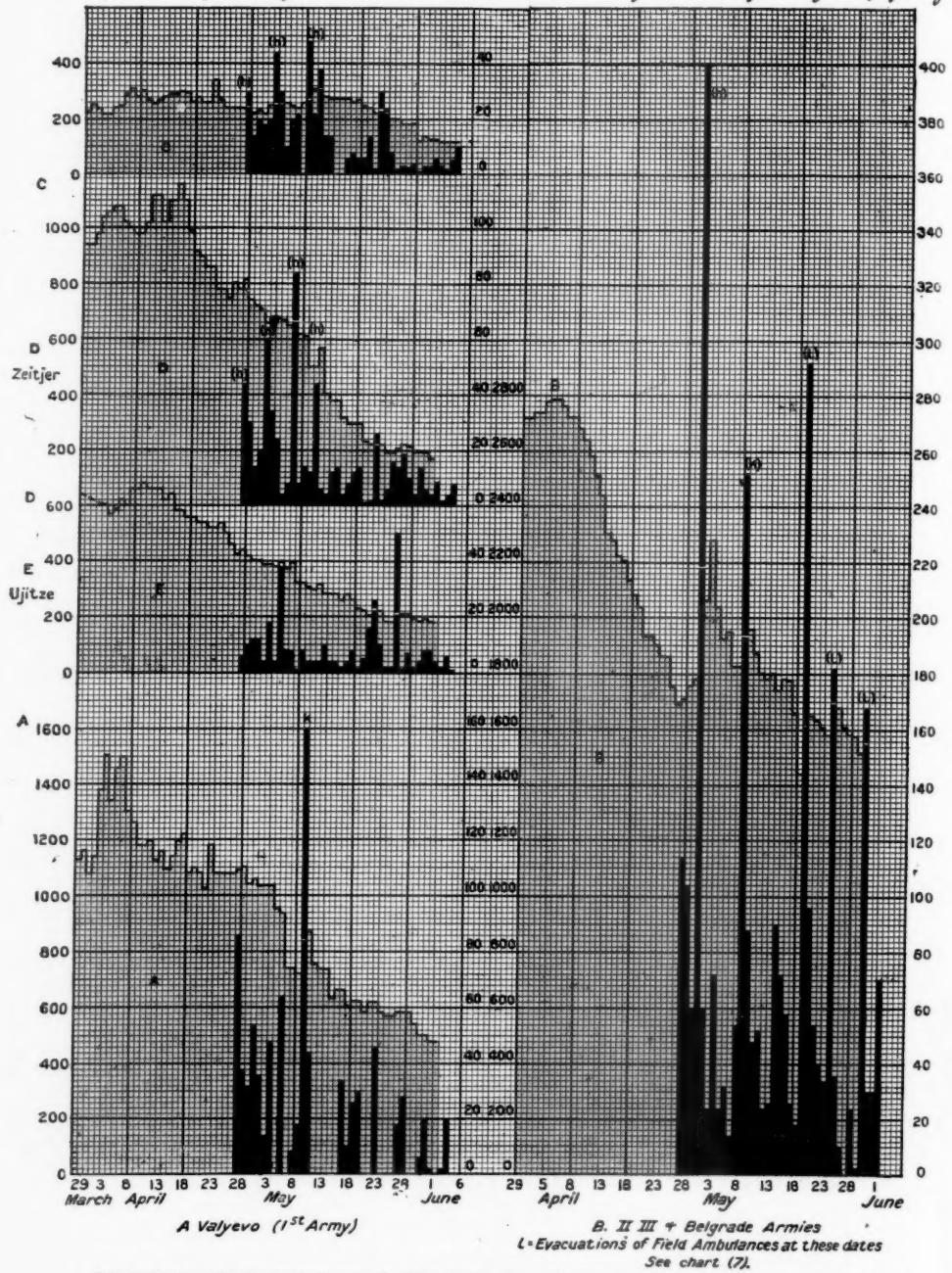
The fall in the latter fourteen days was interrupted by slight rises in admissions (*l*) on certain days. These were connected with sudden

Chart 3 Course of Epidemic of Typhus in Serbia in 1915, as shewn by { T N.° of Cases in Hospital; A " " New Cases (April 17–June 4); B " " Deaths; C " " Discharges }



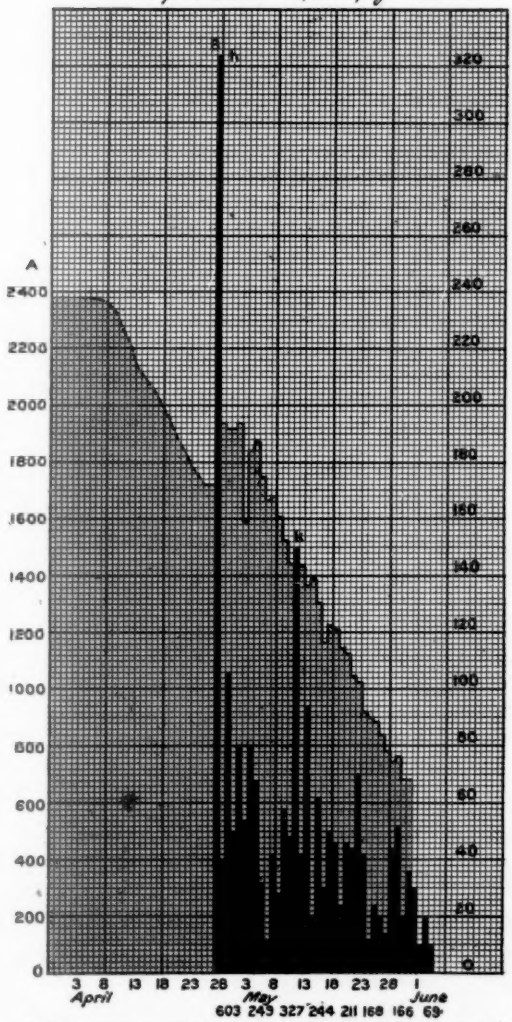
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Chart 5: Course of Epidemic of Typhus in Serbia in different War Areas.
A. Valyevo (1st Army); B. Belgrade + Danube (2nd + 3rd Armies); C. Railway Towns; D. Zeitjer Army; E. Ujitze Army.



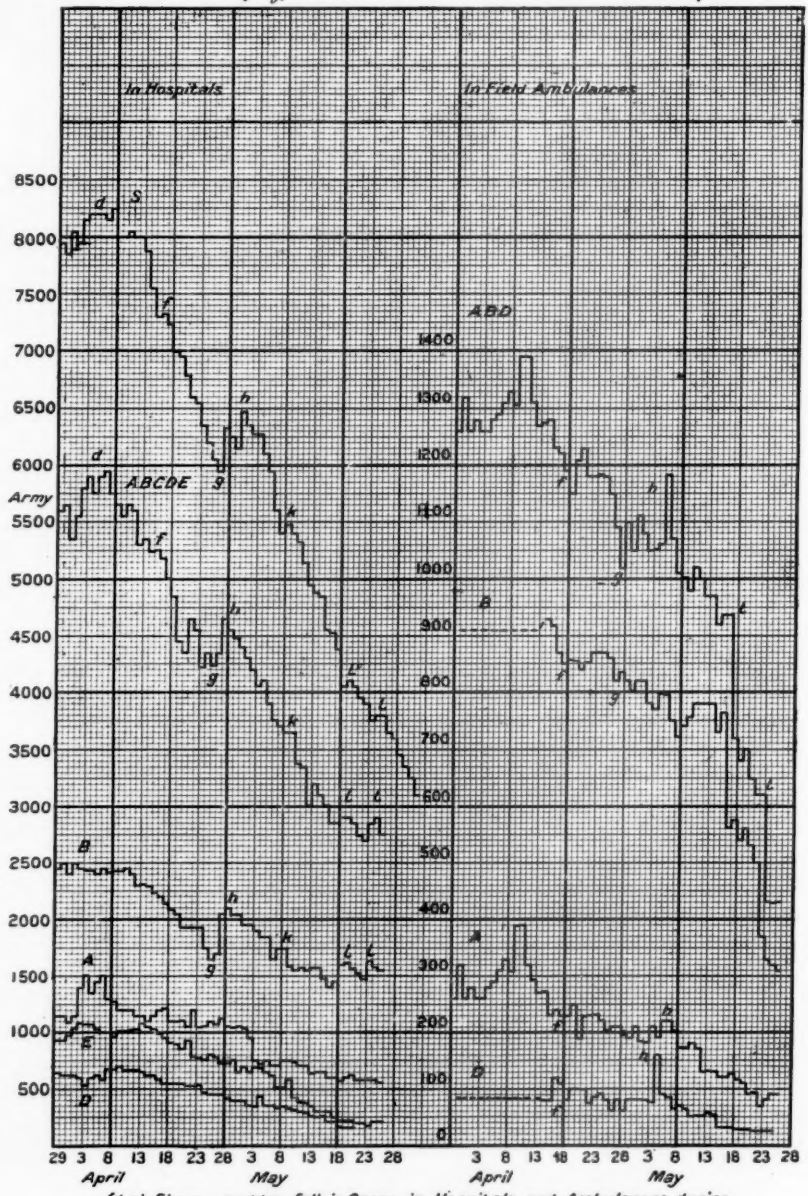
Black Blocks = Nº of Admissions Daily (April 27 - June 4). Shewing recrudescences (h & k) of New Cases on April 27 - May 13, commencing 12 days after resumption of Railway Traffic on April 16.

Chart 6. Course of Typhus Epidemic in Civilian Areas in Serbia, as shewn by; A. N°. of Cases in Hospitals; B. N°. of New Cases (April 17 - June 4).



Black Blocks = N°. of Admissions Daily (April 27 - June 4). Shewing recrudescences (h & k) of New Cases on April 27 - May 13, commencing 12 days after resumption of Railway Traffic on April 16.

Chart 11. No. of Cases of Typhus in War Territories (A.B.C.D.E.) + in whole of Serbia (S).



(d-g) Shews sudden fall in Cases in Hospitals and Ambulances during suspension of Railway Traffic, and 10 days after its resumption at (f).
(h) Sudden increase of Cases with Hospitals and Field Ambulances 10-12 days after resumption of Railway Traffic at (f).
(k) A second increase 10 days later.
(l) Sudden Evacuation of Field Ambulances in Army area B. on May 16 & 26 and rise in number of Cases in Hospitals on these dates.

evacuations of field ambulances in war area B, preparatory to forward movement of troops (May 17 to 31), which by this time were once more in full fighting order. (*See* Chart 11, p. 98.)

The figures of admissions, deaths and discharges for this period (April 14 to May 31) are given in Tables, pp. 76, 62, and represented graphically in the black blocks in Chart, p. 95.

TABLE D (CHART, p. 98).

Distribution of Typhus Cases in Hospitals and Field Ambulances, March 27 to May 31.

(*a*) Army areas, A, B, C, D, E.
(*b*) Civilian areas, F.

In Hospitals	March 27	(*d*) April 3	(*g*) April 26	(*h*) April 27	May 3	May 17	May 31
Army area A ...	1,240	1,221	1,126	1,120	1,069	671	602
,, ,, B ...	2,868	2,789	1,692	1,710(*h*)	2,033	1,660	1,437
,, ,, C ...	229	216	244	255	252	267	129
,, ,, D ...	896	1,013	771	813(*h*)	684	294	174
,, ,, E ...	650	571	431	436	381	258	187
	5,883	5,810(*d*)	4,264(*g*)	4,342(*h*)	4,425(*h*)	3,150	2,429
Civilian area, F ...	2,167	2,403	1,715	1,972(*h*)	1,838	1,226	669
Total ...	8,050	8,213	5,979	6,314(*h*)	6,263	4,376	3,098

Field ambulances	March 30	April 7	April 26	April 27	May 3	May 17	May 25
1st Army, area A	316	329	207	208	206	205	127
2nd ,, ,, B	(200)	(200)	163	163	198	43	67
3rd ,, ,, B	(500)	(500)	388	393	357	281(*l*)	101
Belgrade ,, B	(400)	(400)	271	279	215	252(*l*)	147
Total B area ...	(1,100)	(1,100)	822	835(*h*)	770	586(*l*)	315(*l*)
,, E	(100)	(100)	58	78	162(*h*)	33(*l*)	23
,, D							
	(1,500)	(1,500)	1,087	1,121(*h*)	1,138	746(*l*)	428
Grand total of cases in hospitals and field ambulances	(9,500)	(9,700)	7,066	7,435(*h*)	7,401(*h*)	5,122	3,526

This Table shows :—

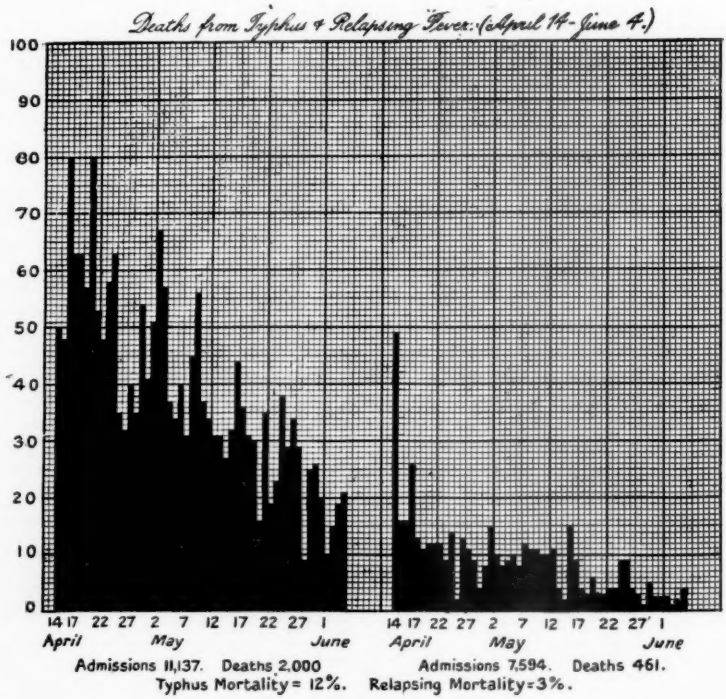
(*d*) and (*g*) the fall which occurred in cases in hospital between April 3 and April 26.

(*h*) The rise which suddenly occurred on April 27, exactly twelve days after resumption of railway traffic on April 16, affecting both the army and the civilian areas. This rise also occurred in the field ambulances at this period (April 27 to May 3). (Chart, p. 98.)

(*l*) Between May 17 and 31 sudden evacuations of cases took place from the field ambulances of area B, keeping up the number of cases in hospitals (*see* Chart 11).

The chart of admissions shows very clearly the steady fall between April 17 to 26, and the sudden rises (*h*) on April 27 and 30, and (*k*) on May 7 to 11, and then the more gradual fall, varied by incidental rises (*l*) connected mostly with evacuation of field ambulances (*see* Charts, pp. 95, 98).

CHART 16.



Total Admissions and Deaths from April 14 to May 31.

The total admissions in thirteen days, April 14 to 26, had been 3,634 cases, an average of 280 cases daily. The total admissions April 27 to May 31, a period of thirty-five days, was 6,996, a daily average of 200 cases. Altogether between April 14 and May 31, a period of forty-eight days, the total admissions into hospitals were 10,630, a daily average of 221, and the total deaths 1,948, a daily average of 40 cases. The total completed cases during this period was 15,070, representing a mortality of 12·2 per cent. (Chart 16; Table, p. 62).

Chart 7 Number of cases of Typhus in Armies in Serbia, detained in Field Ambulances, shows sudden evacuations in 2nd Army (May 15) & 3rd Army (May 20) preparatory to forward movements.

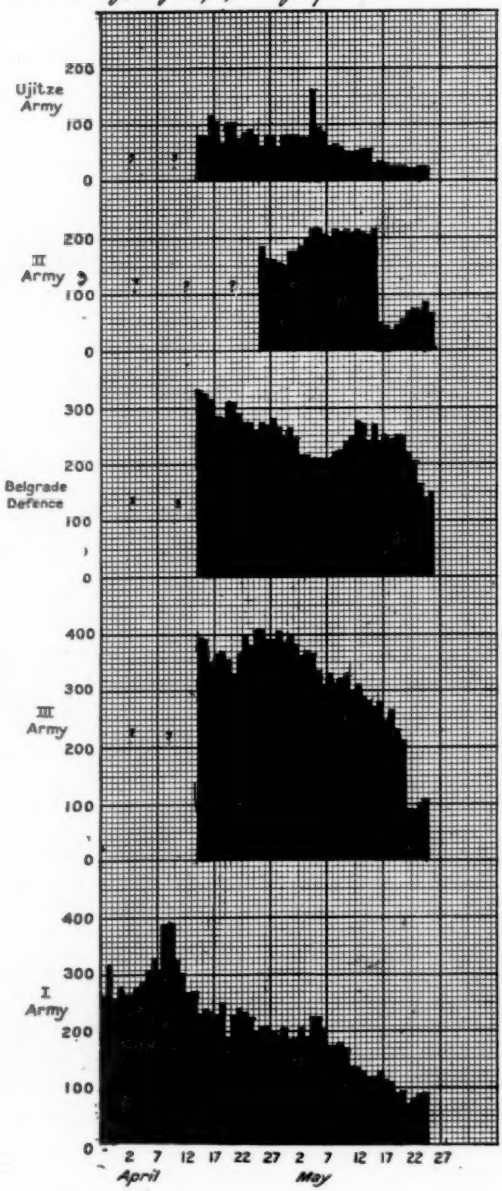
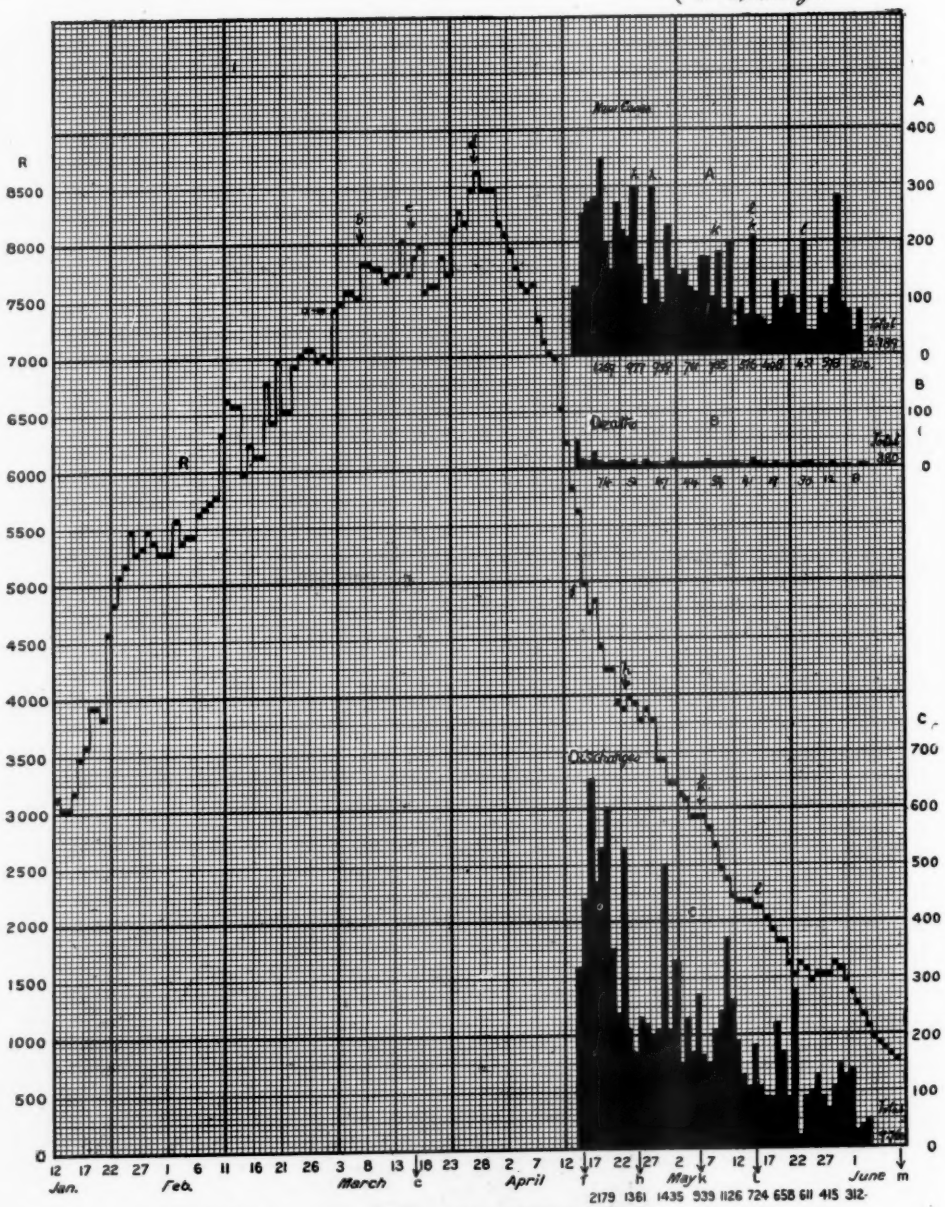


Chart 4. Course of Epidemic of Relapsing Fever in Serbia 1915, as shown by R = No. of Cases in Hospitals; A = " New Cases (Apr. 7th–June 4); B = " Deaths; C = " Discharges



a = Arrival of Mission
b = Programme of Prevention
c = Suspension of Railway Traffic
d = Arrest in No. of Cases in Hospital 10 days after c.

f = Resumption of Railway Traffic
h = Increase of Cases in Hospital 10 days after f.
k = A Second increase 10 days after h.
L = A Third increase 10 days after k. due to evac. of Fd Ambul. (Chart 7)
m = Mission left Serbia.

The mortality corresponded very closely with that shown in the more detailed figures for the First Reserve Hospital in Kragujevatz—viz., 18 per cent. in April and 14 per cent. for the first half of May, as compared with a mortality of 30 per cent. during January and February and 40 per cent. during March (*see* Chart, p. 71). The fall in the epidemic was thus accompanied by a marked fall in mortality in its later periods.

The mortality for 2,302 cases in First Reserve Hospital over the whole period January 1 to May 14 was 30 per cent.

(*E*) *Course of Relapsing Fever Epidemic* (*see* Chart facing; Tables, pp. 58, 64, 76).

This was the first to show itself—preceding the typhus epidemic by several weeks (*see* Chart, p. 45); largely exceeding it in numbers up to the end of January (5,500 and 1,000 cases respectively in hospital), also up to the end of February (7,000 and 3,500 respectively), and finally caught up by the typhus at the end of March—viz., 8,500 and 8,000 respectively.

It will be seen from the curve of cases in hospital that the relapsing fever maintained itself at a high but steadily increasing figure of 6,000, 7,000 and up to 8,500 from February 10 to April 2, a period of fifty days, whereas the typhus curve of cases rose steeply in this period from 2,000 cases up to 8,000 cases and then maintained this latter level only for fifteen days.

The two epidemics were broken about March 28, the relapsing beginning to fall on April 2, the typhus on April 12. The subsequent fall as shown by cases in hospital was very rapid in both, but always greater and about ten days in advance in the case of the relapsing fever. In the three weeks between April 3 (*d*) and April 26 (*g*) the numbers in hospitals fell from 7,810 to about 3,800—a total of 4,000 cases, a fall of 52 per cent. (Table, p. 64).

The figures of admissions (shown in Table, p. 64) show that the daily average in the period April 14 to 17 was 213 cases, 252 in the next five days (April 17 to 21) and then down to 195 in the period April 22 to 26. In the next period, April 27 to May 1, there was a sudden rise in admissions to 304 cases, keeping up the numbers in hospital (*h*).

The fall in numbers continued from 4,000 cases down to 3,000 on May 7, when a slight further check was shown May 2 to 7 (*k*) precisely as was the case in the typhus curve. Then to 2,200 on May 17, when another slight check occurred (*l*), then to 1,500 and finally to about 1,600 on June 4—a total fall of 80 per cent. (Table, p. 64).

The curve of admissions (Block A, Chart, p. 102—also Block C, Chart, p. 45) shows that, on the whole, the fall in the case of relapsing fever between April 26 and June 4 was not so regular or so striking as in the case of typhus fever.

TABLE E.

Distribution of Relapsing Fever cases in Hospitals in Army areas (A, B, C, D, E) and *Civilian* (F).

Army area	March 27	April 3	April 26	April 27	May 3	May 17	May 31
1st Army A	1,116	1,043	305	419	412	294	298
2nd ,, 3rd ,, Belgrade B	2,002	1,486	661	687	685	314	512
C	393	414	267	290	216	156	121
D	561	516	336	331	251	144	59
E	384	291	128	125	90	55	32
Total	3,456	3,750	1,696	1,852	1,654	673	1,022
Civilian, F	5,194	4,050	2,112	2,052	1,512	1,447	620
Grand total	8,650	7,800	3,808	3,904	3,166	2,120	1,642
Total of Typhus and Relapsing	16,700	16,013	9,757	10,218	9,429	6,496	4,740

Total Admissions and Deaths (*April* 14 *to May* 31). (Table, p. 64.)

The total admissions in the thirteen days, April 14 to April 26, was 2,874 cases, an average of 221 cases daily. The total subsequent admissions, April 27 to May 31, was—a period of thirty-five days—4,453, an average daily of 127 cases. Altogether the numbers of cases admitted, April 14 to May 31, was 7,327 cases, with 463 deaths. The total completed cases during the period was 11,552 cases representing a mortality of 4 per cent. This case mortality (relatively high for this disease), shows that although much fallen in incidence, it still retained its virulence. (*See* Chart 16, p. 100.)

Summary.

The period April 27 to May 11 was then marked by a definite recurrence of both infections. This was not great in amount having regard to the extent of infection which had existed at the end of March, when the combined total of admissions of typhus and relapsing reached probably (on estimate) a daily average of 2,000 to 2,500 cases. But it was of extreme interest from an epidemiological and ætiological

point of view. For it occurred at a time when the weather conditions were those of summer, whereas the preceding great fall which set in suddenly at the end of March occurred while the conditions were those of winter—and very bad at that. Moreover, it was not due to periodicity—e.g., typhus: for, as I afterwards found, it occurred only in certain areas—chiefly in Area B, in which the previous fall had been most marked (42 per cent.); and it did not occur in the area where the disease was most marked and prevalent (Area A); and relapsing fever chiefly in Area A in which the previous fall had been greatest (71 per cent.).

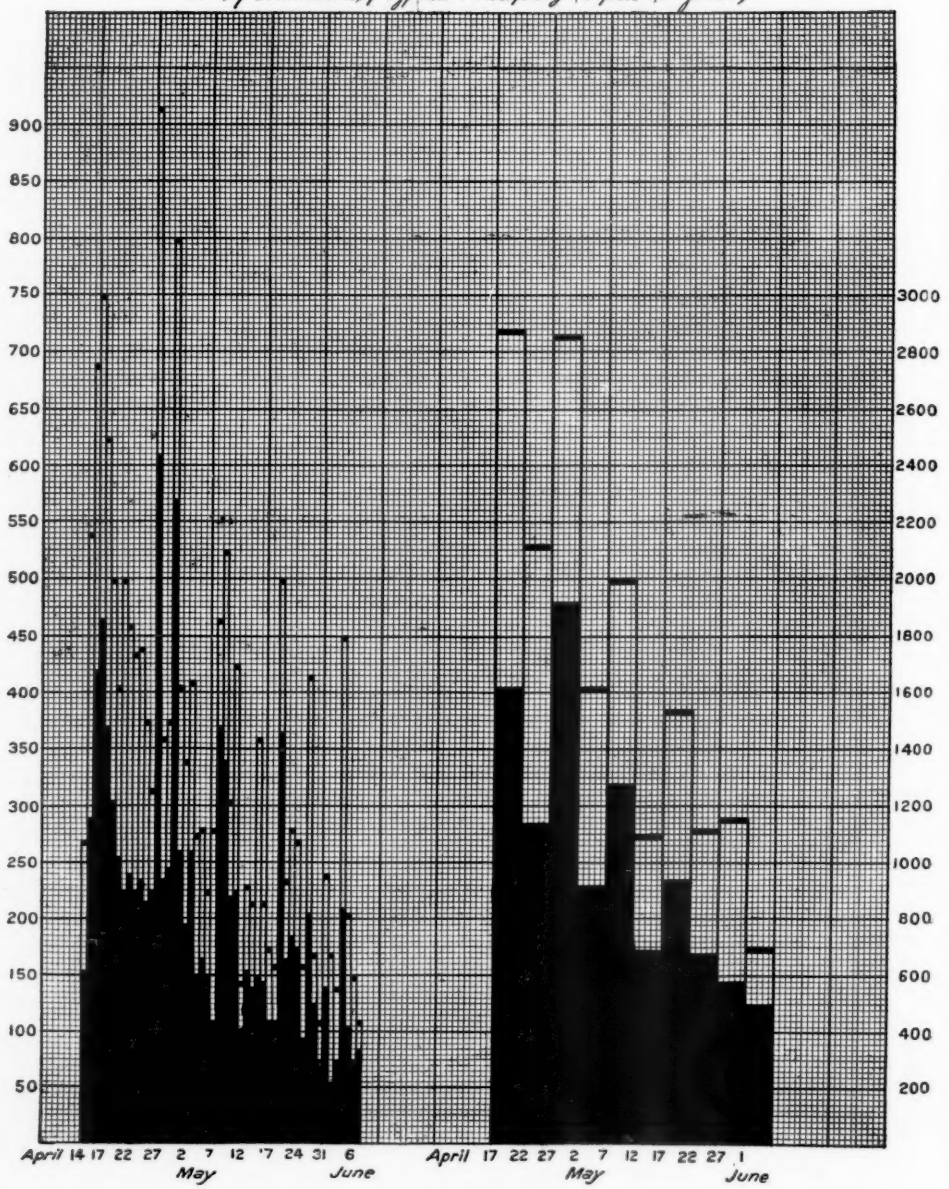
The features of this recurrence have therefore been made by me the subject of a very close study, relating to daily figures of its incidence in forty-two places in Serbia, with results which have proved the most interesting of all my experiences in Serbia from the ætiological point of view; for the result shows that the recurrence was definitely due to, and was in direct relation to, the movements of soldiers and civilians following the resumption of railway traffic on April 16.

Suspension of all passenger railway traffic (only one train up and down Serbia daily) and stoppage of all leave from the armies had been the first measures we advised and urged five days after our arrival in Serbia on March 4 to meet the overwhelming pestilence then prevailing. They had been put in force on March 16—originally for a period of fifteen days up to March 30. But again on our urgent advice ("it is absolutely necessary") in order to give our other chief measures of prevention a chance (viz., formation of our great disinfecting station at Mladenovac and of our special sanitary train for disinfection and inoculation) it had been extended for another fifteen days, viz., to April 15. The original sudden check in the epidemic had set in on March 16 as shown by arrest in numbers of cases in hospitals on March 28, and by rapid fall in numbers of cases in hospitals and in admissions.

Traffic had been resumed on April 16, by which time the number of admissions had fallen (*see* Chart, p. 106) to 707 cases daily. The fall continued for another ten days—down to 317 cases daily—i.e., during a period corresponding to the average incubation period of the disease. Thereafter on April 27, i.e., following the necessary incubation period, the numbers rose suddenly to 915 cases; and there were daily averages (five-day periods) of 570, 400 cases, and then, interrupted by rises, eventually 250 cases daily.

These subsequent rises did not occur over the country as a whole, but were confined to certain areas and places specially affected by movement of soldiers on leave or of civilian traffic.

CHART 17.


No. of Admissions of Typhus + Relapsing. (April 14 - June 4.)
900
850
800
750
700
650
600
550
500
450
400
350
300
250
200
150
100
50
3000
2800
2600
2400
2200
2000
1800
1600
1400
1200
1000
800
600
400
200
April 14 17 22 27 2 7 12 17 24 31 6
May
June
April 17 22 27 2 7 12 17 22 27 1
May
June

Conditions in Railway Stations during Suspension of Traffic.

The chief towns passed through on the way to Nish included Lapovo (the junction for the Kragujevatz branch railway), Jagodina (the centre of railway administration for army areas); Tupria; Parachin (the junction for the Zeitjer, Eastern branch line); Stalac (the junction for the Ujitze, Western branch line; Alexinatz; and then Nish.

From the study of reports which I received nightly I came to be well acquainted with the number of fever cases in all these, and forty-two other, places in Serbia. When on my journeys I was able to take note of the striking change in the appearances presented by all stations passed as compared with those I had observed on my first journey to Kragujevatz (on March 8) after our arrival. The changes were similar in character to those observable in Kragujevatz railway station—viz., an entire absence of travelling public, instead of the presence of crowds of peasants; the cleanliness of the station precincts and buildings; all station woodwork, stonework and waiting rooms whitewashed; and above all the quiescence everywhere prevailing.

This increased and increasing cleanliness was beginning to be, and soon afterwards became, noticeable in the case of the restaurants, very few in number, seen in Kragujevatz and Nish. In Nish, the chief café-restaurant in which we had our first meals on the day of our arrival, on March 4, was then a dirty, ill-kept place, and the same applied to the restaurant in Kragujevatz. From this time onward there began to appear greater efforts of cleansing, whitewashing, distempering, and painting. The result finally was that when it was once more opened to the public, some of its old habitués, it was reported, were so astounded at its brightness and transformation that at first they hesitated to enter it—but only for a moment. The same change was effected in the chief restaurants in Kragujevatz, and doubtless similar changes were carried on elsewhere. For the Serbian is nothing if not a sportsman, and nothing appeals to him more than to outrival, or surpass if possible, the efforts of his neighbours in well doing.

Section IX.—Suspension of Railway Traffic as a Preventive Measure against Epidemic Disease.

(Tables, pp. 58, 62, 78. Charts, pp. 44, 45, 95-98.)

This measure, immediately recognized by us as a great necessity on the first journey we made on March 8, after our arrival in Nish on March 4, and strongly urged, and no less strongly applied by the Serbian authorities, was put in force a week later, March 16, the period of suspension to be for fifteen days. It was due to terminate on March 30, by which time under ordinary circumstances traffic would in all probability have been resumed, to the great relief of all concerned. For the inconveniences caused by the suspension were very great, and felt by all, and by no means least of all by myself in connexion with my work. On our strong representation at a critical period (March 30) the suspension was continued for another fifteen days, with the most important results. The stoppage of leave from the army particularly affected the home living, domesticated peasant soldier folk. Their lot—fighting as they had been almost continuously in three great wars over a period of nearly three years—was indeed one which only intense patriotism and self sacrifice of the Serbian soldier could have endured.

March 16—*April* 16.—*Effect of Suspension of Railway Traffic.*

(Charts, pp. 96, 97; Table, p. 62.)

These Charts show in a most instructive way the course of the epidemic of typhus from April 3, when the number of cases in hospitals were at their height, up to April 26, ten days after traffic was resumed on April 16.

Army Area (*April* 3 *to* 26).—In the first (Valyevo) army area (A), completely shut off from the rest of Serbia by the suspension of railway traffic the epidemic had continued. The number of new cases had equalled the discharges and deaths, and the figures in hospital had kept up to nearly their original highest mark. On April 26 the fall in the number from the highest mark was only 4 per cent., the number of new cases had fallen to 89.

Army Area B.—In the second and third and Belgrade army areas (B), comprising three-fourths or more of the whole armies, through the

middle of which the railway passed through the chief Northern towns, the fall by April 26 in the number of cases was very striking, viz., 42 per cent.; number of new cases (admissions) had fallen to 56.

Army Areas D and E.—In the other two war areas (D and E) the fall was only about the same, 23 and 24 per cent. respectively, viz., D from 1,013 to 771 and E from 571 to 431; number of new cases only 21 and 10 respectively.

Army Area C.—The only group not showing any fall was the small group of four or five railway towns (C) south of the war areas (B). Their numbers in hospital had risen from 216 to 261 (17 per cent.); number of new cases 11. The total percentage fall for all army areas was 27 per cent.

Civilian Area F.—The fall in number of cases in hospitals had been from 2,403 to 1,715—a fall of 28 per cent., and number of new cases daily only 41.

April 27 to May 1.—Effect of Resumption of Railway Traffic.

Railway traffic was resumed and leave from army again permitted on April 16, and during the next week, as I learnt afterwards, the traffic of soldiers returning to their homes was very heavy. The effect was very strikingly shown on April 27 to May 1, exactly ten to twelve days later, and during the next ten days. The number of admissions in all hospitals which had steadily fallen since April 3 down to 228 cases (an average of 230 cases on April 22 to 26), suddenly rose to 611 cases on April 27, and to a daily average of 380 for the next five days (*h*). This was due to a sudden and definite rise in new cases in the war areas, B, C, D, from a daily average of 88 new cases on April 22 to 26 to an average of 205 for next five days; and in the civilian areas F from a daily average of 41 new cases on April 22 to 26 to an average of 120 for next five days (*see* Table, p. 62).

The whole sudden rise might have been due to periodicity of the disease, affecting all parts of Serbia. But if so, it was an extraordinary freak on the part of the disease to wake up once more all in one day.

Area A, First Army.—The area over which such a rise, if due to periodicity, might have been expected was area A, where the disease from the first had been virulent. I found, as a matter of fact, it was the only one where the sudden rise on April 27 to May 1 was absent. Its daily number of new cases April 22 to 26 had been 89, and this fell during the next five days to a daily average of 49 new cases. Periodicity

therefore could not explain the rise. The other possible factor was the passage of soldiers on leave on resumption of railway traffic from the first army area (A) southwards into the other areas; contracting infection during the two or three days of their railway journey to their various destinations in remote parts of Serbia.

Army Area B, Second, Third and Belgrade Armies.—The area immediately adjacent to this first army area (A) was that of the second and third and Belgrade armies in area B, the junction for the two areas being at Mladenovac, which we had selected as the seat of our quarantine and disinfecting station. This was the area most likely to be affected by such a transit of troops on leave. As a matter of fact such proved to be the case, and my first detection of this break through of infection from the first army area was made by me at the very moment (May 1) when it first manifested itself, and I immediately took measures to rectify it by observation of the conditions which I found there on my visit to Mladenovac on May 1, as fully described elsewhere (*vide* p. 119). As has been seen, the fall in the epidemic had been greatest in this area—the number of cases in the hospitals serving it having fallen from 2,789 on April 3 to 1,692 on April 26—a fall of 42 per cent.; and the daily number of new cases admitted on April 26 had been only 56 cases, or 280 in the preceding five days. The reinfection of this area was so definite that the number of new cases (as low as 56 on April 26) suddenly rose to 114 on April 27, and two days later to 417, altogether to a daily average of 151 or a total of 754 cases in the next five days (April 27 to May 1), exactly, be it noted, ten days from the resumption of railway traffic and passage of troops on leave (April 16 to 23). The average daily number of new cases admitted in the following six periods of five days (May 2 to 31) was 33, 98, 54, 60, 60.

The result was that during the next month the number of cases in the hospitals was maintained pretty much at the level obtaining on April 26, falling only from 1,692 cases to 1,437—a total fall of 47 per cent. since April 3, and only a further fall of 5 per cent. since April 26.

This increase of cases in this area after May 11 was due, however to other factors than mere increased incidence. For this area from middle of May became the seat of a great concentration and movement of troops from other areas and evacuation of field ambulance (*see* Chart p. 101) preparatory to advance being made by the armies once more restored to fighting strength. It was also partly due to the important strategic policy which we had laid down from the first, and which was

now being carried out—viz., of breaking the line of infection between the armies in the North and the civilian areas of Serbia, and of retaining as many fever cases as possible in this area B, and not allowing them to be transferred south to other areas. (*See* Chart, p. 46.)

The result finally was that while on April 3 when the epidemic was at its height, this area had 2,789 cases in its hospitals, or 34 per cent. of the total cases in hospital in Serbia; on May 31 it had 1,437 cases or 46 per cent. of the total cases. It was thus holding up 12 per cent. more of the total cases than before, and as will afterwards be seen 5 per cent. of this total 12 per cent.—nearly one half, was being held up in Mladenovac, the seat of our quarantine station. The re-infection also occurred in other war areas south of area B, viz., in the railway towns (C) in the Zeitjer army (D) and the Ujitze army (E).

Army Area C (Railway Towns).—These towns (Jagodina, Parachin, Tupria and Alexinatz) were the chief stations on the railway line south of army area B. The numbers of cases in hospitals were few—between April 3 and 25 they had risen from 216 to 260. The number of new cases on April 26 was eleven. On April 27 this number suddenly rose to 31 cases and for the next five days averaged 21 cases; and the next ten days 24 cases. During the following twenty days (up to May 31) it fell to an average of only 9 cases. The number of cases in hospitals, which had not fallen between April 3 and 26 (had actually risen 17 per cent.), now fell from their previous highest total of 260 on April 25 to 129 on May 31—a fall of 40 per cent.

Army Area D (Zeitjer Army).—This army lay to the east of the main line along the Bulgarian frontier. The number in its hospitals on April 3 had been 1,013 cases, and this had fallen to 771 cases on April 26—a fall of 23 per cent. The number of new cases on April 26 was 21 cases. On April 27 this suddenly rose to 43 cases; and in the following five days (April 7 to May 1) averaged 33 cases, and in the next ten days averaged 24 cases. In the following twenty days it fell to an average of 10 cases. The final result was a fall in the total cases in hospital from 1,013 on April 3 to 174 cases on May 31—a fall of 82 per cent.

Army Area E (Ujitze Army).—This army lay to the west of the main line close to the Bosnian frontier. The number of cases in the hospitals on April 3 was 571; and this had fallen on April 26 to 431 cases—a fall of 24 per cent. The number of new cases admitted on April 26 was 10 cases. No rise occurred here during the first five days (April 27 to May 1), the number of new cases averaging only 9; but a definite rise

occurred in the next five days (May 2 to 6), the daily average being 16 cases—the chief rise occurring on one day (May 4) when 40 cases were admitted. Allowing ten to twelve days for incubation this meant that infection had been contracted about April 25 to 27, i.e., in the second batch of men sent home on their ten days' leave, after resumption of railway traffic on April 16. Thereafter for the next twenty-five days the average daily number of cases admitted was only 5; but on one day (May 25) the number admitted was no fewer than 49 cases, pointing to contraction of infection about May 13. The total number of cases in hospital fell from 571 on April 3 to 187 on May 31—a total fall of 67 per cent.

Civilian Area F.—The rise that occurred here was the most definite and interesting of all. The area referred to extended from the level of Nish down to the south of Serbia. It included hospitals in twenty towns. The number of cases in these hospitals on April 3 was 2,403, and this had fallen to 1,715 cases on April 26, a fall of 28 per cent. The number of new cases on April 26 was only 41. On April 27 this number suddenly rose to no fewer than 327 cases, the number of cases in five days averaging 121. The next five days it averaged 50 cases, the next 61 cases, and the next twenty days it averaged 40 cases or about the same as it was five weeks before. This number of new cases scattered over so wide an area was very small, viz., about two cases a day in each town. The total number of cases in hospital fell from 2,403 on April 3 to 669 on May 31, a fall of 76 per cent. (*See* Chart, p. 97.)

Total Result.—The total result shown in all areas was a definite break through and spread of infection following the resumption of railway traffic on April 16 to 23, manifesting itself by an increase of new cases in nearly all areas ten to twelve days later, viz., on April 27 to May 6. This increase was most marked and earliest shown in Southern Serbia by an admission of no fewer than 327 cases on April 27, instead of a previous admission of 41 cases (punctually to the incubation period), and a total admission of 1,159 cases in the next fifteen days—an average of 77 cases daily instead of a previous and subsequent average of about 40 cases daily. It was also most marked and shown within twelve days' time in the second and third armies (area B) by an admission of 114 new cases on April 27 from an average of 56 cases on April 26, and a total admission of 1,409 cases in the next fifteen days—an average of 94 cases, instead of a previous daily admission of 56 cases, and subsequent average of about 68 cases daily.

Effect of Resumption of Traffic.

The effect of resumption on April 16 was thus marked and sudden, for it was followed by a sharp recurrence. The most interesting points revealed by this recurrence in the case of typhus were the following (Charts, pp. 66, 96, 97).

(1) The persistence of infection in army area A up to that date. The fall in number of typhus cases in hospitals since April 3 had been only 4 per cent., whereas in the adjacent army area B the fall had been no less than 42 per cent. It was this area A that was intended to be and was very successfully cut off from all Serbia by the suspension of railway traffic on March 16.

(2) After the resumption of railway traffic and grant of leave to the troops, the traffic from this remote area A was very great. Fresh infection was thereby conveyed and contracted, and exactly twelve days later (April 27) this was shown by a very sharp sudden rise in number of admissions into hospitals—viz., from a daily average of 230 cases in the previous five days to 611 cases on April 27, and 370 on May 30 —or a total of 1,919 cases for the five days, as compared with 1,151 in the preceding five days. The greater number of these new cases were in civilian Serbia (Chart, p. 97). The outbreak here was quite sharp. The admissions, which had been as low as 41 cases daily, rose suddenly on April 27 to 327 cases, and averaged 150 daily during the next five days—a total of 600 cases. They fell the following five days to 254.

A second recrudescence occurred ten to twelve days later (May 7 to 11), but on this occasion the increase of cases was mostly in the army areas. The explanation of this, as I interpret it, was the following: In the case of men going on leave from the army areas (e.g., A and B) on April 16 to 20 in the north to distant parts of Serbia (e.g., Uskub), the increase of cases if infection were contracted during this journey would be chiefly in the civilian areas most remote. Allowing for an incubation period of twelve days, their illness would fall due about April 27 to May 1. This is exactly what happened. An increase occurred in all army areas at this period (except area A—which was the chief seat of the disease, and the area of the first army, from which most of those on leave were travelling), and it commenced exactly on April 27—i.e., twelve days later, and most of all in the civilian areas (*see* Table, p. 62). In the case of men returning on leave from their homes on the expiration of their ten days' leave (April 26 to 30) the increase of cases, if infection were contracted on their journey, would be chiefly in the

hospitals of the army areas, and this illness would fall due May 7 to 11. This is exactly what happened.

TABLE V.

(A) Effect of Resumption of Traffic on Typhus Admissions (see Table, p. 62; Charts, pp. 66-96).

(a) First Recrudescence.

		Army areas		Civilian		Total
April 26	... ...	187	...	41	...	228
April 27	... ...	284	...	327	...	611
April 28	.. ..	195	...	39	...	234
April 29	... ...	137	...	107	...	244
April 30	.. ..	522	...	48	...	570
May 1	... ...	181	...	79	...	260
	Total	1,319	...	600	...	1,919
	Average	264	...	120	...	384
	(b)					
May 2 to 6	Average	151	...	50	...	201

(c) Second Recrudescence.

May 7	...	...	...	70	...	42	...	112
May 8	...	...	...	340	...	28	...	368
May 9	...	...	...	285	...	58	...	343
May 10	...	...	...	173	...	48	...	221
May 11	...	...	...	74	...	153	...	227
	Total	...	...	942	...	329	...	1,271
	Average	...	...	188	...	66	...	254

(B) Effect on Relapsing Fever Admissions (see Table, p. 64; Chart, p. 67).

The effect was equally marked. The number of admissions on April 26 in all areas had been only 89 cases—viz., 39 in the army areas and 50 in the civilian. On April 27 the total numbers suddenly rose to 304—viz., 176 in the army areas and 128 in the civilian areas. The chief seat of this rise was army area A—viz., 150 cases, and civilian areas F with 128 cases.

SECTION X. — THE RECRUDESCENCE OF INFECTION ON APRIL 27, TWELVE DAYS AFTER RESUMPTION OF RAILWAY TRAFFIC ON APRIL 16.

I had left my headquarters at Kragujevatz on April 26, and was absent between that period to May 2 on visits to Nish, to our inoculating train near Belgrade and our disinfecting station at Mladenovac. At the time I left on April 26 the number of admissions was steadily

falling, viz., down to 228 cases of typhus and 89 cases of relapsing for the whole of Serbia—a most satisfactory state of matters.

During my visit to Mladenovac on May 1, I observed conditions that were extremely unsatisfactory. This was the junction station for the Valyevo line (army area A). The conditions referred to were that large numbers of peasants and refugees were congregated for hours in and around the station waiting for trains; my officers reported that they were very verminous, and that no official arrangements existed for getting them up to our disinfecting station. I took immediate action to remedy these conditions as described in the following. I returned at once to Kragujevatz in the following day to represent to G.H.Q. the extreme importance of carrying out the recommendations I made—for the whole conditions I had observed seemed to me to point to the possibility that infection might once more break through from the Valyevo area (A) into the war area (B) and thence to the southern civilian area—precisely as it had originally done.

The conditions presented to me were an exact duplicate of those seen on my first day of travel north to Kragujevatz on March 8, four days after our arrival in Serbia, viz., soldiers and peasants waiting for hours at railway stations, spending hours in crowded railway carriages, with all the possibilities of conveying infection thereby entailed, and of a possible recrudescence of infection. I did not know at this time (May 1) that any recrudescence had occurred, for everything had been going on right on April 26 when I started on my tour.

On my return to G.H.Q., on May 2, I reported to the chief of the sanitary department what I had seen, and I sent him a report on the subject, on which he took immediate action by telegram, and subsequently by various orders. I then examined the returns for the period April 27 to May 3, during which I had been away, with most informing results. I found that during my week's absence, the fall in number of cases in hospital, which had been going on steadily since April 3 had stopped exactly on April 26, and there had occurred a definite rise April 27 to May 3. The total number of admissions—which had been 228 on April 26, had risen to 611 on April 27, followed by 553 cases on April 30. This increase was specially shown in Belgrade and Mladenovac.

In a further report (communication B of May 7) I submitted to Headquarters the data on this point, showing how infection had once more broken through from army area A into the adjacent army area B, causing increase of cases in that area, specially shown by the increased hospital admissions in Belgrade, Mladenovac and Pojarevatz.

TABLE.—TYPHUS.

Number of Admissions into Hospitals on April 26 and 27 in Different Areas in Serbia, showing Increase of Cases on April 27 to May 3, exactly Twelve Days after Resumption of Railway Traffic on April 16. (Charts, pp. 66, 96; Table, p. 62.)

	April					May		
	26	27	28	29	30	1	2	3
Army area A ...	89	89	38	32	34	36	15	48
,, ,, B ...	56	114	104	59	417	60	24	72
,, ,, C ...	11	31	15	19	18	20	24	30
,, ,, D ...	21	43	39	15	20	60	35	23
,, ,, E ...	10	7	9	12	13	5	18	5
	187	284	195	137	522	181	137	178
Civilian area F ...	41	327	39	107	31	79	55	81
Total ...	228	611	224	244	553	260	192	259

Daily average of admission, April 20 to 26, 235.
,, ,, ,, 27 to May 1, 384.

TABLE.—RELAPSING. (Chart, p. 67; Table, p. 64.)

	April					May		
	26	27	28	29	30	1	2	3
Army area A ...	0	150	30	34	31	25	42	28
,, ,, B ...	10	6	11	47	79	51	4	23
,, ,, C ...	3	20	5	6	2	18	7	26
,, ,, D ...	15	0	12	1	1	0	18	9
,, ,, E ...	14	0	0	4	1	2	1	4
	42	176	58	92	114	96	72	90
Civilian area F ...	47	128	68	41	117	49	72	63
Total ...	89	304	126	133	231	145	144	153

These figures showed (*see* Chart, p. 66) that army area A (Valyevo) was still the seat of the greatest infection of typhus on April 26, viz., 89 admissions, but no increase of typhus cases had there occurred either on April 27 or during the following seven days. The increase was therefore not due to some natural periodic rise. On the other hand the increase was definite in army area B, viz., from 56 admissions on April 26 to 114 on April 27 and 417 on April 30. There was, however, a very marked increase of relapsing in area A, viz., from none on April 26 to 150 on April 27, suggesting that it had been reintroduced into that area by peasants and refugees returning to the Valyevo area (A).

There was the possibility that the increased admissions of typhus into hospitals in area B might be due to evacuations from the field ambulances of the three armies in this area. The data as to the numbers of cases in these field ambulances I had at this time not been

able to obtain, but they were furnished me shortly before I left Serbia (Chart, p. 98). I found on analysing them (Table, p. 99; Charts, pp. 98-101) that no special evacuations of field ambulances had occurred at the period referred to (April 27 to May 1). On the contrary the numbers of cases in the field ambulances had increased at the same period as the numbers in the hospitals (*see* Chart, p. 98).

Any doubt as to the significance of the increase was dispelled by the figures which I found at that period for civilian areas (F)—viz., from Nish to Uskub. There, no field ambulances existed. At the period referred to, the number of admissions of typhus in these areas (F) had fallen on April 26 to as low as 41 cases. On April 27 it rose suddenly to 327, and during the five days April 27 to May 1 it averaged 120 cases (*see* Table, p. 62; Chart, p. 66). The number of cases of admissions of relapsing on April 26 had been 50. It rose on April 27 to 128, and during the following five days averaged 80 cases (Chart, p. 67).

The area which for purposes of my work I have here designated "Civilian areas F" extended from a little north of Nish to the southern Serbian frontier. It comprised some twenty-two towns with hospitals, and over two-thirds of all Serbia.

The Object and Effect of Mladenovac Disinfecting Station.

These were the conditions found as stated on my official visit to Mladenovac on May 1. As the result of resumption of railway traffic, infection had once more broken through from the army area A into other army areas, and also most of all into the civilian areas as already described. The break through was not an incident confined to the adjacent army area B, but definitely extended south into the civilian area F as already shown (Table, p. 62). Lastly, most interesting of all, the further the distance from the army area, the greater was the increase of cases admitted (*see* Map, p. 31).

The civilian area included as its chief towns, Nish in the middle, and Uskub, Monastir and seventeen other towns in southern Serbia. As already stated, the total number of admissions of typhus fever in the whole civilian area (F) on April 26 was 41 cases. This number rose suddenly on April 27 to 327 cases. On April 26, the admissions of relapsing fever in civilian areas had been 50. On April 27 they rose

to 128. This rise, it is to be noted, was much higher, actually and still more proportionately than any increase that had occurred in the war areas adjacent to war area A and to each other.

In other words, the further the distance from the war area, the greater the increase of typhus cases had been. The interpretation appeared to be that the longer the journey involved to soldiers on leave returning to their homes (and the journey from the war areas in the north to Uskub and other places in the south, involved at this period, with stoppages at junctions, at least two or three days or even more in the train) the greater the number of cases contracting the disease and admitted into hospital, twelve to fourteen days later.

The resumption of railway traffic and grant of leave to troops commenced on April 26. The rush of troops for home was great. For the following week, the traffic, as I was informed later by the chief military transport officer, was very great. The trains were crowded, affording all the conditions favourable to infection by contact with each other in crowded carriages. Assuming infection to have been contracted at this time (April 16 to 20) and allowing for an incubation period of twelve days as an average, the cases contracting infection would develop the disease about April 27 to May 1.

The above data show that on the 27th the admissions of typhus rose from 228 to 611 cases, and April 30 to 553 cases; those of relapsing from 89 cases to 304 cases. The daily average admissions of typhus for the five days April 22 to 26 was 230. It rose the following five days to 338 cases. The leave granted was for ten days. The men who left for home between April 16 and 20 were therefore due to journey back to their units approximately about April 26 to May 1. Assuming possibilities of infection being contracted by travelling back to the army areas, and allowing for an incubation period of twelve days, cases infected would develop the disease about May 8 to 14. The figures in Table, p. 62, show that on May 8 to 19 the admissions of typhus into the army areas were 340 and 285 cases. Moreover, most interesting of all on the 9th, no fewer than 162 cases were admitted in area A—the area in which in the preceding twelve days the admissions had been very low.

DIARY RELATING TO AND FOLLOWING VISIT TO MLADENOVAC, MAY 1 TO 10.

April 25 *and* 26.—In Nish, connected with the formation of a Board of Health for Serbia.

April 27.—Nish. Telegram to War Office: "Rapid improvement continues. During past two weeks there has been a fall of 6,186 cases in hospital of the three chief fevers, viz., a fall of 25 per cent. in typhus, 48 per cent. in relapsing and 35 per cent. in enteric."

April 28.—6.30 a.m.: Arrived in Kragujevatz. Found second consignment of cholera vaccines and Malta food stores, 139 cases and seven sacks. Arrangements for taking them that night to Kievo where there were fifteen officers (the train), and Mladenovac where there were three officers. Requisitions received from train for vaccines, food stores, iodine (20 lb.), alcohol (80 pints), spirit stoves (8), boiling vessels (16), needles most urgent to fit 5 c.c. glass syringes required by May 8. No alcohol was obtainable from any shop in Kragujevatz. I obtained some from Arsenal and had the rest commandeered by Serbian officer from a chemist who said he had none. 12 midnight, left for Kievo.

April 29.—Arrived at train, Kievo, at 12.30 p.m.

April 30.—9 a.m.: Visit to Belgrade. 5 p.m.: Returned to train at Kievo.

May 1.—7.30 a.m.: Left for Mladenovac. 11 a.m.: Arrived. 11.30 a.m.: Visited quarantine and disinfecting station with Lieutenants Clements, Cameron and Wall. 1 p.m.: Lunch. 2 p.m.: Called on railway director. 3.30 p.m.: Visited lower camp. 5 p.m.: Interviewed police director. Gave order for area in front of station to be cleaned up. 7 p.m.: Dinner with officers and Colonel Boro-Savlwitch and Colonel Nicholaievitch, with whom afterwards two and a half hours' close conversation.

May 2.—7 a.m.: Visited area in front of station. Found fifty men at work since 4 a.m., and ground levelled, cleaned up and limed. 10.30 a.m.: Saw police director and railway director and station master and thanked and congratulated them on work done. 11 a.m.: Left for Kragujevatz. 6 p.m.: Arrived at Kragujevatz.

May 3.—Report to Sanitary G.H.Q. regarding Mladenovac (communication A). Three interviews that day with chief of sanitary department. Statistics obtained regarding epidemic.

May 4.—Further interviews *re* Mladenovac. Arrangements with architect *re* receiving-room at First Reserve Fever Hospital. Visited Sixth Reserve Fever Hospital.

May 5.—8 a.m.: Inoculation of 670 men. 9 a.m.: Letter to Health Committee, enclosing my plan of receiving-room for First Reserve Hospital. 10-12: Visit to Mrs. Stobart *re* her plan for roadside dispensaries. 2 p.m.: Arrangements made for inoculation of 10,000 troops at Lapovo to commence on May 11. 5 p.m.: Twelve bottles of cholera vaccine given to Colonel Michailovitch. 9 p.m. to 1.30 a.m.: Worked at statistics *re* Mladenovac (communication B, of May 7, showing increase of fever cases, April 27 to May 1, after the resumption of railway traffic on April 16).

May 6.—8 a.m.: Inoculations. 9.30 a.m.: Visit to Stobart Mission *re* plans of roadside dispensary further considered and settled. Telegram sent off to Serbian Relief Fund *re* above. 11.30 a.m.: Telegram to War Office. Further work at statistics relating to Mladenovac.

May 7.—9 a.m.: Communication B sent in *re* spread of infection through Mladenovac. 9.30 a.m.: Visit from Dr. Elsie Inglis just arrived. 10 a.m.: Visit from Mrs. Stobart's doctors. 10.30 a.m.: Letter to Lieutenant-Colonel Stammers on train. 12 noon: Major Antitch (director) called *re* plan for receiving-room First Reserve Hospital. 4 p.m.: Letter to War Office. 5 p.m.: Visit with Major Antitch to Stobart Mission.

May 8.—Statistics relating to Mladenovac and in Serbia (April 27 to May 3). Letter from G.H.Q. ordering that all troops be detained at Mladenovac for twenty-four hours for disinfection, as I had requested in communication A of May 3.

May 9 (*Sunday*).—Statistics.

May 10.—Inoculations in Kragujevatz finished; total 7,229. Arrangements made for four officers to go to Lapovo to-morrow (Tuesday) to inoculate 3,000 troops, also on Wednesday and Friday. Arrangements also made for two officers to go to Pojarevatz to-morrow, south of the Danube, to inoculate about 16,000.

The above decision, taken by G.H.Q. on May 8, in response to my request on May 3, was the most important one that had been taken in connexion with the disinfecting station. Up to that time no definite official orders as to troops passing through Mladenovac being disinfected. The work of the disinfecting station had been limited owing to difficulties in getting people up to the station. This order altered the whole state of matters. The number dealt with each day rose from about 20 to as many as 1,700 in a day and averaged about 800 daily. The disinfecting station was in full action from May 9 onwards.

Section XI.—Arrangements made to Check the Spread.

These were connected with our cleansing station at Mladenovac, and are described in the following communication sent in by me, and received during the period May 3 to June 4, 1915.

Recommendations for Improved Working Arrangements at Mladenovac.

(II) *Quarantine and Disinfection Station at Mladenovac.*

I visited Mladenovac and closely inspected the arrangements and conditions there, and I am of opinion that the work to be done there is of the utmost importance for the health of the armies.

Conditions.—I found the conditions around the station extremely bad, large numbers of refugees and soldiers lying on the ground for many hours daily just outside the station and the whole area of ground close by made extremely dirty and offensive by 250 prisoners who occupy the station sheds a few yards from the station, and my officers reported that the whole of the refugees were in an extremely dirty and verminous condition.

I consider that the return of these people, convalescents and soldiers to the army is a real source of danger to the troops, and that unless the strictest measures be taken to disinfect them before they go through there will certainly be a recrudescence of infection among the troops.

Recommendations.—I therefore recommend as follows, and Colonel Bori-Savlyevitch and the other chiefs of departments there (police, station and railway directors) are in thorough agreement with the recommendations.

(1) That all troops and refugees returning to the front through Mladenovac shall be detained there for twenty-four hours to allow of their inspection and disinfection by my officers at the disinfecting station. (Ordered by G.H.Q., May 6.)

(2) That immediately on arrival of trains, the troops should be marched up under the charge of responsible officers to the disinfecting station, be bathed (in magazine No. 1) and have their clothes disinfected; and that all refugees be taken up to the same camp, and be similarly treated (in magazine 2). (Ordered by G.H.Q., May 6 and officer appointed May 13.)

(3) That no troops or refugees be allowed to return to the neighbourhood of the station till room can be found for them in the trains. (Ordered May 6.)

(4) That wood be provided at once (the disinfecting station was without any wood yesterday) in large quantity to allow disinfection to be carried out from 4 a.m. every morning and for the whole day, by our two large disinfectors and twenty-five barrel disinfectors. (Wood supplied.)

(5) *That all prisoners be forthwith removed* from the sheds close to the station and be lodged elsewhere—the sick in the hospital, the healthy in some place (or in the open fields) above the town away from other people, and that the sheds be thoroughly cleaned out and disinfected under the immediate supervision of my officers, and be reserved as shelters, or waiting rooms. (Ordered by telegram by G.H.Q., May 3.)

(6) That the whole land around the station (now extensively befouled, and with an open sewage ditch running through its middle) be levelled, covered in with fresh earth, and kept thoroughly clean with lime. (At my request the Police Director effected a most striking change between 6 p.m. on the day of my arrival and 10 a.m. the next day), the drain cleaned out, and all refuse of every kind removed or buried. (Commenced at once, May 1, and carried out in a week.)

(7) *Mladenovac from its situation and importance to the health of the field armies should be made the cleanest and best ordered station in Serbia,* and my officers will be only too glad to make it such if they can carry out what I desire. To facilitate their work I have asked for the loan of three tents from

Sir Ralph Paget (pending the arrival of the 150 store tents sent out from England and now in Salonica), and we can then carry out our disinfection work close to the water pumps, as well as in the magazines, and thus save much time (in carrying water up to the magazines).

(8) After their return to their commands I recommend that every company of troops (250 men) should have a barrel disinfector at their disposal, and that disinfection be carried out in a systematic way daily of all soldiers' clothing and bedding.

Lieutenant (Major) Clements, who is in charge, is a sanitary officer with the largest experience, and if he is given a free hand I know no one who is better fitted to carry out what I consider to be a very urgent and very important piece of work for the health of the armies.

On my return to Kragujevatz on May 3 I immediately called at Sanitary H.Q. and reported the result of my visit to the Chief of the Department, who took immediate action by the following telegram :—

To Serbian Authorities in Mladenovac.

May 3, 1915.

The Chief of the English Military Mission, Colonel Hunter, states that the prisoners who work at the station are very badly accommodated in the railway sheds; there are sick cases amongst them and cases of death; that the prisoners mix with the people and with our soldiers. Inform us at once why they are not sent into hospitals, and why the prisoners cannot be sent elsewhere in order that the sheds can be thoroughly disinfected.

At the time I visited Mladenovac on May 1 and made the observations and recommendations with a view to prevent spread of infection, I had been away from my headquarters in Kragujevatz since April 26, and did not know that any infection had recurred. The figures when I left on April 26 were extremely satisfactory: typhus admissions reduced to 228 daily, relapsing fever to 89 daily. On my return on May 3, I found the returns of the previous seven days awaiting me, and I immediately examined them and found that my fears of further spread of infection were well grounded—typhus admissions on April 27 increased to 611 cases; relapsing cases, 1,304. I therefore wrote at once to G.H.Q. as follows, under date May 6:—

COMMUNICATION B.—THE IMPORTANCE OF MLADENOVAC.

Kragujevatz, May 6, 1915.

(1) In my letter to you yesterday regarding the conditions at Mladenovac, I expressed my fear that unless the strictest measures of disinfection were

taken, there was grave risk of infection being conveyed from the Valyevo area, and also from the south into the Belgrade Army areas.

(2) I now beg to report for your information the following figures drawn from the official daily statistics for the week ending April 27 to May 3. (Average daily figures of cases in hospitals: Total number of new cases admitted in the following centres.) (*See* p. 114.)

(3) The foregoing figures (relating to Valyevo, Belgrade, Mladenovac) show that there has been a striking increase of typhus in Mladenovac and Belgrade—the number of admissions in Mladenovac in one week being no fewer than 92 (raising the number in hospital from 38 to 89), and the number of new cases in Belgrade being 108, rising daily, from 9 on April 27, to 28 on May 3.

(4) This increase is without doubt connected with (*a*) the resumption of railway traffic on April 15; (*b*) the passage of refugees and troops on leave through Mladenovac into the Belgrade area—either from the deeply infected Valyevo area or from the South. Since the whole epidemic originally came from that area it is quite possible that it may again be spread from that area—as indeed it has already begun to spread.

(5) In view of these facts, the question arises whether it would not be the wisest and best precaution to stop all passenger traffic of peasants and refugees from the Valyevo area for another period of a month—or during the important period when troops are returning to the Belgrade area. This would cut off at its source the chief supply of infection. I therefore have the honour to recommend this to your consideration. It would greatly simplify the problem of disinfecting all the troops (passing through Mladenovac—either from Valyevo or from the South) during a temporary stay of twenty-four hours in Mladenovac.

(6) It would help most materially if a young Serbian officer could be appointed at Mladenovac to meet all trains of returning soldiers, and carry out the arrangements (described in my previous letter of May 3) connected with their stay, and departure after being disinfected.

Immediate steps were once more taken, as shown in the following reply of May 5, followed by that of May 8:—

Kragujevatz, May 7.

I am in receipt of your letters of May 3 and 7, and thank you for your information about work of your officers in inoculation of troops.

I shall soon also inform you about all orders, which were upon your proposal given to the quarantine station at Mladenovac.

The barrel disinfectors are being prepared in the arsenal—and one portion of them has already been received by Dr. Kopsha for his sanitary train.

May 8, 1915.

I have duly received your letter of May 6 *re* quarantine station in Mladenovac, and have referred the same to Chief of the Headquarters Staff (Colonel Pavlovitch), who has, in consequence, ordered: "That all men

returning from leave or as convalescents be retained at Mladenovac for twenty-four hours to have their clothes disinfected."

I also beg to request you, for information, what has hitherto been done in Mladenovac with regard to the creation of quarantine station, and thus enable me to order anything else wanted.

The order here given was a strong and most important one, exactly meeting the needs of the case. It was important that a young Serbian officer should be appointed as an M.T.O. to direct the movements of troops, and I therefore wrote again (May 9), with following result :—

May 13 (Thursday).

I beg to inform you that by order of Staff of Headquarters dated May 12, it has been decided that Second Lieutenant Milan Provoulovitch be placed at the disposal of the Chief of the British Military Division in order to direct soldiers, and taking care of them at the quarantine station at Mladenovac. Order has been given that this officer be deputed to the City Commander at Mladenovac.

The effect of these developments was immediately shown in the Report asked for and received on May 11 from the O.C. in charge of the station, Captain Clements.

Mladenovac, May 11, 1915.

Sir,—I have the honour to present the following report on the disinfecting and cleaning station established at Mladenovac.

You will remember that it was originally intended to use this place as a true quarantine station for troops returning from leave or after convalescence from illness, detaining them fourteen days after disinfecting and cleansing them. Owing to the large numbers, it was found impossible to deal with all troops in this way, and our plans had to be modified. We now attempt to disinfect and clean all soldiers passing through the railway station, and allow them to proceed forward to their commands on the same day.

The site selected for the disinfecting and cleansing station is within easy distance of the railway station ; two large magazines capable of holding 700 or 800 men have been placed at our disposal. These were at first intended to be used as sleeping accommodation for those in quarantine, and will at any time be available for this purpose should a body of troops require to be placed in quarantine ; but at present they are used as quarters for the staff, and as waiting, bathing and dressing rooms for those undergoing disinfection.

Some difficulty was experienced in getting a supply of water. This has been overcome, and there is now an abundant supply from the two artesian wells situate about 700 yards from the magazines. The carrying of water by buckets is laborious, and is a cause of delay, but as soon as the tents

arrive, which you have secured for us, we will be able to carry out the bathing and disinfection close to the wells, as originally intended.

A staff of eighteen men arrived on April 14, and were occupied for some days in cleaning and whitewashing the magazines, removing a large manure heap, and in surface draining the ground, preparing soakage pits, latrines, &c.

On April 20 the equipment supplied by the Serbian Government, such as baths, towels, &c., were received, and on the same day our own stores were removed from the railway station, and all were arranged in the magazines. On April 22 the station staff were disinfected and cleaned, and from that date the station was ready for the purpose for which it was designed.

Refugees.—For a period of ten or twelve days there were large numbers of refugees passing through Mladenovac towards Belgrade and the Danube: the condition of these people appeared to me to be a danger to the health of those places to which they were returning, and I therefore approached the Commissioner of Police with a view to having them disinfected. At the same time I heard from Colonel Borisavlevitch, who was anxious that something should be done for them, and suggested using some empty sheds adjacent to the railway. I prepared a scheme for dealing with these people, and while this was under consideration the men and children were removed to the magazines and disinfected. During the last week there have been no refugees detained at Mladenovac, and should they come here again it will be easy to deal with large numbers of them with the assistance of our railway disinfecting wagons. In presenting the above scheme for disinfecting refugees, I requested the Commissioner of Police to clean out the open sewer which runs near to the railway station, and also to remove certain latrines used by Austrian prisoners, which were offensive to public decency and most insanitary. The adjoining ground was much fouled with human excreta, and it was requested that this should be removed. Part of this work was commenced by the Commissioner of Police, who has at all times afforded us every assistance, but there was some dispute as to the authority responsible for cleaning these places, and it was not until you, Sir, arrived here on May 1 and made additional and urgent representations that these matters were put right. During your visit you instructed me to see that the Austrian prisoners were disinfected and their sleeping quarters cleaned, and that they should be removed to new quarters away from the railway station.

On May 6 all the Austrian prisoners occupying sheds near to the railway station were disinfected and cleaned, their quarters were thoroughly cleaned, disinfected and whitewashed, all old straw and material that could be burned was dealt with in this way and fresh straw, &c., provided. About thirty prisoners who were ill or so weak as to be unfit for work were transferred to Nish, and the military commandant of the station now informs me that he has made arrangements for the remaining prisoners to be removed to quarters in a more open and less frequented place.

Wood Supply.—Several days' delay has been caused through the want of wood. A good supply of wood is essential, and without it disinfection cannot

be done. Orders have now been issued from headquarters to the local command to supply us with wood, and I do not anticipate any trouble from this cause.

Arrangements for Stopping Troops and Removal to Quarantine Station.—Satisfactory arrangements have, after some delay, been made for the removal of troops passing through Mladenovac to the cleansing station. All troops now proceed from the railway station to the City Commandant's office, from which place they are sent in charge of policemen to the cleansing station. They are all inspected by a medical officer, and if any are found suffering from fever they are transferred to the military field hospital, while those found with physical defects likely to interfere with active service are instructed to report themselves to the medical officer on arrival at their commands; a note is also made on their passports as to the condition found. After disinfection they return to the railway station and proceed forward by the next train available. Occasionally when large numbers arrive late in the afternoon, it is necesssary to detain some of them over night, and sleeping arrangements have been provided for these.

Recruits.—The City Commandant informs me that 15,000 recruits will pass here during the next eight to ten days; such large numbers will probably be more than the disinfecting station under present conditions could deal with. If the tents which have gone by mistake to Belgrade could possibly be obtained before these recruits commence to pass through it would enable us to deal with them more expeditiously. I would recommend that a special messenger be sent to Belgrade to inquire about the tents, as no reply has been sent to my telegrams.

Additional Equipment Required.—(1) The tents already mentioned; (2) three additional boilers for hot water; (3) overalls for the men. (Those engaged in haircutting and handling dirty clothes have on several occasions been infected with lice and fleas, and had to get disinfected again); (4) a heater for supplying steam to the disinfecting wagon now at the railway station. The station authorities cannot spare one of their engines to generate steam. I have this morning telegraphed to the Chief of the Military Railway Inspection Department, Yagodina, and asked for a heater. I am waiting for a reply.

When our equipment is complete and the staff get familiar with the routine of the work, I estimate that we could disinfect from 800 to 1,000 troops a day.

Communication C.—Base Mission Hospital Proposed.

There still remained, however, the necessity of providing increased hospital accommodation at Mladenovac, especially for fever cases; and therefore in forwarding the preceding report I submitted the following proposals on May 17, 1915, and again on May 20 and 25.

I have the honour to submit for your information the report received

from my officer, Lieutenant Clements, who is in charge of the quarantine station at Mladenovac.

(1) The information he gives fully confirms the opinion I have expressed in my letters to you, regarding the importance of this station, in relation not only to the spread of infection northwards among the troops, but also to its conveyance and spread southwards (from Valyevo and adjacent areas) to the civil population.

(2) I heard from Lieutenant Clements by telephone last night that since he sent in his report on May 11 the number of cases (up to that date 1,053) which he had been able to deal with in the previous fortnight has risen to over 6,000 in the last six days—the highest number dealt with in one day being no fewer than 1,700.

(3) He informs me that the need for further and better hospital accommodation is very urgent, as some of the troops have medical and surgical complaints for which a temporary detention is necessary.

(4) I am of opinion that an *additional hospital* there—preferably a tent one—with a *mission attached* could render a great service not only to Mladenovac, but also to the cause of the general health of the army. To enable Lieutenant Clements to carry out disinfection as rapidly as possible I ordered the new wagon disinfector with heater to be sent to him from our sanitary train, and they were duly sent. But he informs me that they have not arrived, but have gone astray somewhere on the railway.

(5) I also learn that the three tents brought down to the station at Skople by my orderly, Sergeant King, whom I specially sent to Skople to see them packed the week before last, have not yet reached Mladenovac, and are reported to be still at Skople. I should be greatly obliged if they could be sent to Mladenovac, where they are urgently needed.

COMMUNICATION D.—*Re* ESTABLISHMENT OF A BASE MISSION HOSPITAL AT MLADENOVAC.

May 20, 1915.

(1) I have the honour to submit for your information a chart which I have prepared showing the number and distribution of cases of typhus at the present date (May 17) in the Northern, Middle, and Southern areas of Serbia; and, in comparison therewith, their number and distribution in the same areas on April 3, when the epidemic was at its height. The chart shows in a graphic way the great improvement that has taken place in the last six weeks: an improvement that commenced ten to twelve days after the railway traffic was suspended, on March 16, and leave from the army stopped.

(2) I understand that all field hospitals have recently been emptied, and the patients sent back to the rear in view of probable contingencies; and the chart shows in a striking way the increasing importance of Mladenovac as a hospital, as the result of this change. The number of typhus cases in

Mladenovac has increased from 71 to 330. In relation to the troops in the Belgrade area, Mladenovac has thus largely taken the place as an important hospital centre, formerly taken by Palanka (the number of cases of typhus in which has fallen from 402 to its present number of 110). (Chart A, p. 46.)

(3) From its position on the railway line at the junction of the Belgrade and Valyevo line, Mladenovac is a station of exceptional importance; and if a good hospital—preferably a tent one—could be established there in charge of an English mission, it would be possible in conjunction with the necessary work of disinfection carried out by the military mission under my charge, to control the whole spread of disease from Valyevo and Belgrade areas. (I think the choice of Pojarevatz, as the seat of work of the new unit of the Scottish Women's mission (under Dr. Alice Hutchison), an exceedingly good one.)

(4) I hear from Dr. Inglis, of the Scottish Women's Hospitals, that she has another tent hospital on the way. If this were established at Mladenovac in charge of her mission it would, in my judgement, render the greatest possible help, and prove of greatest value in checking the spread of infection from the army area southwards.

(5) I have the honour to strongly recommend this for your consideration and acceptance. Mladenovac would then be made the most important quarantine and disinfecting station in Serbia. It would be supplied with:—

(*a*) A well-equipped disinfection station (now in full action), with two large disinfectors, twenty-five barrel disinfectors, and one or two railway van disinfectors, under the immediate charge of English officers.

(*b*) Its existing hospital in one of the magazines for medical and surgical cases (non-infectious) requiring temporary detention.

(*c*) A tent fever hospital under the charge of an English nursing mission.

Kragujevatz, May 25.

Re Mladenovac Quarantine Station.

With reference to my report thereon, and the need for new hospital accommodation, preferably a tent hospital, under the charge of a mission, and my suggestion to you that Dr. Elsie Inglis of the Scottish hospital be asked, as I know she is anxious to undertake this important duty. I beg to inform you that I have instructed Captain Clements at Mladenovac to furnish me with a report on the hospital there.

The official figures as to the number of typhus patients arriving in Mladenovac during the past three weeks show the great importance of establishing a great base hospital for fever cases in that place. I find that the number of new cases arriving in hospital there, during the three weeks April 27 to May 17, has been no fewer than 486, while the number discharged in the same period was 165. No fewer than twenty-four cases arrived on one day, May 8, followed by daily groups of thirteen to forty-five cases in the following week; the number in hospital was no fewer than 332.

On May 18, 157 were discharged, or evacuated by train elsewhere—e.g., the

number admitted into Kragujevatz that day increased by about 100. The transference of typhus cases by train from one place to another must always tend to spread the disease. This could be altogether avoided, if, as already suggested, a great base hospital, for fever cases coming from the army areas, were established at Mladenovac. I have the honour to strongly recommend this to be done, as a matter of great necessity and urgency, not only in relation to the spread of typhus now, but also to the spread of cholera should any outbreak occur during the coming summer.

COMMUNICATION E.—MEMORANDUM ON FINAL ARRANGEMENTS IN MLADENOVAC DISINFECTION STATION.

From Captain Clements. Mladenovac, May 30, 1915.

Hospital Accommodation.

Sir,—Following your instructions, I have the honour to submit a report on the hospital accommodation in (*a*) Arangelovatz, (*b*) Mladenovac.

(*a*) Arangelovatz is a town of about 2,000 inhabitants, situated on the Mladenovac-Valievo railway, and distant about three hours by rail from the former. In the summer months it is a popular health resort; the splendid water, of which there is an unlimited supply, attracting many on account of its supposed medicinal properties. The hotels and houses are clean and attractive looking, the surrounding country is well wooded, and there are numerous paths through the woods provided with seats which render it a desirable place for convalescent patients.

At present there is only one hospital in the town, called the Arangelovatz Reserve Hospital. This comprises a medical and surgical hospital of 460 beds housed in a large hotel which lends itself admirably for hospital purposes, and a short distance away a fever department of 120 beds also housed in a small hotel. Both hospitals appear to be extremely well managed, the wards are well lighted, well ventilated, and every place connected with the hospitals is scrupulously clean. The arrangements for the admission and discharge of patients are also excellent, every precaution being taken to ensure the destruction of vermin and to thoroughly disinfect all garments and infected material.

At the time of my visit there were 300 empty beds in the general hospital, but the staff are ready at any moment to deal with large numbers of wounded or other cases. The staff comprises four doctors, three qualified dispensers, sixty nursing orderlies, and an adequate general staff for cleaning, cooking and laundry purposes, all of which are carried out on the premises.

The epidemic of typhus, which was particularly bad in the area supplying this hospital, is now said to be over. In the fever hospital there are at present only fifty "fever" cases, all in the convalescent stage. When the epidemic was at its height all the available beds, surgical and medical, were devoted to "fevers."

I made a careful inspection of every part of the general and fever hospital, and without going into details, I can say that it is the best administered hospital I have visited in Serbia. The major in charge does not think that further hospital provision is required at present, and this appears to be borne out by the fact that there are a large number of empty beds available. However, the character of the town and its surroundings make it eminently suitable for convalescent or other hospitals, and its proximity to the railway makes transport an easy matter.

(*b*) Mladenovac: There are two hospital units at Mladenovac: (1) The field hospitals at present occupying two of the magazines, but liable to move forward as soon as the divisions of the army to which they belong advance; (2) the Seltzers Reserve Hospital. This latter is a small hospital of 150 beds for medical and surgical cases. It is situated about fifteen minutes from the railway station and on the opposite side of the valley from the magazines. The hospital buildings are permanent structures and are well suited for their purpose. The scope of this hospital could be much extended by erecting hospital tents on the adjoining grounds and utilizing part of the permanent building for administrative purposes.

The field hospitals consist of: (*a*) First Field Hospital, Timok Division; (*b*) First Field Hospital, Morava Division; (*c*) Fourth Field Hospital, Morava Division. They are housed in two of the large magazines, each magazine being divided by wooden partitions into five parts; four parts serve as wards and one as a dispensary. The total capacity of the two magazines is 800 beds, of which 220 are now occupied. Cases of typhus exanthematicus are sent to the First Field Hospital, Morava Division; all other cases, including typhus recurrens, are allocated to the other two field hospitals. Patients are brought to hospital in bullock carts, or special ambulance bullock wagons if these are available, and those transferred by rail to base or convalescent hospitals are similarly removed to the railway station. Previous to discharge, patients are bathed and their clothing disinfected. The beds consist of a straw mattress placed on the floor and two or three blankets. The latrines are unsatisfactory and the arrangements for preparing food and dealing with soiled linen are not all that could be desired. It should, however, be remembered that these are field hospitals and intended only to provide temporary accommodation.

Should these hospitals be removed from Mladenovac, as it is anticipated that they will be in a few days, there will be required a large hospital to take their place and a suitable opportunity would then present itself for improving or removing the drawbacks attached to the present field hospitals.

Postscript.—In connexion with the foregoing account of the developments of Mladenovac at this period—not merely as a great disinfecting station, "the most important in Serbia," as I declared it to be my aim to make it, but also the seat of a base hospital for fever work, and my recommendation of Dr. Elsie Inglis's (No. 6 Reserve) Hospital at Kragujevatz for that purpose—the following references to this move on her part are of interest. They are to be found in "The Life of Dr. Elsie Inglis," by Lady Francis Balfour (Hodder and Stoughton, 1918):—

"So much for what we *have* been doing; but the day before yesterday (May 28) we got our orders for a new bit of work. They are forming a disinfecting centre at Mladenovac, and Colonel Ghentitch, who is the head of the medical service here, wants us to go up there at once with our whole fever staff under canvas. They are giving us tents till ours come out. Typhus is decreasing so much that No. 6 is to be turned into a surgical hospital, and there will be only one infectious disease hospital here. I am so pleased at being asked to do this, for it is part of a big and well thought out scheme.

"The surgical hospital is to remain here. (Dr.) Alice Hutchison goes to Pojarevatz or Valyevo, also for infectious diseases."

Communication F.

Disinfecting and Quarantine Station, Mladenovac.

Objects.—To cleanse and disinfect all soldiers passing through Mladenovac, to examine and detain if necessary those who are ill, referring those requiring hospital treatment to the field or reserve hospital, and to supply temporary dressings and treatment to those requiring it.

Accommodation and Equipment.—Two large magazines, each capable of holding 400 men: Ten store tents with accommodation for 200 to 300 men; three hospital tents, capacity about fifty beds.

Stores.—Axes, 4; buckets, 25; water boilers, 2; lanterns, 24; pickaxes, 12; shovels, 12; saws, 4; soap bars, 300; soft soap, 1 box; soap dishes, 10; scrubbing brushes, 6; two large portable (Thresh) steam disinfectors; twenty barrel disinfectors; baths (long), 10; basins (enamelled), 20; razors, 20; razor strops, 5; jugs, 6; hair cutting scissors, 6; blankets, 200; mosquito netting, 50 yards; bath towels, 400; hammers, 2; kidney dishes, 10.

In addition to the above, there is a large quantity of general stores, consisting of sprayers, liquid formalin, mercurial ointment, string, surgical dressings and appliances, drugs, &c. There is also a heater and a large disinfecting wagon held in reserve at the railway station.

Water Supply.—Water is obtained from two artesian wells which have been sunk on the south side of the station.

Wood is obtained through the divisional headquarters.

Staff.—Three medical officers and twenty-five men. The men sleep and occupy a part of one of the large magazines, they are supplied with food from the adjoining field hospital.

Latrines.—Short shallow trench latrines are provided on the ground at the back of the magazines, also large soakage pits to absorb all waste and bath waters.

Method of Working.—Soldiers are detrained at the railway station and report themselves at the office of the City Commandant, who sends them in charge of policemen to the disinfecting station where they are inspected by one of the medical officers. If any of the men are suffering from an infectious

fever or are too ill to undergo bathing they are transferred to the appropriate hospital, the remaining men pass into the portion of the magazine reserved as a waiting room, and thence in batches of ten to twelve they proceed to the undressing room where they are shaved and have their hair cut and all their clothing is removed for sterilization. After bathing they are given a blanket and proceed to the room set apart for those who are cleansed, and wait there until their clothing is brought to them from the disinfectors. After disinfection and cleansing their passports are stamped and they are allowed to proceed to their commands.

Numbers that can be dealt with.—The station with its present staff is capable of disinfecting and cleansing from 500 to 800 men per day, and during the period in which it has been working some 10,000 men have been disinfected in the way indicated above.

Section XII.—Summary of Programme of Prevention.

Thursday, March 4 to 7.—Arrival of Mission in Nish. Inspection of hospitals. Conferences with Army Sanitary Authorities and with Parliamentary Sanitary Commision. The following seven communications were sent to the Serbian authorities on March 8:—

(*A*) Disinfecting Measures against Typhus and Relapsing Fever.

March 6 to 7.—(1) An appeal to the people of Serbia, "War against Disease. To every Man and Woman in Serbia."

(2) Means to prevent the spread of relapsing and typhus fevers.

(3) A simple, inexpensive, improvised steam disinfector (Stammers' "barrel disinfector").

(*B*) Sanitary Measures against Typhoid, Cholera, and Water Contamination.

(4) Water supply and water for drinking.

(5) Simple methods of purifying water by means of chlorine.

(6) Chlorine method of sterilization of water for troops on active service.

(7) The probable occurrence of cholera in Serbia in future operations.

(8) Telegram home for 300,000 sets of doses of anti-cholera vaccines.

March 8.—Arrival in Kragujevatz, G.H.Q.

These communications were followed by others in quick succession.

(*C*) General Measures relating to Serbia as a whole against Typhus and Relapsing Fever.

Proposed Scheme of Work of the Officers of the Royal Army Medical Corps with the Serbian Army (Communication 8).

March 10.—(1) Suspension of railway traffic.

(2) Stoppage of leave from army.

(3) Quarantine and disinfection station at Mladenovac, to control spread of infection between army and civilian areas.

Decisions regarding the first two schemes were taken at once and put into effect on March 16.

Sanction of Quarantine and Disinfection Station, which was hoped for in a day or two, was delayed and not received till March 22—a delay the results of which on the course of the epidemic from April 17 onwards were very definite, and were controlled by the action of the disinfection station.

Conference with G.H.Q. sanitary authorities at Kragujevatz regarding medical arrangements and needs in Serbian armies.

(From this time onwards I had interviews daily and generally several times daily with Chief of Sanitary Department.)

March 11.—(*a*) Further information obtained regarding numbers and characters of infectious diseases prevalent in Serbia, and also in Kragujevatz hospitals. (Reports of number of cases in hospitals for forty-two towns of Serbia, and for Kragujevatz, were afterwards supplied me every night.)

(*b*) Information obtained regarding wants of hospitals in Serbia. (Telegram to War Office.)

(*c*) Sixteen officers joined up from Nish, followed by remainder about a week later. (Detained by illness of two officers in Nish.)

March 14.—Instructions issued to officers to visit and report on Kragujevatz hospitals.

March 16.—Railway traffic suspended for fifteen days up to March 30. Leave from army stopped.

(*a*) Suspension very thoroughly carried out; only one passenger train in every twenty-four hours for all Serbia (e.g., from Kragujevatz starting at midnight, run during the night).

(*b*) Effect on railway, as seen by me on March 24 and then subsequently in going to and fro, was very marked in regard to quietness; cleansing of all stations and their precincts; cleanliness of railway carriages, only wooden third-class carriages being allowed.

March 22.—"Scheme of Work" (No. 8) sanctioned by Serbian authorities.

March 24.—Communication 10: "Further proposals relating to scheme of work of Royal Army Medical Corps."

(*a*) "An English Inoculation and Disinfecting Train" for use in the army areas.

(*b*) Necessity of continuing suspension of railway traffic strongly urged.

March 27 to *April* 16.—Formation of Disinfecting and Quarantine Station at Mladenovac commenced and completed.

March 30.—Suspension of railway traffic continued for another fortnight, to April 16.

April 2.—Official order given for the formation of an English Disinfecting and Inoculating Train. The work put in hand on April 5 and completed on April 16.

April 6.—Official orders given to supply the various articles asked for in

connexion with the formation of the disinfecting station at Mladenovac. Also expressing thanks for information given that the British Government had sent large supplies of clothes, &c., to Serbia (including 150 store tents).

April 16.—Resumption of railway traffic, and leave from army.

April 17 *to* 19.—Train completed and arrived at Kragujevatz. On the 10th it left for the second and third army areas (Area B) under charge of Lieutenant-Colonel Stammers with fifteen officers. Other disinfecting wagons used by Captain Kopsa on his Serbian train.

(*c*) Effect of suspension of railway traffic on epidemic—especially on typhus—very marked from the first, but not clear for fourteen days (incubation period of cases contracting infection prior to March 16) during which period the numbers in hospitals rose from 5,361 cases to 7,980. During period March 1 to 14, the numbers of typhus cases in hospital had risen from 3,743 to 5,361. During period March 15 to 28 they rose from 5,361 to 7,980, and a week later to 8,213—their maximum. On estimate the number of admissions daily between March 14 and 28 was 1,000 to 1,500 cases daily, the infection contracted before March 16.

The effect first showed itself about April 3, by the number of cases in hospital becoming stationary for five or seven days, indicating a lesser number of new cases, then by a rapid fall in numbers of cases in hospitals indicating a great fall in the epidemic.

Between April 3 and 26 the numbers in hospitals fell from 8,213 cases to 5,979 cases, a fall of 27 per cent. The average number of admissions, April 15 to 19, was 370; this fell to 235, April 20 to 26.

April 27.—Recrudescence of typhus and relapsing fever ten days after resumption of railway traffic. The number of admissions, which had been 317 on April 26 and had averaged 230 between April 22 and 26, rose to 611 on April 27 and averaged 340 cases—April 27 to May 3.

May 1.—Visit by me to Mladenovac; observation of conditions and difficulties there.

May 3.—"*Mladenovac Communication A*," relating to the above conditions and developments immediately required, viz., all troops passing through to be stopped and disinfected. These were carried out at once, making the station able to deal with men up to 1,700 daily.

May 7.—"*Mladenovac Communication B*." Report on state of epidemic, with data showing the increase in the typhus infection in army area B (second and third armies), and also in the south (civilian area F), following the resumption of traffic (April 27 to May 1). The break through of infection was first checked on May 6, and then arrested after May 15. The average daily number of admissions of typhus for the four weeks May 4 to June 1, were 215, 260, 187, 110.

May 7.—*Mladenovac disinfecting station* in fullest working order. All men in transit stopped there for twelve to twenty-four hours to be disinfected. The numbers dealt with averaged about 1,000 men daily. The number dealt with in four weeks was over 10,000; carrying out the purpose for which it was

established of holding up infection passing from the armies to Southern Serbia. The station was left in charge of a Serbian base hospital with personnel on June 4.

March 20.—"*Mladenovac Communication C.*" Further developments recommended for the establishment of two new base hospitals at quarantine station, one for fever work (the Scottish Mission). This was carried out in a week's time.

May 28.—All inoculations completed.

June 1.—Order of recall of Mission to Malta, where services were urgently required.

June 10.—Mission left Kragujevatz.

June 14.—Mission left Salonica for Malta on conclusion of its work.

(*D*) Disinfecting Measures.

Devising of "Barrel Disinfectors" and "Railway Van Disinfectors," "Railway Van Baths," and Special Sanitary Train for Preventive Work.

March 6.—"Barrel disinfector" devised by Lieutenant-Colonel Stammers.

March 9.—The formation of the barrel disinfector put in hand in the arsenal of Kragujevatz: completed on March 11.

March 13.—It was tested and found most satisfactory in No. 3 Reserve Hospital (Scottish Women), at Kragujevatz.

March 18.—Orders were given to have these made in hundreds in the arsenal, at the rate of at least 50 to 100 per week. Subsequently also on March 28 in the railway works at Nish. Orders were subsequently given for the barrel disinfectors to be sent to the Mission hospitals in Kragujevatz, Uskub, Vrynatchka Banja, and Belgrade.

March 14.—*Communication* 4A.—Method of using the improved disinfector (Stammers' barrel disinfector), afterwards known in Serbia as the "English barrel disinfector"; and, later on, in all Eastern war areas where subsequently introduced into use as "the Serbian barrel disinfector."

(*a*) Serbian pamphlets and brochures drawn up describing its use, and the use of other methods of disinfection.

(*b*) Classes of instruction in Kragujevatz were formed, each lasting four days, and made up of fifteen men from regiments and divisions who were practically trained how to use them.

(*c*) The barrel disinfector was taken by me to Nish, and its use demonstrated in the Parliament House and before the Medical Society in Nish; others were made in the railway works, and orders given by the Prime Minister that 100 should be immediately made and sent as samples to the Prefects of the chief towns, with orders to them to form similar ones in each place.

(*d*) There was a widespread use of them in the Serbian armies and generally (and subsequently in British armies in Eastern war areas as described by me in an address on "Prevention and Arrest of Lice-borne Diseases by New Methods of Disinfection." Royal Society of Medicine, July, 1918, *Lancet*, ii, pp. 377, 378).

March 25.—A new type of disinfector devised by Lieutenant-Colonel Stammers (*railway van disinfector*) and permission granted to form one experimentally from a wooden van. While under formation another one was formed by me at Nish (April 6) from a meat van, also a "*railway van douche bath*," devised and formed and at the same time to be used in conjunction with the railway van disinfector, the steam for heating it being supplied by a heating engine coupled between them. Three more similar disinfecting and bath vans formed by order of the Prime Minister.

Sequel.—Subsequent introduction of "barrel disinfectors" and "railway van disinfectors" in Eastern war areas, viz., barrel disinfectors by Lieutenant-Colonel Stammers in Mudros and Salonica, van disinfectors by Colonel Hunter in Egypt and Palestine (February, 1916) (and subsequent widest use in Egypt, Gallipoli, Salonica, Mesopotamia, East Africa, Palestine) from August 15, 1915, during the next two or three years (with results described by me in address, "Prevention and Arrest of Lice-borne Diseases in Eastern War Areas by New Measures of Disinfection," July, 1918. Royal Society of Medicine. *Lancet*, September, 1918).

(*E*) Hospital and Sanitary Measures.

Kragujevatz Hospitals.

March 16 to 27.—Conferences with State Commission for Prevention of Diseases and Kragujevatz Sanitary Authorities. (*a*) Information about municipal arrangements, number of hospitals, notification and isolation. (*b*) Decisions taken to establish compulsory notification, and, so far as possible, isolation of fever cases. To provide additional accommodation; to isolate typhus and relapsing fever cases in separate hospitals. To improve sanitary conditions in hospitals; to provide disinfecting arrangements: all of which were carried out in due course.

March 16.—*Communication* 9. "Proposals for a public bath and disinfecting station in Kragujevatz."

March 20.—Reply from Town Sanitary Authorities *re* establishment of public disinfecting station. Subsequent establishment of a good one dealing with 500 to 1,000 men daily.

March 17.—Visits to, and reports on, hospitals in Kragujevatz.

March 18.—Interview with Chief of Sanitary Department *re* results of above. Subsequent great improvements in sanitary conditions. Provision of two new fever hospitals, Sixth and Seventh Reserve (Scottish Mission). All typhus cases isolated in two hospitals (First and Sixth Reserve). All relapsing cases isolated in one hospital (Seventh Reserve).

March 24.—First visit to Nish to meet Sir Ralph Paget *re* a Foreign Office Commissioner for British Hospital Missions in Serbia, which I strongly urged him to accept, and which he did accept four days later.

March 22.—Reports from Dr. Barrie at Uskub, Mr. Berry and Captain Bennett in charge of Hospital Missions at Uskub and Vrynatchka Banja.

March 28.—Second visit to Nish to Conference of Representatives of British Hospital Missions, four in number, then at work in Serbia, viz., Lady Paget's and Dr. Barrie's Mission at Uskub; Scottish Women's Mission (three hospitals) in Kragujevatz; Dr. and Mrs. James Berry's Private Mission, and Captain Bennett's Red Cross Mission in Vrynatchka Banja.

April 28.—Formation of Board of Health for Serbia under H.R.H. Prince Regent, as President; Sir Ralph Paget, Vice-President; Colonel William Hunter (Army Area), Colonel Joubert (French) (Middle Area), Dr. Strong (American) (Southern Area), Directors.

(*F*) Anti-Cholera Inoculations.

March 7.—300,000 sets of doses of vaccines ordered from War Office.

March 18.—First consignment of 50,000 sets of doses sent off from London. (Arrived in Salonica April 4; sent off on April 9; lost for a week, and reached me at Kragujevatz on April 17, the *day before* our inoculation and disinfecting train arrived.)

March 25.—Proposals for "inoculation and disinfecting train" submitted and sanctioned. (Train ordered April 2, put in hand April 6, and completed April 17. Inoculations commenced April 18.) Second consignment of 50,000 sets of doses of vaccine sent off from London. (Arrived in Salonica April 15; Nish, April 21; and eventually after being lost, found in Kragujevatz on April 28—the day before our first supply had almost been exhausted.)

March 28.—Third consignment of 100,000 sets of doses asked for. (Sent off April 9; arrived May 6.)

April 2 *to* 5.—Inoculation train ordered and put in hand at Nish.

April 17.—Completed and dispatched to Kragujevatz.

April 18.—Left for Second and Third Army Areas with fifteen officers, under charge of Lieutenant-Colonel Stammers.

April 20.—Inoculations commenced on train and from Kragujevatz and carried out on about 250,000 men and completed May 28. On the train the inoculations were carried out with greatest dispatch and rapidity—up to as many as 10,235 men in the course of five hours.

April 23.—Fourth consignment of 100,000 sets of doses asked for. (Sent off on May 10 and reached Salonica May 30.) Total sent, 300,000 sets of doses as originally requested.

May 16.—Fifth consignment of 200,000 sets of doses asked for in sets of 50,000 at a time. (First set sent off on May 26; others to follow at weekly intervals on June 3, 10 and 17. These reached Serbia after departure of Mission on June 10.)

May 28.—Anti-cholera inoculations completed:—

On second and third armies (200,000 men)	200,000
Kragujevatz	7,000
Lapovo, Jagodina, &c., Badynevatz	20,000
Pojarevatz	15,000
Negotin	14,000
	256,000

Total consignments of anti-cholera vaccines requisitioned and immediately dispatched by the War Office were 500,000 sets of doses.

(*G*) Laboratory Work.

March 12.—Laboratory formed in Kragujevatz under Captain W. W. C. Topley, with one officer to assist.

March 13.—Research work on the bacteriology of typhus and routine work carried out till departure of Mission.

June 10.—When laboratory and all its equipment was handed over to the Serbian Authorities (reported on in *Journal of the Royal Army Medical Corps* by Captain Topley, August, 1915).

Epidemic Conditions on Arrival of Mission (March 4) and on Departure of Mission (June 4).

Number of cases in hospital	Typhus	Relapsing
March 1	3,743	7,069
,, 16 (*e*)	6,000	7,883
April 3	8,213	7,810
,, 16 (*f*)	7,324	4,960
June 4	2,860 (*m*)	1,609 (*m*)
Percentage fall, April 3 to June 4	63	80

(*e*) Supension of railway traffic and leave from army.
(*f*) Resumption.
(*m*) Most of these were convalescent.

PART III.

Section XIII.—Position at end of March. An Anxious Period. (*See* pp. 84, 88.)

The second measure we had advised on March 10, viz., the establishment of a *quarantine and cleansing station*, had not come into operation; it had only been sanctioned on the 22nd, by which time we had hoped to have made a start with it. In submitting it on March 10, two days after our arrival at Kragujevatz, we had hoped to have got official sanction and acceptance of it in two or three days, so that, on arrival of officers two days later, I might have been able to get forward a number of them to the war areas and make a start with our quarantine and disinfecting station at Mladenovac, simultaneously with the suspension of railway traffic which was to commence on March 16.

I was informed, indeed, two or three days later that the principle of our proposed scheme of work had been approved of, and I therefore waited expecting every day to hear officially.

Our time during this period of waiting was utilized in ascertaining the facts about the hospitals in Kragujevatz, and dealing in various ways, as I shall afterwards describe, with the problems of notification, isolation, sanitation and disinfection presented by them. But in relation to our chief purpose of getting forward to the army area, especially to Mladenovac to commence our disinfection station there, and to commence also our disinfection and inoculation work amongst the troops there by means of the new barrel disinfectors and, later on, van disinfection specially devised since our arrival, the needs of Kragujevatz were subordinate to our main object. The period of waiting day by day for ten days was, therefore, a most trying one; especially as I saw from the official data supplied to me, that the epidemics were steadily on the increase, and the prospects of starting our disinfection station in time to check them appeared to be steadily lessening.

Our third measure—viz., the formation of a *disinfecting* and *inoculation train*, had only been submitted; while approved, it had not been officially sanctioned. (It was sanctioned on April 3, put in hand on April 5 and completed with great dispatch in about ten days.)

The whole position at this time, as I have said, caused me much anxiety. The instructions I had received for the Mission was to form a programme of work that would control or check the epidemics as a

whole. It had been our aim to effect this result by measures we advised and pressed on the attention, with every degree of urgency, in the series of *communications*, eight in number, submitted to the authorities within seven days of our arrival.

The interval of fourteen days that had elapsed since our arrival in Kragujevatz and the submission (March 10) of our proposals regarding suspension of railway traffic, stoppage of all leave from the armies, and the establishment of our cleansing station at Mladenovac, measured in days—was not long. In relation to the urgency of checking the widespread and increasing epidemics, it seemed to magnify itself into as many weeks. (*See* pp. 84, 89.)

The outlook at this period appeared therefore sufficiently black. The epidemic had steadily increased for twelve days despite the suspension of railway traffic. In the past two or three days, the net increase of typhus cases in hospitals had shown signs of lessening—being only about 50 to 100 a day (March 25 to 28) (an increase which, allowing for discharges and deaths in a total of over 8,000 cases in hospital—represented probably 800 to 1,000 new cases daily) instead of about 400 on March 23-24, and 600 on March 20 and 25. But the possible significance of this was not at that time clear; although lessening, the daily increase was still continuing in spite of twelve days' suspension of railway traffic, and this measure was timed to cease in two days' time, viz., on March 30, having been introduced only fifteen days.

It was at this important period that our communication (No. 8) relating to the formation of the cleansing station at Mladenovac was sanctioned, and our further proposals (No. 10) for forming a sanitary train were sent in. It was there emphasized in the first sentence, that "it was *understood that all leave from the army had been stopped and would remain stopped until the epidemic ceased, or other causes arose making it necessary or advisable to again grant leave*"; that this was a most important and valuable measure since it cut off at one blow the greater part of the infection that had up to this time spread from the rest of Serbia to the army, or from the field army to the rest of Serbia; unless this measure were continued it would be practically impossible to establish a properly conducted disinfecting station owing to the number of troops that would be passing to the field army; and the proposal to establish such a station at Mladenovac was definitely stated to be subject to this condition, viz., continuance of suspension of traffic and leave.

The sequel to this communication was the decision, the very important one, taken by the army authorities, viz., to continue the

suspension of railway traffic for another fortnight. The importance of this decision will be afterwards seen. The sequel showed that the continuance of suspension determined materially the striking arrest of the epidemic which afterwards followed.

In the light of the subsequent result, it appeared to be a natural and wise decision to take. But in view of the circumstances of the country at the time, the many urgent needs felt by the army authorities to resume railway traffic, the many inconveniences to the public caused by the suspension—keenly felt by them as I had occasion to learn subsequently in conversation with Serbian civilians—the decision of the authorities to continue suspension was a very strong one, indicating their sound appreciation of the gravity of the situation, and their resolve to deal with it with a firm hand.

As a mere piece of counsel, our strong recommendation to continue the suspension might or might not have carried weight. Its effect on the epidemic had so far been little if at all apparent, the inconvenience occasioned had been great; and, under the circumstances, had traffic been resumed at this period, it might possibly have been held that a fair trial had been given to it—with little, if any, appreciable result (*see* p. 84).

But, as I have made clear, our recommendation was not merely an academic counsel; it was based primarily on the necessity of giving fair conditions for carrying out the measures of our programme of prevention, namely—the establishment of a disinfecting station at Mladenovac which the army authorities had duly sanctioned (March 22) and the further proposal to supplement this measure by forming a disinfecting and inoculation train—a measure also generally approved of but not at this time (March 27) officially sanctioned. This official sanction only came to me on April 3 in Nish.

The part played by these two proposals in determining the decision of the authorities to continue the suspension of railway traffic was not known to me at the time and only became known to me afterwards. These proposals, it then appeared, had a double effect. On the one side the resumption of railway traffic at this period would have rendered valueless, in a large degree, the establishment of the disinfecting station at Mladenovac which had been officially sanctioned; and, to give this measure a fair chance of success, the suspension of traffic and of leave from the army was therefore continued for another fortnight. On the other side, the urgent need for resuming traffic as soon as possible, was an additional inducement to get both the disinfecting station and the sanitary train into action as soon as possible.

The two measures were intended by us to be not merely of a temporary character, to meet present exigencies. They were of a permanent character, purposely designed, firstly, to meet the future needs of the armies in the event of future contingencies continually before them—viz., first of facing another attack of the Austrians upon their Northern borders, and secondly to face any possible recrudescence of typhus and relapsing fever the following winter. The authorities quickly recognized their importance and potentialities in these two relations, and were induced thereby to throw themselves with characteristic energy into carrying out measures which they realized to be of a practical and practicable nature.

Of the two measures, the formation of the sanitary train for inoculation and disinfection appealed to them strongly. It was an entirely new measure, such as had never been previously introduced into any army. While difficulties of many kinds were in the first instance encountered by my officers in connexion with the formation of the cleansing station at Mladenovac (already referred to, p. 125), the effect of which was to retard its getting into full working order, the railway authorities at Nish, after the train was officially sanctioned on April 3, threw themselves with greatest energy into its formation. The result was that in the short period of ten days (owing to Eastertide, April 2 to 5, the work did not commence until April 6) they turned out a train of fifteen coaches and vans, unique in its character and complete in all respects for the purposes of prevention, for which it was designed. They could have completed it for all purposes in an even shorter period of seven days, so far as my instructions for its details were concerned. But with characteristic pride in workmanship, they determined that not only for its practical purposes, but also in its appearance, the train should be as complete as could be made; and in addition to the interior re-arrangements and fittings I had ordered, they gave the whole train an entirely new and unexpectedly attractive appearance by a new coat of varnish to all its coaches. They worked day and night to get it ready by April 17, the date fixed for the resumption of railway traffic; they attached so much importance to its usefulness, that they were anxious to have it in readiness before traffic was resumed.

The train was thus completed on April 17, and went forward with fifteen of my officers on April 19, three days after railway traffic was resumed. This relation of events appeared at the time to be a happy coincidence. It was not till afterwards that I learnt from one of the

chief railway staff officers concerned that the two events were not mere coincidences, but were directly related to each other—the formation of the sanitary train, having been purposely speeded up to fit in with the resumption of railway traffic, and to meet possible needs for disinfection after its resumption.

Following the acceptance of our scheme of work in communications 8 and 10, I had, along with Lieutenant-Colonel Stammers and three of my other chief officers, an interview with the Chief of the General Staff, Colonel Pavlovitch, who from first to last gave the Mission his strongest support, in which I explained to him the general purpose of our work. The following is a précis of the information submitted :—

STATEMENT TO CHIEF OF STAFF (MARCH 25).

I thank you for your letter, and we are glad that the proposals which we have had the honour to submit to you have received your approval. We have carefully considered how we may most efficiently and quickly carry out plans for—

(1) Protecting the Serbian troops from infection conveyed to them from civil population, and conversely from the armies to the civil areas.

(2) Our proposals for this end are: (*a*) To establish a quarantine station at Mladenovac, the important railway junction immediately behind the Second and Third Armies and the First Army, and make this point the basis of our operations with the army. (*b*) And we have worked out a plan of operations which will enable us, if approved by you, to go forward without delay and (i) inoculate the troops against cholera; (ii) at the same time, we hope, thoroughly disinfect the troops.

(3) Our plan is the following: To form an inoculation and disinfecting train (*a*) carrying sixteen to twenty officers, and fully equipped with our disinfectors and formaldehyde, sprays, syringes, vaccines, &c., with cars for baggage and stores, &c., and with sleeping and dining staff cars for officers, interpreters, and staff. (*b*) A Serbian military officer attached, knowing the disposition of the troops, and the nearest places on the railway line to which these troops, with least disturbance of military dispositions, can most easily be brought. (*c*) To stop at any point on the line where groups of soldiers can be most conveniently brought, and carry out with utmost dispatch their inoculation against cholera, and complete disinfection of their clothes and person if possible. Since at no time can the troops be removed far from the positions they now occupy, it will be necessary for the English doctors to move from their central base (Mladenovac), established on the railway line, to other places on the railway that are within easy reach of the troops. To effect this, certain special wagons, sleeping and living and disinfecting accommodation require to be placed at the disposal of the staff.

(4) The military situation will determine the number of troops who at any period can be spared from one section of the front. The nearer the stations can be to the point on the railway the more men can be spared.

(5) *Leave from the Field Army.*—This is absolutely necessary to be stopped from now until the epidemic ceases.

(6) My senior officers will proceed forward in a day or two to Mladenovac to ascertain the conditions. You state your intention of building new barracks there for 500 men. This may not be necessary, and on this point they would report to you. Please do not build any more new barracks at Mladenovac until our officers have been able to see and report.

(7) The hospital at Mladenovac should be prepared to take all cases of infectious disease discovered in the quarantine camp.

We ask your approval and authority to carry out this, and to authorize the preparation of the railway wagons as improvised disinfectors by the arsenal and railway authorities, and the other accommodation necessary for this train.

April 2 (Good Friday): *Formation of English Sanitary Train.*

From Chief of Sanitary Department, G.H.Q., Kragujevatz, to Colonel Hunter, Nish.

"Inform the English Colonel Hunter that the chief of staff of G.H.Q. has ordered the formation of an English disinfecting and inoculation train asked for by the English doctors. The commander of this train will be the first class cavalry captain Nikola Christitch. He has been ordered with the commander of our sanitary train (No. 5), named Dr. Kopsa, to put himself at the disposal of the railway directorate and the English to form this train. Dr. Kopsa is coming to Nish to-night, and Colonel Hunter should await him."

From Colonel Wm. Hunter to Chief of Sanitary Department, G.H.Q., Kragujevatz.

April 13.

"I beg to acknowledge the receipt of your letter of April 2, No. 12979, conveying to me the orders of the chief of headquarters, dated April 2, with regard to arrangements connected with the formation of the English disinfecting and inoculation train. The delay in replying to your letter is due to the fact that I have been absent in Nish for the last ten days in connexion with the above mentioned train. I have already met there Captain Christitch and Captain Kopsa, who have been officially attached to the train, and I am already much indebted to them for the help they have rendered. All the arrangements are now near completion, and I hope to have the train sent on to me on Wednesday night or Thursday at latest (April 14 or 15). From the time the official orders were issued by the chief of the headquarters, and telegraphed forward to Nish on April 2, the formation of the train has proceeded satisfactorily, interrupted only by the Easter holidays."

Formation and Character of Train.

Having put their heart, and also their energies, into the formation of the disinfection and inoculating train, they also determined that its start from Nish should coincide exactly with the resumption of railway traffic. Before its start it was visited by the Prime Minister and the English Minister (Sir Charles des Graz) and all the other chief Government officials. It duly started off on the evening of April 17 (the day traffic was resumed), and arrived in Kragujevatz on the morning of the 18th (Saturday).

Instead of the number of coaches I had selected for the purposes of the train—viz., three for inoculation, two van disinfection baths (van disinfector and a new bath van), and six for sleeping and living accommodation for English and Serbian officers-in-charge, with three others for stores—about fourteen in all, the train had come up the line along with the ordinary Serbian Red Cross train. The central feature of the whole train was its new van disinfector and the new van bath with heating engine between them, both of which the Serbians had painted white to denote their purpose; and the coaches adapted for inoculation work and for sleeping accommodation of eighteen officers of the staff.

The alterations I had arranged were :—

(1) The removal of the seats and partitions in the third-class coaches adapted for inoculation work and the fixing up of three small tables along one side in each coach as inoculating tables.

(2) The removal of the seats in four third-class coaches and the provision of three stretcher beds for officers in each of the two halves of the coach, with small wall basin for washing for each group of three officers, and with a lavatory and a small douche for each group of six officers.

(3) In one of the coaches the two halves were reserved as the combined office and sleeping-room for each of the two chief staff officers—one half for Lieutenant-Colonel Stammers, the other half similarly for Captain Christitch, the Serbian Commandant and liaison officer of the train. The two chief officers were thus in close relation to each other to conduct the work of the train.

The interiors of the coaches had been painted white, as all the coaches were old, and had been in a very dirty state. The extra touch given by the Serbians was to wash down and give a coat of varnish to the whole of the exterior of the coaches. The whole appearance of the train on arrival was thus one of extreme neatness and cleanness, very different from that of their original condition when selected by me.

Section XIV.—Character and Summary of Mission's Work, March 4 to June 4.

May 17, 1915.

I have just returned from a week's travelling to Nish to attend the opening meeting and constitution of a Board of Health for Serbia; then back to Kragujevatz; then on the following night northwards to Belgrade to visit the inoculating train at successful work near there; then back to Mladenovac to visit and inspect the quarantine and disinfecting stations we have established there, and then back to Kragujevatz on May 2.

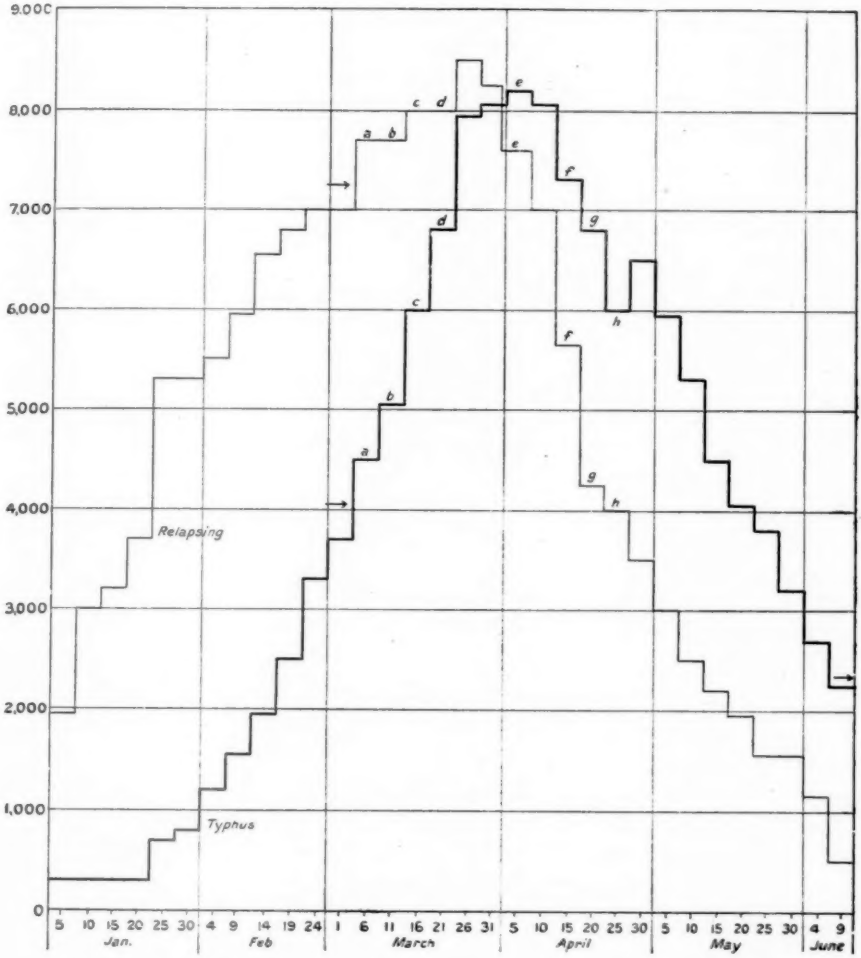
I have kept you informed by my weekly telegrams of the general conditions in the country and the progress of the epidemic; and I have now pleasure in sending herewith three charts which at a glance will show you precisely how matters stand. It is now only a little over two months since we first came here. It seems more like two years for the number of experiences crowded into it. Indeed, I never knew how many experiences of one kind or another could be crowded into one day till I came here.

We arrived here on March 4 at the very darkest period of the epidemic—fever cases increasing by leaps and bounds (*see* Chart, p. 44) and no means of controlling them, and no settled policy except that of getting as many doctors as possible into the hospitals to try and help patients under absolutely impossible conditions. In inviting help of doctors from our Government and others it is certain, I think, that the Serbian authorities had as their chief thought to get further and much needed clinical assistance. The task you assigned us was, however, a different and a more difficult one of endeavouring to check the spread of the epidemic as a whole by administrative measures and by sanitary measures.

The course of the epidemic (shown in Chart appended) shows how effective these measures were. The chief of them were the temporary cessation of railway traffic, suspension of leave from the army, and the improvisation of a method of disinfection which could be at the disposal of everyone. Chart (p. 147) shows the result in a striking way, viz., within a period of ten to twelve days from the suspension of railway traffic on March 15, and the thorough cleansing and disinfection of all stations and rolling stock, the numbers of new cases of typhus began to fall, and continued to rapidly fall so long as the suspension lasted and for ten days afterwards till April 26. Then within a period of ten to twelve days (in both instances this period corresponding with

CHART 4 (*a*).

Course of Epidemics of Typhus and Relapsing Fever in Serbia, 1915, and their Relation to the Preventive Measures adopted.



Number of cases of typhus and relapsing fever in hospital in Serbia on dates specified (five days intervals), January 1 to June 9, 1915.

March 4: Arrival of Mission in Serbia; June 10: Departure of Mission from Serbia. *a*, March 8: Barrel disinfector devised. *b*, March 13: Barrel disinfector made and tested. *c*, March 15: Suspension of railway traffic. *d*, March 22: "Van disinfector" devised ("Van 1"). *e*, April 5, Van disinfector ("Van 2") formed in Nish; "van bath" devised. *f*, April 15: Resumption of railway traffic. *g*, April 19: English sanitary train (inoculation, disinfection, and bath vans). *h*, April 25-30: Sudden check in fall of epidemic ten to fourteen days after resumption of railway traffic.

the incubation period of typhus) the resumption of railway traffic was followed at first by arrest of the fall, and then for a period of two weeks or so by an increase of new cases, with a slight increase in the total of cases.

I wish I could describe to you the change which has now come over the face of the country: the measures of cleanliness now in force in the stations and carriages of the railways and, indeed everywhere—in restaurants and houses; the feelings of hopefulness and happiness that now prevail instead of the gloomy fear that pervaded all classes when we first came. The extraordinary changes in the conditions of the hospitals; their cleanliness and relative comfort and order, instead of the overcrowding and shocking hygienic conditions which at that time prevailed. The changes in this respect I must reserve for a later report. But in the meantime I send you in Report E some information regarding the state of the hospitals in this town when we arrived, and in Chart (p. 87) the notable improvement that has here occurred, affecting not only the number of cases but the conditions of the hospitals and increasing use of disinfecting measures.

Statistics.—When we came here the only figures available were the total number of cases in hospitals each day and their increase or decrease (Table p. 58). When in Nish, forming the special inoculation and disinfecting train (described on p. 145) I pointed out to the Chief of the Sanitary Department that what I wanted was not only the totals but each day: The number in hospital, the number of new cases admitted, the number discharged, the number dying, and the number remaining. He took note of this, and since the middle of April (Table p. 76) these returns are daily officially given for each town in Serbia and are supplied to me daily. They yield most interesting information when closely analysed, as I am in the habit of doing; and some of the results of that interest and analysis will be found recorded in Report B (p. 93), relating to the conditions in Mladenovac, where we have our quarantine and disinfecting stations, and also in Chart (p. 46) which I have made, and in which I show the distribution of typhus cases throughout the whole country and their relation to the railway lines.

Number of Cases of Fever.—To show the full extent of the changed conditions I give here the official figures for typhus, relapsing and enteric fever, when they were at their maximum and as they now (May 16) are:—

	Maximum		Now (May 16)
Typhus	8,192	(April 4) ...	4,529
Relapsing	8,584	(March 25) ...	2,149
Enteric	2,304	(February 25) ...	391

The low incidence of enteric is remarkable having regard to the unhygienic conditions prevailing. But I think an outbreak of this disease is not unlikely later on in the summer, and therefore a good supply of anti-typhoid vaccines now on order will be well timed.

Plague.—You inquire about plague in Salonica. This matter received my immediate attention as soon as I heard of it on April 17. I wrote at once, and the correspondence is given in Report D, herewith enclosed.

Cerebro-spinal Meningitis.—Two or three cases, more or less doubtful, have occurred in Kragujevatz and have been carefully investigated in our bacteriological laboratory by Captain Topley. One of the cases is in one of the Sisters of the Scottish Women's Hospital. In relation to this disease, and also to typhus, they seem to have had extremly bad fortune, no fewer than one doctor and three sisters being recently down with typhus; all of them happily recovered.

Bacteriological Laboratory.—In this connexion I draw your attention to the work of this laboratory, described in Report C herewith sent. Captain Topley and his junior, Lieutenant McCall Smith, have made an excellent laboratory and are continuously engaged in work. The results foreshadowed promise to be of a most interesting kind, but it is too early to speak of them.

Inoculations against Cholera.—After many delays and disappointments our disinfecting and inoculating train, which I formed in Nish after Easter time, was duly completed, dispatched from Nish (after being visited by Mr. Paschitch, the Prime Minister, the English Minister, and many others), and reached here on Sunday, April 18; and, by a piece of great good luck, our cholera vaccines, which had taken from two to three weeks to come up from Salonica, arrived here on the day before. So our train was complete, and, after a farewell dinner on board before departure, it left on Monday, April 19, for Kievo (*see* map, p. 31), a station some ten miles south of Belgrade, to begin its work among the Second and Third Armies, under Lieutenant-Colonel Stammers, in charge, and Captain Christitch, as military commander of the train, and Captain Kopsha, representing the Serbian Sanitary Corps. I visited the train at Kievo about a fortnight later (*see* p. 119) and found everything exceedingly practicable and comfortable. It has enabled the work of inoculation to be carried on with the greatest dispatch. It has recently moved down the line to Ralya and Vlashka. The total number of inoculations carried out up to date is 109,261, carried out by fifteen officers, who have not only been in the train but have been stationed in adjacent areas, on 74,841 men, subsequently increased to about

150,000 men. In addition, with the four officers I have retained here at headquarters, we have made here a total up to date of some 25,000 inoculations in 20,270 men, viz. :—

	Inoculations	
Kragujevatz garrison	9,473	
Laporo, Badynvatz, Brzan, Radja ...	11,000	
Pojarevatz	4,200	(subsequently 10,000)
	24,673,	on 20,270 men

All officers are now away engaged in inoculating.

I have endeavoured, in the foregoing and in the Reports A-E and Chart, p. 147, which I now send to give you some, however incomplete, picture of some of the matters which, from first to last, have unceasingly engaged our attention.

British Military Hospital with the Serbian Armies.

Reports submitted by Colonel William Hunter, A.M.S. :—

May 17, 1915.

Contents.

Report A.—Correspondence between Headquarters Staff of the Serbian Army and Chief of British Mission relating to programme of work submitted by the Mission.

Report B.—Communications regarding the work of the Quarantine and Disinfecting Stations established by the Mission at Mladenovac (*see* p. 120).

Report C.—Work of Bacteriological Laboratory.

Report D.—Correspondence relating to the occurrence and prevention of plague in Salonica.

Report E.—The conditions in the hospitals at Kragujevatz at the time of arrival of British Misssion : with Chart, p. 87.

Chart 1.—Map of Serbia showing railways and chief towns, and numbers of cases of typhus in hospital in each town on May 15, 1915 (Charts, pp. 46, 47).

Chart 2.—Showing number of cases of typhus, relapsing and enteric fever in hospitals in Serbia, January 2 to May 14, 1915 (Charts, pp. 44, 45, 95, 102, with Tables, pp. 58, 76).

Chart 3.—Showing the number of cases of infectious diseases, typhus and relapsing fever, in Kragujevatz, March 10 to May 14, 1915, and increasing interest in the use of measures of disinfection (Chart, p. 87).

British Military Hospital, Serbia. Report A.

Letters from the Headquarters Staff of Serbian Army in reply to the programme of work submitted to them by the British Military Hospital attached to Serbian Armies.

Programme.—Submitted March 10, 1915; accepted March 22, 1915. ("Communication No. 8" already sent in to the War Office.)

Details of Train Programme.—Submitted March 27, 1915; orders for inoculating and disinfecting train issued April 2, 1915.

Train arrived at Kragujevatz, April 18, 1915, and left following night for Kievo, near Belgrade.

May 17, 1915.

(Signed) WILLIAM HUNTER, Colonel, A.M.S.

Course of Combined Epidemics of Typhus and Relapsing (*Chart, p.* 44), *as shown by Numbers of Cases in Hospitals* (*January* 1 *to June* 4).

Period 1.—January and February.
Period 2.—March.
Period 3.—April.
Period 4.—May.

The course as shown by numbers of cases in hospitals and other data was the following: (Charts, pp. 44, 45).

Period 1.—It commenced about the middle of December—following the defeat of the Austrians as already described between December 2 and 13, 1914. Prior to this period, according to the information supplied me, the Serbian forces had been very free from epidemic diseases. [This is supported by data of mortality from typhus, Kragujevatz area, which I afterwards obtained (*see* Chart, p. 86) extending back to August 14, 1914. During August and September and up to October 21, no deaths from typhus were recorded; in November nineteen, and in December thirty cases (fourteen cases in the first half, and sixteen cases in the second half). In January the number of deaths rose to 126; in February to 554, and in March to 581]. By the middle of January the number of cases in hospital had risen to 4,000 cases, and thereafter its course in periods of fifteen days was the following: In the last fifteen days of January, the number rose to 6,000; in the first fifteen days of February to 8,500, in the last fifteen days to 11,000.

Period 2.—It was at this date (March 3) that the English Mission arrived, and for the next fifteen days of March the number of cases rose to 14,000 (March 18), and eventually reached a maximum of 16,500 cases, ten days later, viz., on March 28. The combined epidemics reached their maximum of cases in hospitals on March 28, the rise in numbers of cases being greatest in the last ten days, March 18 to 28, viz., from 14,300 cases up to 16,500. Allowing for an average incubation period of about ten days, the rise in cases in hospital denoted a maximum contraction of infection in the ten days March 8 to 18.

Period 3.—After attaining their maximum on March 28, the numbers in hospitals remained fairly level at about 16,000 cases for a period of about ten days till April 7, indicating that the number of admissions only equalled the discharges and deaths instead of largely exceeding them. Thereafter the number fell in the most rapid manner—from nearly 16,000 on April 3 to below 10,000 on April 26, a total fall of 39 per cent. in twenty-three days. This degree of fall represented a very marked fall in numbers of admissions of cases. Between April 19 and 26 the daily combined numbers of admissions fell from 750 cases down to 317 cases in seven days.

Period 4.—Thereafter the numbers in hospitals fell from this number of 10,000 on April 26 to 3,460 on June 4, the daily number of admissions falling from the figure of 317 on April 26 to one of 185 on June 4. The fall in this latter period of forty days was then less marked and more gradual than in the preceding period of twenty-three days; or in the immediately preceding period of ten days.

The total fall between April 3 and June 4 was 78 per cent.

Recurrence of Infection.

The cause of this was a sharp recurrence of infection which manifested itself by a sudden rise in the number of cases in hospitals on April 27, and by a very sharp rise in the number of admissions (best shown in typhus curve, Chart p. 45). On April 26 (Chart p. 106) the number of admissions had fallen to 317 cases. On April 27 the numbers rose to 915 cases, and during the next five days (April 27 to May 1) they averaged 570 cases. In the following two days they fell again to an average of 326, but in the next five days (May 7 to 11), i.e., ten days after the original rise, they rose again to an average of 400 cases (May 7 to 12). They then fell once more to an average of 240; but rose (May 17 to 22) again to an average of 270 daily, and eventually (May 27 to 31) they averaged 250 cases daily. Apart from rises on particular days, the lower curve of fall, as shown over periods of five days, was from 300 cases daily to 100 daily.

On my urgent representation on May 2 (the exact period of the recurrence of infection, but before I knew of its actual recurrence) the recrudescence was again limited and controlled by our disinfecting station at Mladenovac—the work of which from this period up to the end of May became greatly developed (p. 121).

Section XV.—Final Communication to Chief of Sanitary Department, General Head Quarters, Serbia.

From Colonel William Hunter to Colonel Ghentchitch.

Kragujevatz, June 3, 1915.

Dear Colonel Ghentchitch,—I have the honour now of conveying to you by letter the information which, immediately on its receipt, I at once communicated to you personally on June 1. On that date I received an urgent message from the War Office in England ordering me with the Royal Army Medical Corps Mission under my charge to proceed at once to Malta where our services are urgently required.

(I) (1) In communicating this order to you, I confess I do so with mixed feelings. The first feeling is one of regret, that it terminates for the time being the great and honourable duty which I have had the satisfaction of discharging since our arrival in this country, viz., of co-operating with you in every way in my power in measures to check the great epidemic now happily ending. This feeling is only relieved by one of extreme satisfaction, that the present condition of health of the army and country, of which I have kept my Government fully informed since my arrival on March 4, renders our help in the opinion of our Government less urgent than it was at the time (middle of February) when we were at a few hours' notice dispatched here officially. If we have succeeded by any measures we have taken or any counsels we have given in contributing to this happy result, it will always remain a permanent source of satisfaction to us, particularly to myself, since I have had a most intense and absorbing desire to help in every way in my power.

(II) In view of the urgency of that call and the necessity it entails for our early departure, it is a matter of great satisfaction to me that it is possible for us to leave without any material alteration of the lines on which we have carried out our work.

(1) The problem of providing a simple and efficient method of disinfection available for every one and for railway purposes, has been solved, *not only now, but for all time.*

(2) The preventive anti-cholera inoculations (which, thanks to your provision of our special disinfecting and inoculating train, we have been able to carry out on some 300,000 men, at the rate of 10-15,000 men a day—and have carried out in the army with the vaccines we had prepared especially for Serbia from strains of cholera bacilli prevalent in Galicia) can be successfully continued by your own officers with the 300,000 to 350,000 doses of vaccine on the way from England, which I have had the pleasure in a former communication of placing at your disposal (No. 92, date May 29).

(3) The preventive inoculation against typhus abdominalis can similarly be commenced and continued as opportunity presents, with the 70,000 doses of anti-typhoid vaccine which I have now here, and are at your disposal.

(4) Lastly, the choice of Mladenovac by us as a town particularly well

adapted by its situation for the purposes of a great disinfection and cleansing station (to cut off infection coming from the armies to the general population or vice versa) has proved a wise one. It has during the past week received an important extension, which will add greatly to its value and efficiency, by decisions taken at your hands, with my strongest approval and support (*see* p. 127). These decisions are: (1) The establishment at Mladenovac of two large base hospitals, viz.: (*a*) the one transferred from Parachin; (*b*) the choice of that town as the seat of the Scottish Fever Mission work of Dr. Inglis, with complete tent equipment for 400 beds. (2) The choice of Valyevo and the dispatch thither to-day of another great Unit of the Scottish Fever Mission, also completely equipped with tents. The fullest provision has thus happily, and I think most judiciously, been made for: (*a*) the continuance of disinfecting and cleansing station at Mladenovac by handing over the work of that station to the Serbian personnel of the Serbia Fever Unit which you have transferred from Parachin. (*b*) Ample hospital fever accommodation (in Valyevo, Belgrade and Mladenovac) for fever cases occurring in these important areas. (*c*) The effective prevention of any spread of infection south of Mladenovac.

For purposes of disinfection we leave our two large disinfectors with twenty-five additional barrel disinfectors. For purposes of isolation and hospital equipment, thirty large English store tents are available, accommodating 600 patients.

(III) For purposes of bacteriology I have pleasure of placing at your disposal the equipment of our bacteriological laboratory, and I may take this opportunity of adding for your information that Captain Topley, who has had charge of the laboratory, has been successful in isolating a special organism present in all cases of typhus exanthematicus, and not found in any other disease. This discovery, if followed up and confirmed by further investigations on his return, will itself be a most important outcome of our work here.

(IV) Lastly, I have pleasure in handing over to you a large store of formaldehyde and other material which I hope may prove of value to the hospitals, specially those in Kragujevatz, in which I have taken a very close interest.

(V) I shall have the honour of communicating with you at an early date regarding the arrangements for our departure, which, I regret, must be very soon.

(VI) I desire, in concluding this communication, to express on behalf of myself and all my officers, our deep sense and appreciation of the numerous tokens of welcome and consideration which we have received at the hands of so many during our sojourn here. I desire particularly to thank you, Sir, and all the staff of your department, for the great courtesy and consideration which you have always extended to me personally in the many interviews and conversations which we have had during my stay here. I desire further to express my deep sense of indebtedness to Colonel Bojidarevitch and to Lieut.-Colonel Deroc of the Arsenal, for the great interest they took from the

outset in the construction of the Barrel Disinfector devised by us a few days after our arrival in Kragujevatz; and for the invaluable help which, in my judgment, they have rendered by the continual energy they have shown in providing such a large output of these most important apparatus. To Captain Christitch, who has commanded the English sanitary train, we owe a special debt of gratitude. Without his able and ready assistance at all times and under all circumstances, it would not have been possible to carry out a work we have done in inoculating against cholera. His services have simply been invaluable to our Mission, and, we consider, also to the army whose interest he has so closely at heart. I would also finally express my deep personal sense of the important services rendered to us and to the sanitary services on the railway by Captain Kopsha, who has helped so energetically and enthusiastically in all the matters relating to the work of disinfection and inoculation.

I have the honour to remain, Sir,
Your obedient servant,
Kragujevatz, June 3, 1915. WILLIAM HUNTER, Colonel, A.M.S.

The State Commission for the Prevention of
Infective Diseases, No. 2120, Nish.
May 26, 1915 (June 8, 1915).

To Colonel Dr. Hunter, Chief of British Military Sanitary Mission.

At the moment that you and your colleagues—doctors of the British Mission, are starting on your way back to your beautiful country, the State Commission for the Prevention of Infective Diseases, has the honour to express its most hearty thanks for your conscientious, devoted and useful work in the service of Serbia—and Serbian nation—in the hard times when infection was destroying this nation, already hardly tried and worn out by this war. Receive, Gentlemen, in the name of the whole Serbian nation, the greatest thanks. Glory and honour to your noble country, that you have represented amongst us so agreeably, usefully, and with so much dignity.

(Signed) The President VELISLAV VOULOTITCH,
Ex-Minister and National Representative (M.P.).

Members: Inspector of Military Sanitation, Colonel Dr. Sondermeyer.
Dr. Drag. Pavlovitch, M.P., Professor of Belgrade University.
Miloye Johannovitch, M.P., Director of the State Bank of Credit.

General Headquarters, Sanitary Department,
No. 1896.

To the Chief of the British Military Sanitary Mission.

I have received your letter of June 3, and I thank you for the kind thoughts in it. Your departure and that of your officers has been so unexpected that it is not possible for me at the present moment to say all I would have wanted

to say. I ask you, therefore, to allow me to thank you and your officers in a few words only for all you have done for the Serbian army and nation. I regret that your useful work cannot be continued, and the friendship that arose between your and our officers could not be developed further. Your Government has recalled you to give you new duties more important than the work that has still to be done in this country. Though you and your Mission have worked for a short time only, extraordinary results have been obtained. The help your Mission has given us in every way, under your experienced leadership, will be put at the head of all foreign aid we have had in this war. Wishing you and your officers a happy voyage, I thank you heartily for all the materials you brought down with you and that you have left us, and I thank you also for all the materials that have been sent to us from England at your request.

The Chief of the Sanitary Department of the
General Headquarters,
Colonel Dr. LAZAR GHENTCHITCH.

Kragujevatz, May 25, 1915 (June 7).

To the Chief of the British Military Sanitary Mission in Serbia,
Colonel W. Hunter.

Before all, please accept my deepest thanks for the numberless kindly attentions which you, as also all gentlemen of your Mission, have extended to me. From every side, and from the side of the Chief of our Sanitary Department, the remarkable activity of your Mission for the good of our Fatherland has been recognized and acknowledged. I should like now further to assure you that great as was our joy when we heard of your coming, and we could greet you with us, double as great are the feelings of our gratitude, now that we take farewell of your Mission. I should like to mention two special grounds for our joy and gratitude :—

An old proverb says: True friends are recognized in need. Now, you came to help us when our need was endless great, and have thereby shown us true friendship.

A second more important ground for our joy and gratitude is the following: The unfavourable geographical and political position of our Fatherland has brought it about that the great British nation, up to the time of the present world war, has only seen us through spectacles which were made in Austria, Hungary, and Germany, and allowed us always to appear in the most hateful light. Now, through your visit, the moment has come when these spectacles are for ever broken. You have seen our people as in reality they are, and for that we are both joyful and thankful to you.

We have always known and esteemed the great British nation, now we have learnt to love her. Please be the interpreter of this feeling to your

nation. I hope that it will find a response with her, and wish that the reciprocity of this feeling may form the basis of a sincere and lasting friendship between our nations.

Yours sincerely and attached and grateful,

Dr. MILUTIN KOPSHA,

Sanitats Captain and Commander of No. 5. Serbian Sanitary Train for Disinfection.

Nish, May 27, 1915 (June 9).

ACKNOWLEDGEMENTS.

With these final kindly tributes paid to its efforts and results of its work in Serbia, the Mission left Kragujevatz on June 10 and Salonica on June 14 for Malta, where the services of its officers were urgently required for the hospital needs created by the Dardanelles operations. If, as stated, its efforts had been successful, this was in a special measure due to the kindly welcome and warm support given to it by the Serbian authorities from first to last; it was also largely due to the military experience, sanitary knowledge, ingenuity in devising new and simple and practical and pre-eminently successful methods of disinfection of Lieutenant-Colonel G. E. F. Stammers, R.A.M.C., its chief staff officer. From the first, in my official reports, it was a pleasure to me to draw attention to the special character of his services. And looking back on the work of the Mission and its results, I feel more than ever how valuable they were in attaining the object of the task allotted to us—on such short notice—of restoring the Serbian armies to military efficiency by freeing them and the sorely stricken Serbian people from the overwhelming epidemics that hampered their action and added so terribly to their previous sufferings and losses. From all the other officers, the list of whom I here enclose, I likewise received strongest and warmest support; and I bear with me as one of my chief memories of our experiences—amid the rough and trying conditions of our life we had to meet on our arrival and for some weeks subsequently—the warm personal feelings of friendship I formed for all the officers constituting the Mission. Two of them fell ill within forty-eight hours of our arrival (Lieutenants Shorten and McFadden), and had to be invalided home four weeks later. Three of them (Captains R. H. Spittall, C. G. Smith, and S. E. McClutchey) were afterwards killed on the battlefields in France, and the others continued their work in various war areas of the East, where I afterwards had occasion to meet them.

Corps of Officers.

The Corps of Officers of the Mission was the following: Colonel Wm. Hunter, A.M.S., O.C.; Major (later Lieutenant-Colonel) G. E. F. Stammers, R.A.M.C. (Sanat.); Captain W. W. C. Topley, R.A.M.C. (Bacteriologist); Lieutenant F. F. Brown, R.A.M.C.; Lieutenant J. M. Clements, R.A.M.C.; Lieutenant B. C. Ewens, R.A.M.C.: Lieutenant C. M. Foster, R.A.M.C.; Lieutenant L. W. Forbes, R.A.M.C.; Lieutenant J. M. Hill, R.A.M.C.; Lieutenant S. W. Lund, R.A.M.C. (Special Reserve); Lieutenant S. E. McClutchey, R.A.M.C.; Lieutenant J. McFadden, R.A.M.C.; Lieutenant B. McCall-Smith, R.A.M.C.; Lieutenant C. R. Nicholson, R.A.M.C.; Lieutenant F. H. Ravenhill, R.A.M.C.; Lieutenant H. Y. Riddell, R.A.M.C.; Lieutenant P. J. Seccombe, R.A.M.C.; Lieutenant J. H. V. Scott, R.A.M.C.; Lieutenant W. W. Shorten, R.A.M.C.; Lieutenant R. H. Spittall, R.A.M.C.; Lieutenant C. G. Smith, R.A.M.C.; Lieutenant E. S. Walls, R.A.M.C. (Special Reserve); Lieutenant G. Whittington, R.A.M.C.; Lieutenant W. M. Will, R.A.M.C.; Lieutenant J. S. Williamson, R.A.M.C.; Lieutenant A. L. Walker, R.A.M.C., and Major Cox, R.A.M.C., who joined us from Shanghai two days before our recall.

Section of Epidemiology and State Medicine.

President—Dr. E. W. Goodall, O.B.E.

First Principles: and Epidemiology.[1]

By F. G. Crookshank, M.D.

Physicians nowadays, even epidemiologists, are, as a rule, content to avoid discussion of first principles and of general notions; they seem to hold, with John Hunter, that definitions are, of all things, most damnable. Yet undoubtedly it is largely for lack of such discussion and of attempt at definition that high medical debate is so often profitless; and that the dry bones of periodical literature rattle in the catacombs of our libraries. So long as we are content, both in thought and in word, to confound names, notions and happenings: so long will observation, however accurate, and experiment, however ingenious, fail to advance greatly our comprehension of the problems that beset us. Nevertheless, the physician who boasts himself as practical, and the bacteriologist who esteems himself scientific, do not hesitate to fletch the blunt arrows of their wit, against him who strives to appreciate fundamentals: they contemn him for a metaphysical trifler, a mere juggler with words. The right use of words, however, is a necessary part of the technique of every thinker: and, to the thinker, is no less important than the test-tube, the platinum loop, the formula, or the graph, to their respective and respectable devotees. Moreover, as the late Dr. Mercier once said, with all his accustomed point: "In every calling . . . a time must at length come when a definition of its fundamental concepts is needed . . . Such a time is now arrived in the history of medicine" (*Science Progress*, 1916, xlii, p. 229).

It is indeed true that no calling is really scientific if its exercise be not based upon the acknowledgment and definition of fundamental concepts; and, when we cease to confound occurrences with names and

[1] At a meeting of the Section, held January 23, 1920.

notions, but not till then, we shall no longer waste time in vain discussion as to whether *this* disease is *that*, or something new, and shall not continue to reckon the first happenings of a certain nature from the year in which a new conception arose, a new appellation was devised, or a new form of notification received sanction.

(I) What is a Disease?

Under the influence of certain schools of thought, and certain habits in expression, we have become accustomed to speak and write as if a disease were a natural object, permitting physical recognition, by those whose business it is to be familiar with diseases of various kinds, in the manner of schoolboys who "spot" a coin or postage stamp, and duly label it, after consulting a catalogue and pictures.

For this habit of ours Sydenham must be held, in part at least, responsible, since in one of his least consequent passages he declared "All diseases then ought to be reduc'd to certain and determinate kinds, with the same exactness as we see it done by botanic writers in their treatises of plants." And again—"The same disease appears attended with the like symptoms in different subjects: so that those which were observed in Socrates, in his illness, may generally be applied to any other person, afflicted with the same disease in the same manner as the general marks of plants justly run through the same plants of every kind. Thus, for instance, whoever describes a violet exactly as to its colour, taste, smell, form, and other properties, will find the description agree in most particulars with all the violets in the universe." ("The Entire Works of Dr. Thomas Sydenham" . . . John Swan, 1769: "The Author's Preface," Secs. 7, 12.)

In a later passage (op. cit., p. 240), Sydenham went even farther, saying "For tho' . . . all fevers have some symptoms in general, yet there are certain distinguishing signs which Nature has particularly affixed to every species" (cf. *ibid.*, p. 97. "The term Nature explained").

There is, in these passages, no suggestion nor hint that Sydenham considered diseases aught but, in the late Dr. Payne's words, objects of natural history, or that he appreciated the truth recognized by Oliver Wendell Holmes, at least, amongst doctors, that disease is a vital process: a function, and not an entity. ("The Poet at the Breakfast Table," xi.) The truth is, of course that, whilst *disease* is a wide and comprehensive term, covering not only all disorders of function and all

symptoms or signs of disordered function but also all results of disorder of function, "a disease" is a mental construct; the idea of a symptom or group of symptoms, correlated with or by a single intra-corporeal cause, known or postulated (Mercier, loc. cit., 1916, xlii, p. 231; and 1917, xliii, p. 421). So that, while when we speak of *the* disease from which A or B is suffering we have in mind the whole of the correlated disorders of function, their manifestations and their results, actually present to A or B, yet when we speak or write of a disease by name—whether it be botulism, encephalitis lethargica, or influenza—we do not indicate anything with objective existence, but certain concepts, or mental constructs, with which it is usual to compare our observations on persons who are ill. When we establish correspondence between our observations on the patient and a disease-concept, we "make the diagnosis" and the patient becomes "a case of the disease." Special diseases in fact are as entirely conceptual as are the points, lines, and plane superficies, defined by Euclid.

If we allow ourselves to be led away by false analogy and to think of diseases as things which are evident to our senses and to be described and classified like flowers, we are at once involved in a form of realism less defensible even than that of Duns Scotus and Guillaume de Champeaux. We may analyse and classify our disease-concepts, if we will: but we must surely admit that they are not in the same category as violets.

Now, obviously, the idea of the single intra-corporeal cause is a most important ingredient in the construction of a true disease-concept. He who simply describes a group of symptoms promotes but the conception of a *symptom-group;* the idea of a special *disease* does not come into being until an intra-corporeal cause is ascertained (or postulated, if on sufficient grounds) for the whole group of symptoms described. Justin Kerner, of Weinsberg, a hundred years ago, described symptoms that have been observed many times before and since. He postulated, and thought he had demonstrated, a specific intra-corporeal cause—the presence, in the stomachs of his patients, of some extremely subtle fatty acid derived from Swabian sausages; and he is credited with having discovered botulism. Van Ermengem, twenty odd years ago, described the same symptom-group. He postulated, but did not demonstrate, an intra-corporeal cause—the presence in the blood of his patients of a toxin derived from an organism found in a ham of which they had eaten; but he is credited with having discovered the new botulism—that which has no necessary correlation with sausages, but depends upon the *Bacillus botulinus*.

As already remarked, the same, or very similar symptom-groups have been described by many other observers, who also have thought to demonstrate a single intra-corporeal cause for the associated symptoms; but at the most they have postulated new correlatives. They too have been credited with discovering new diseases. The fact is that our concepts change constantly: as knowledge extends they must do; and "new diseases" really mean, almost always, new concepts—new groupings and arrangements of our ideas and perceptions concerning certain classes of sick persons.

New kinds of *occurrences* are rare: they do not take place unless new causes, or conditions prevail—"T.N.T." poisoning, for example, refers to a set of really new clinical occurrences, and is a veritable new disease. We cannot, however, admit the occurrence, in the "real" sense, of new *microbial* diseases, without abandoning belief in the specificity of forms of microbial life. I do not say that this is undesirable, yet a recent writer (*School Hygiene*, 1919, iii, p. 130) who says that every known complaint must have had to make its début at some time, and who sees no reason why we should be sceptical merely because we happen to have been present at the birth of encephalitis lethargica, would be doubtless horrified at the notion of the spontaneous generation of new microbes, or the transmogrification of old ones.

But, to resume: It must certainly be admitted that, even though the frequent concurrence of special symptoms does not necessarily warrant us, in the absence of a known morbific agent, in postulating a specific correlative for them, and so in speaking of a special disease, nevertheless, under certain circumstances, the idea of a special intra-corporeal cause becomes so imperative that justification cannot be withheld for its recognition. Yet, for any disease-concept to be valid, the chosen correlative, ascertained or postulated, must be peculiar to the symptom-group described, and must be operative in all cases of its occurrence. The occurrences need not, however, always and entirely correspond with the whole content of the disease-concept. When there is absolute or near correspondence, we say that the case is typical. When there is lack of such correspondence, we say the case is atypical. It is the *concept* that is the "type"; and the concept may be so complex as to admit of the notion of several sub-types or varieties of the disease.

These notions will not, I think, be controverted by anyone amongst us after a little reflection; though we have become so hardened to positivist methods of expression that to think of disease in terms of

conceptualism is sometimes difficult. Even Sydenham himself was so gloriously inconsistent as to admit, by implication, the truth of part of the conceptualist position. For, after asseverating that, in his opinion "the principal reason of our being yet destitute of an accurate history of diseases, proceeds from a general supposition that diseases are no more than the confused and irregular operations of disorder'd and debilitated nature" (op. cit., "The Author's Preface," sec. 13), he proceeded to declare that "A disease . . . is no more than a vigorous effort of nature to throw off the morbific matter, and thus . . . recover the patient," and asked: "What is the plague but a complication of symptoms to throw out the morbific particles?" (*Ibid.*, "Of Acute Diseases in General," secs. 1, 3.) The fact is, Sydenham was swayed backwards and forwards by his "common-sense" materialism and by such smack of philosophy as he may have acquired at Montpellier: and, to our present confusion, he never quite decided which horse to ride.

Now, whilst philosophically we must regard special diseases as symptom-groups, correlated by a single intra-corporeal cause, it is nevertheless true that the realities—the symptoms in actual cases—never are, and never can be, attributable to a single cause *alone*. As the late Sir Edward Fry once said: "There is no physical or logical distinction between principal and minor causes, or between causes and conditions, in the case of two or more constituent parts of a cause, each of which is necessary, and none of which is of itself sufficient." Every effect is the product of a long and complicated web of causes, stretching back into infinity, all of which are necessary to produce the effect. (cf., Mercier, *Journal of Mental Science*, 1916, "On Causation," chap. v, sect. 5.)

If a man fall and break his leg, there is no logical distinction in necessity between the various co-operating causes; the banana skin, the muddy pavement, his sudden movements, and the extra glass of whisky taken an hour before. If a man contract pneumonia through exposure, the exposure is logically as necessary a cause of the illness as is the particular bacillus that happened to be lurking at the time.

We are, however (again to quote Dr. Mercier), accustomed to speak of the cause (of any effect) in which for our purposes we are most interested, as *the* cause of that effect: and so, in forming our concepts of special diseases, we select, as correlatives for the symptom-groups chosen, those causes in which we are most interested. Fifty years ago we preferred gross or fine anatomical changes in the tissues, which were supposed to be the "causes" of various diseases. We now recognize

these changes as expressions of the disorder of inner or nutritive functions of the cells, provoked by something else which we think really is the *causa vera*. This "something else," until recently, was generally an ascertained, or postulated, specific and visible organism, that had scrupulously reproduced, throughout countless generations a day, the specific ancestral characters it acquired during the Archæozoic Epoch. To-day, however, fashion leads us to select symptom-groups having reference to particular regions — the hippocampus minor or the red nucleus, let us say—and to postulate, as *the* cause of the symptoms, something filter-passing, invisible, indescribable, unknown, and almost unknowable; but having a specific affinity and destiny for certain portions of our anatomy and none other. These be the causes in which to-day, students of disease, *for their purposes*, are most interested. And so, one of these seen or unseen, ascertained or postulated organisms becomes *the* cause (the *causa causans*, or *causa vera*, in the language of the more erudite) of what is said to be a disease *sui generis* caused by it, and defined accordingly.

But the clinical physician varies from day to day the content of his disease-concepts, in accordance with *a posteriori* results and the dictates of academic convenience: not only in respect of the causes but, as his knowledge accrues, and his point of view changes, in respect of the symptoms described. He makes cross-references, and mentally files his observations sometimes one way, sometimes another. So inevitably, what is but a symptom-group to-day may become a "disease" to-morrow: what now appears a disease may, next week, be considered but a part of a greater construct.

Hippocrates, first of epidemiologists, described occurrences and not diseases. (cf. "The Genuine Works of Hippocrates," Sydenham Society's edition, 1849, pp. 342-43.)

The Galenical and systematic description of ideal forms of disease has been abused, and is still abused, by those who, under the unconscious influence of scholastic realism, persist in confounding mental constructs with real occurrences, and in attributing objectivity to what has no existence save in the mind.

Let us, however, acknowledge that, while no "disease" *sui generis*, boasting a single *causa vera*, exists, save as a justifiable mental construct, useful and convenient, we may regard the idea of a special disease as one involving the subordinate notions of (1) a single person manifesting (2) a defined group of symptoms, correlated by (3) a single intra-corporeal cause.

(II) What is an Epidemic?

To the ordinary clinical observer who is concerned principally with case-occurrences and with disease-concepts, an epidemic means nothing more than a crop, or prevalence, of what he deems cases of a special disease. The cause of the epidemic is, for him, the cause of the disease: only there is a lot more of it. It is perhaps admitted by him that this *causa vera* of the disease, which existing in bulk is also the *causa vera* of the epidemic, has become, somehow or other, "exalted in virulence"; though he will not agree that, save within extremely narrow limits, a micro-organism can vary in morphological or cultural characters. He finds it also allowable to hint that the diffusion of the *causa vera* may be "predisposed to" by variations in "resistance" on the part of human beings; though how and why micro-organisms should thus vary in virulence, and human beings in resistance, is not usually explained by him. All this, however, is not epidemiology. It is the medicine of the schools and the journals.

The fact is, the word "epidemic" has been, and is still used in several senses, of which one is adjectival and obvious, and the others substantive. This plurality of usage has tended greatly to obscure thought, to the present ascendancy of positivism in epidemiology. We may speak of "epidemic disease," meaning thereby disease which we conceive as tending to become prevalent; and we may speak of special epidemic diseases, referring thereby to special disease-concepts which embrace the notion of a tendency to prevalence or diffusion.

When, however, we speak of "an epidemic," greater difficulty arises. We may intend to refer to a certain prevalence amongst a certain community; something that is happening. This usage is analogous to that of the term "a case." There is, we say, a case of scarlet fever in Wimpole Street; and an epidemic in Marylebone. Thus spoken of "an epidemic" usually means nothing but a number of "cases" of a particular kind occurring simultaneously. Or we may use the word *generally*, as we also use the word "case," without prefixion of an article; and say that cases and epidemics of encephalitis lethargica occur in the spring, while cases and epidemics of poliomyelitis occur in the autumn. (We feel on safe ground in so doing, for we have "defined" encephalitis lethargica as something that blooms in the spring, and poliomyelitis as something that flourishes in the autumn.)

So long as by "epidemiological inquiry" we mean the *post hoc* investigation of "cases" that comply with the necessities of a pre-

ordained conception, these usages of the word "epidemic" will serve their turn well enough. But, this again is not epidemiology. There is an older and a truer sense in which "epidemic" may be used substantively; a sense better expressed by the French word *épidémie* as also by the now obsolete English form, *epidemy*.[1]

The sense I have in mind is that of a *concept*, similar in construction to that of "a disease," but of different content; a concept that is, to which actual prevalences can be logically referred for verification, just as case-occurrences are referred to *disease-concepts*, in diagnosis. Three notions are again involved: (1) That of the subject deemed to be affected, (2) that of the disorders manifested in or by the subject, and (3) that of the correlating factor, which we speak of as *the* cause. But what are the differences?

Dr. Greenwood, in his most valuable paper, read before the Seventeenth International Congress of Medicine, in 1913 (sect. xviii, discussion 3, part 1(a)) observed, with great, but only partial truth, that the classical definitions (of an epidemic disease) either assert or imply that the chief part in the causation (of such diseases) is played by something external to the sufferer. I think that by "epidemic disease" Dr. Greenwood here means what I now intend by the word "epidemy;" although I admit that the term "epidemic disease" may sometimes be used justifiably in reference to a concept transitional to those of a disease and an epidemy: but at any rate I venture to substitute, in Dr. Greenwood's sentence, the word "epidemy" for the term "epidemic disease." The chief part in the causation then, of an epidemy is, by the classical definitions implied, or asserted, to be played by something external to the sufferer. If by the term "external to the sufferer," we understand something extra-corporeal, as opposed to the intra-corporeal "causes" allotted to special diseases, I fully concur. But Dr. Greenwood's phrase seems to me so far faulty that it implies the sufferer from an epidemy to be of the same nature as the sufferer from a disease.

Now, in the words of Brochin ("Dictionnaire Encyclopédique des Sciences Médicales," xiii, art. "Catarrhe," p. 295): "*Dans les épidémies, bénignes ou dangereuses, il ne s'agit pas d'un homme, mais de la cité, et même de tout un pays.* CIVITATEM NON VIRUM CURABIS."

[1] A very interesting dissertation by Laveran, on the different usages of the word *épidémie*, is to be found in Bouchard's "Traité de Pathologie Générale," though it must be confessed that the distinguished author, as is perhaps natural in view of the special nature of his life-work, retreats somewhat from the advanced position of the great philosophical French epidemiologists, and takes up a Teutonic and positivist entrenchment.

This is no paradox, that the sufferer from an epidemy is never an individual, but always a community or group; and, its truth is evident the moment that the conceptual aspect is accepted.

The phrase "external to the sufferer," is then seen to be erroneous; for the accepted cause of an epidemy, though extra-corporeal, is intra-communal. The present confusion has arisen in two ways: first, because we attach the same label to certain epidemy-concepts that we do to certain disease-concepts (those called "influenza," to wit), and secondly, by reason of the persistent, if unconscious attempt to evade recognition or admission of the differences between the occurrences and prevalences on the one hand, and the special concepts, or mental constructs, on the other. We are again hampered by that scholastic realism which, to my mind at least, is far more reprehensible than the philosophic subtleties of the Montpellier School so adroitly rebuked by Dr. Greenwood in his most recent paper (*British Medical Journal*, September 27, 1919, p. 405).

As soon as we admit that special diseases and special epidemies are conceptions (nominals, or universals, at most, if you will, but even so, of different orders) the superficial difficulty in accepting an intra-corporeal factor as the cause of a disease and an extra-corporeal factor as the cause of an epidemy known by the same name, at once disappears. As Teissier observed, in his admirable report on the influenza in Russia during 1889-90, the cause of the *epidemy* is, not the germ, but those agents or agencies "*qui favorisent l'éclosion ou facilitent la dissémination du germe.*" We imagine the sufferer from the "epidemy" then, not as the person who harbours the germ that we call the *cause* of the *disease*, but the *community* within whose terrain play the agencies which disseminate the germ and favour its pullulation. For the reasons already suggested, we, as epidemiologists, are entitled to select (as in fact we commonly do) for the correlative in our concepts, some cause not necessarily that which interests the clinician, concerned with microbes, men and diseases, rather than with populations and prevalences.

Each observer, whether he be physician or epidemiologist, is, in his own province, justified: but tactics are not strategy, and the corps commander must not think like a company officer. It was therefore with surprise that one heard so many *epidemiologists* during our late debate on influenza (November 13-14, 1918), refer to the telluric or climatic influences, which may or do favour the proliferation or provoke the dissemination of the germs (which we esteem the "cause" of the catarrhal

diseases) as merely "predisposing" agencies. Predisposing? For the physician, Yes! For the epidemiologist, No! Sydenham in this respect was on sounder ground than many moderns, for he distinguished (loc. cit., p. 3) *acute diseases* (arising "from some peculiar indisposition of particular persons," and "not produced by a general cause") from *epidemic distempers* proceeding "from a latent and inexplicable alteration of the air, infecting the bodies of men: and not from any peculiar state of disposition of the blood and juices." The theoretical distinction between the intra-corporeal causes of "diseases" and the extra-corporeal causes of "epidemic distempers" is here plainly indicated, but Sydenham, failing to recognize the relativity of his *concepts*, became fogged (as a less honest observer of clinical and epidemiological occurrences would not have been) at the inevitable lack of adjustment between the realities and the schemata, as he varied his point of view from that of the clinician to that of the epidemiologist, and *vice versa*. Forced to admit that "an occult influence of the air may communicate to the body a peculiar disposition of the blood and juices," he bewailed (*ibid.*, p. 11) the difficulty of classifying epidemics unless "we take such a liberty as no one would easily grant to a botanist who, in writing the history of plants, is obliged to abide by the testimony of the senses." Confusing concepts and realities, he was puzzled sadly at the difficulty in classifying what he actually observed, by species and genera, and failed entirely to grasp that even his own conception of an epidemic relates to communities rather than to individuals.

It is obviously impossible to give a quantitative definition of the subject of an "epidemy," for there are prevalences of various grades. There are grouped occurrences, commensal happenings, village outbreaks, camp devastations, urban prevalences, national disasters, and continental or mundane catastrophes; so that we really require, and do in fact employ, subsidiary concepts to which we refer various prevalences, and which help us to keep in mind the diverse causes that concern us most when we have to do with groups, families, villages, camps, cities, nations, or leagues of nations. Fuster, whose fascinating though long-forgotten treatise deserves careful perusal and study (it is of Montpellier) recognized this long ago. He discriminated between diseases and epidemics, and showed how there is need also for distinction between "*les petites épidémies*" and "*les grandes épidémies.*" ("Des Maladies de la France," pp. 255, 357.) The lesser epidemy boasts only a *local*, but still a communal correlative; the greater epidemy connotes a factor more general, but yet one applicable to the population affected.

So far, one has discussed but two of the notions—the subject and the cause—that enter into the conception of an epidemy. Now while *symptoms* are, for the individual, correlated to form the disease-concept, for the community it is the illnesses of different persons that constitute the "symptoms" of the epidemy. In a "typical" case of a disease, the symptoms all occur in one person. But, in a "typical" epidemic-prevalence, there are different illnesses affecting different persons in one community. This again is no paradox, but a truth, which we at once recognize on reflection, though it is concealed by our habit of speaking of a "protean disease" when we are really referring not to a "disease," but to an "epidemy."

Case scarlet fever, *scarlet fever* the *disease*, when scarlet fever is not prevalent, is seldom diagnosed in the absence of an approximation to the normal disease-concept. But, when scarlet fever is *prevalent* the epidemiologist diagnoses the "epidemy," not merely because there are a certain number of typical cases of scarlet fever, but because there are cases of sore throat, cases of rash, cases of desquamation, cases of nephritis, and the like, as well: so that notifications come to be made and accepted on grounds which, in the absence of a diagnosed *epidemy* would not be considered to justify diagnosis of the *disease*.

To diagnose an epidemy then, one must collate cases in a community, whilst to diagnose a disease one need only collate symptoms in an individual. *Hence* the well-known varied and inconsistent character of "diseases" *when they become* "epidemic"; and, as I ventured to say recently (*Lancet*, May 18, 1918, p. 699): Epidemics are necessary for the realization of the pathological unity of diverse symptom-groups due to the same exogenous virus. Moreover, just as we speak and think of incomplete or atypical *cases* of the disease (how seldom does a case correspond to the whole content of the disease-concept!) so have we incomplete, or atypical prevalences, not embracing the whole content of the epidemy-concept. This was well recognized by Wickman, in the case of Heine-Medin disease; and has also been pointed out, by Wade-Frost in America, for "poliomyelitis." (*Hygienic Laboratory Bulletin*, October, 1913, No. 90.)[1] Just as symptoms vary clinically in different cases of poliomyelitis, so epidemiologically, do cases vary in different foci, foci in different epidemics, and epidemics amongst themselves, in respect of Heine-Medin disease. (cf. Wickman, "Nervous and Mental Disease Monograph Series," New York, No. 16, p. 5.)

[1] Poliomyelitis is really the name given to a disease-concept: Heine-Medin disease is, properly speaking, the name of an epidemy-concept.

There is, however, a point of extreme importance in respect of what may be called the case-symptomatology of epidemics, which was recognized by Sydenham, in certain passages that escape those who only read the customary first chapters of his collected works. In the "Essay on Intercurrent Fevers" (op. cit., pp. 245-46) Sydenham, after setting on one side the ordinary *intercurrents* (as he calls certain "diseases," such as pneumonia), refers to intercurrents sometimes proceeding "*mediately* from some general cause in the air which . . . so disposes the human body, as to occasion certain disorders of the blood and juices, which prove the immediate causes of such *epidemic intercurrents.*" Here we have acknowledgment of the chain of causation, and Sydenham, although obsessed as always by the notion of the absolute values of causes, hesitated to which of the two categories he should refer certain occurrences that he had observed. An intercurrent? or an epidemy? Like a true Britisher, he takes the middle and "practical" course—he calls it an *epidemic-intercurrent.* However, we read on and find (p. 246) "that tho' the diseases I am now to treat under the title of *intercurrents*, were most, if not all of them, essential diseases; yet frequently certain disorders happen in *stationary fevers*, resembling these *intercurrents* as to the phenomena, and likewise characterized by the same name, which however are manifest symptoms of those fevers."

Let us consider what happened during 1918. In May, before anything was heard of the Spanish influenza, I saw, at the London Hospital, about thirty grouped cases of virulent pneumonia from Stepney. They were recognized clinically as cases of an "essential" disease—pneumonia, to wit. Epidemiologically, they were symptomatic of an apparently autonomous prevalence; they seemed to correspond to elements of an essential epidemy. But precisely similar cases, occurring in November, 1918, were everywhere recognized as cases of pneumonia, secondary to influenza, the then "stationary fever." Epidemiologically, the prevalence of pneumonia during November was not deemed autonomous, like that of May, but a "manifest symptom" of a greater prevalence. *Mutatis mutandis*, as much can be said of the nervous cases seen in March-April, 1918, and since observed, though now labelled "influenzal encephalitis" and *not* encephalitis lethargica.

I confess that, in the light of this experience, the two passages that I have just quoted seem to me amongst the most instructive that Sydenham penned. For they shed a beam of light on Dr. Hamer's "seductive hypotheses"—it is no shame in an hypothesis to be

seductive—as well as on the brilliant phrase of Malcorps who, eighty years ago, spoke of certain nervous prevalences as *prodromes nerveux* of influenza on the grand scale. Truly enough, just as, epidemiologically, we think of cases as symptoms of a lesser epidemic, so may we think of prevalences as symptoms of greater epidemics; and moreover, of cases and prevalences alike as prodromas, or signs of invasion, and sequelæ or signs of regression, of such major "epidemics" as we shall presently consider in the light of the concept of "an epidemic constitution." The cavalry scouts of the advancing army are an integral part of it, no less than is the cavalry corps controlled by the chief command.

Again, however, are *names* a rock of offence: one, by "influenza," means the disease; another, the epidemy, and some of us have in mind rather a vast, heterogeneous, unstable, differentiated, and composite pestilence instigated by major forces remote from the minds of those who employ the same name for the simple idea of plain symptoms, in a humble individual, due to the malevolence of as many filter-passers as, like the angels of the Schoolmen, could dance on the head of a pin.

In a "typical" case of influenza we recognize nervous, respiratory and gastro-intestinal symptoms. Other cases present predominantly symptoms that are nervous, respiratory, or gastro-intestinal. In a "typical" epidemic of influenza there are cases which appear nervous, others which are respiratory, others which are gastro-intestinal. In "atypical" epidemics the cases are mostly nervous, respiratory, or gastro-intestinal. But, in the major prevalences extending through a term of months, or even years, there are groups, or lesser prevalences, that appear as essential, or autonomous epidemics, of nervous, respiratory, or gastro-intestinal nature.

Now, it may be that bacteriologists will yet demonstrate the sudden and wide diffusion of some parent organism which, rapidly differentiating, produces strains which preserve, each in their kind, for a certain number of removes, their autonomy and apparent specificity, and which we may one day recognize as the several correlatives for our concepts of the influenzal diseases and lesser epidemics, apparently sometimes essential and autonomous, sometimes secondary and dependent. In this respect the work of Donaldson (*Lancet*, 1918, ii, p. 723) Beaumont, Fry, Edington, and others (*Lancet*, 1919, i and ii) is certainly instructive, and a recent paper by Lewis (*Boston Medical and Surgical Journal*, 1919, clxxxi, p. 540) remarkable for its breadth of view and truly epidemiological spirit, tends to support the suggestion, made by me (*Lancet*, 1919, i, p. 314) as to the importance of the previous

work of Rosenow, whose pleomorphic coccus is undoubtedly the same organism as that described by those writers whose names I have just given. The consequences of acceptance of the work of these bacteriologists may be unwelcome to the orthodox, but that does not justify evasion and neglect. But it may be that the warrantable correlative for the vast disturbances is some physical state or condition—telluric, if you will, or possibly cosmic—that activates Pfeiffer's bacillus, the meningococcus, *et hoc genus omne*, at the same time and for different people.

Surely, however, the duty of the epidemiologist is to pursue the search for the epidemiological correlatives, no less ardently than does the clinical pathologist prosecute *his* researches, though for the former, the *a posteriori* method may be less fruitful than it has been in the hands of the latter. Neither investigator need gibe or gird at the other; but mischief comes when, like Leichtenstern, in his essay on Influenza, in Nothnagel's "Handbuch," we use the same names for different purposes, and for different classes of occurrences and conceptions indifferently.

By an epidemy, then, we mean the whole of the correlated symptom-groups manifested by a community or population. The correlative, known or postulated, we call the cause of the epidemy. We think of the individual *cases* as correlated by a factor which is by no means necessarily identical with the correlativ of the symptoms in each case, and the cases themselves are not necessarily clinically identical.

(III) The Epidemic Constitution.

It is obvious, I think, that there is occasion for a synthetic construct of yet greater scope than the "epidemy," for the word "pandemy" only suggests quantitative difference therefrom. The words "epidemic constitution" however, may be applied (1) to a concept which differs from an epidemy, not in construction, but as an epidemy differs from a disease; and (2) to happenings which differ from epidemic-prevalences as they from case-occurrences.

Now, although both case-occurrences and epidemic-prevalences have duration, yet it is clear, on reflection, that a determinate period of time is no essential component of a disease-concept, or an epidemy-concept.

I am aware that Dr. Greenwood has written of a temporal as well as a spatial theory of epidemics; but this is in reference to special aspects, or qualities, of prevalences and not to essential components of

the concepts. He says himself that it is usual to regard an epidemic disease as a malady which affects a number of human beings either simultaneously or within narrow limits of time. We cannot, however, think of an epidemic constitution without reference to a period; and it is evident that by the term we really mean to imply all the correlated symptomatic disorders and their consequences (some, prevalences; and some, cases) manifested in a community or population during a period of time that is not arbitrarily selected, but has natural duration and limitation, and during which prevalences and occurrences of distinctive nature arise.

The "subject" of an epidemic constitution is in fact a *time-population* unit; the period of time is a dimension of the extension. Now, since we cannot conceive "time" without reference to the astronomical bodies—if this world existed alone, in space, there would be no time—there can be no thinkable correlative for the disorders in a natural time-population unit that does not relate to "heavenly," "celestial" or extra-telluric influences, however vague and ill-defined our thoughts in such directions may be at present, or finally. The idea of domination or influence by the celestial bodies (1) determining telluric cataclysms and disturbances, and (2) profoundly affecting, through seasons and climates, not only the grosser forms of animal and vegetable life but the pullulation and efflorescence of those lowlier (known or invisible) has always been present to the minds of philosophic epidemiologists, though abhorred of late years by those who forget the infinitely great in the contemplation of the infinitely little. Let it not be forgotten however that the current attribution of seasonal prevalences to special diseases (e.g., poliomyelitis and encephalitis lethargica) in itself implies subservience to the *regular* "skiey" influences.

The elements of this conception are scattered confusedly throughout the writings of Sydenham. Sect. 5, chap. ii "Of Epidemic Diseases" (edit. cit.) and sect. 6, suggest atmospheric qualities as the causes of *epidemics*, and "alterations in the bowels of the earth, whence the air becomes impregnated with . . . kinds of *effluvia*," as related to *epidemic constitutions*, it is true; but it is repeatedly implied that these constitutions prevail "through a course of years," and the references to the coincidence of epidemics with the equinoxes, and to the "mysterious successions of time" are well known. But I am not concerned to insist on the prescriptive rights of Sydenham whenever epidemic constitutions are discussed. For, not only does Sydenham's system or philosophy of epidemics derive from the simpler writings of Ballonius but, without

doubt, it is to Ballonius that is really due the renaissance, in the sixteenth century, of the Hippocratic philosophy of epidemics and constitutions, although, in his "Epidemics and Ephemerides," written at Paris in 1574, the Galenist influence—as remarked by his translator Yvaren—is fully evident. As a matter of fact it was Baillou's Galenistical gloss upon the Hippocratic epidemiology that led directly to the strange inconsequences of Sydenham, who has as little claim to be the reviver of classical epidemiology as Wren to be that of classic architecture.

The school of epidemiology founded in France by Ballonius has never died out; it persists to-day, and still tinctures state policy in respect of state medicine, though not, I admit, altogether advantageously. In England it has been otherwise. The followers of Sydenham left hardly any to carry the torch into the nineteenth century, though the name, writings, and thought of Peacock should not be forgotten. The attempt at recognition of epidemiological *principles* seems indeed, during the nineteenth century, to have been almost abandoned. Statistics and field-observations are matters of method, not of principle;[1] and for the rest, the *cathedra* of the professed epidemiologists seems to have wobbled uncertainly on two legs; one, the dogma that specific diseases were real things to be found in the post-mortem room; the other, that epidemics were to be abolished by sanitation. Both beliefs were bred by *a posteriori* methods out of Victorian realism. Within our own memory an attempt has been made to secure equilibrium by the insertion of a third support; that of systematic bacteriology. But this has resulted in a fatal insecurity, for it must be admitted that, in spite of such gallant efforts as those made recently by Dr. Topley (*Lancet*, 1919, ii, pp. 1 *et seq.*), no bacteriological solution of the great problems, no bacteriological theory of epidemics, has yet been achieved. Almost alone, Sir William Collins some years ago raised his voice against the positivist doctrines of specificity in disease and the excessive claims of the bacteriologists (*Public Health*, 1889-90, ii, p. 170). Now that the inflated claims put forward in some quarters have collapsed, his papers may be studied anew.

For how much, then, of the "epidemiology" of the last hundred years is Sydenham really responsible? What would he say if asked to approve the "post hoc" investigation of a few selected cases, chosen

[1] Dr. Greenwood reminds me that there are recognized statistical *principles*. This is true: but epidemiologists less accomplished than he sometimes exercise statistical principles in a manner that, epidemiologically, is quite unprincipled.

because of their conformity to an arbitrary preconception, as likely to throw light upon "the well-known varied and inconsistent character of those diseases we call epidemic"? I think his reply would be unappreciative: but his great contemporary, Willis, more Galenical than he, would be glad to recount at Whitehall to a not unsympathetic audience how what is now called influenza came upon *him* in 1658 "suddenly as if sent by a blast from the stars," and to point out that the epidemic lethargy of 1657, described by Bartholin in Copenhagen,[1] was in no wise to be correlated with the English fevers of the following autumn, whereby men were "grievously afflicted in their brains and nervous stocks," but was a disease *sui generis*. Not so Sydenham, who would be happier at Savoy Hill, studying there a survey of all the correlated disorders of health in London during 1917-18.

Sydenham has really no claim to the parentage of *modern* epidemiology, if, by modern we mean that which is official in *our own times;* and only in an insular sense was he the father even of eighteenth century epidemiology. Ballonius, on the other hand, who not only studied epidemic constitutions in the sense of Hippocrates but who, like Hippocrates and unlike Sydenham, recorded *case-occurrences* and gave us post-mortem records, was the first epidemiologist of modern times, and we should perform a graceful act of international justice in frankly admitting as much.

Now, since both Hippocrates and Ballonius, when recording "epidemic constitutions," narrated cases of disease, which we can hardly call parts of regular epidemics, or prevalences, it is clear that the classical idea of an epidemic constitution embraces, and rightly, *diseases* as well as epidemics; while we are impelled to think of the time factor as one during which a particular cosmic dispensation or conjunction—the *quid divinum* of Hippocrates—exerts an influence which, *ex hypothesi*, is repeated at secular intervals. *Actually*, we observe certain congeries of prevalences and occurrences. We feel that they are correlated, and we put them to the test, by referring them, and records of past constitutions, to our *general* conception of an epidemic constitution: in fullness of time concepts of *special* constitutions may take shape. This is not yet, though clearly it was in this hope that Hippocrates, Ballonius and Sydenham tried to leave behind them records that would be useful to those who would come after. Have we sufficiently profited thereby? Certainly of late we have seemed on the point of being able to

[1] Vide Crookshank, "A Note on the History of Epidemic Encephalomyelitis," *Proc. Roy. Soc. Med.*, 1919, xii (Sect. History of Medicine), pp. 1-21.

"diagnose" certain prevalences and occurrences which tend to recur in association at secular intervals: we must not hesitate to face the facts merely because at first blush the consequences seem incompatible with the aspirations of those who prepare vaccines.

Facing the facts sometimes shows us the best way to escape the consequences; and we have as much justification, in every respect, in seeking, in extra-mundane factors, the causes of secular variations as have working medical officers of health for seeking, in the drains or elsewhere, the causes of parochial visitations, and bacteriologists, in a drop of blood, the *quicquid irritans* that is the focus of a local diapedesis. But the methods of Hippocrates, Ballonius and Sydenham are seldom followed in these days, when the study of the spread of influenza in a bell-tent is considered a "valuable contribution to epidemiology."

The suggestion that there is lack of opportunity is less paradoxical than it seems. Conditions of practice and observation have altered. Hippocrates, Ballonius and Sydenham could observe the general as well as the special characters of prevalences and occurrences. Such observations are not now easily made by the physician who passes his day between chamber consultations at home and the visitation at hospital of selected cases of special nature, while the pure neurologist may well be pardoned if he does not connote the "interesting" cases he sees with those that do not get through the barrage of the casualty department, or those treated by his colleague who "sticks to chests." And, few indeed of those who sit in offices and peruse records, tables and other statistical paraphernalia are gifted with the scientific imagination that enables them to form a picture of *all* that is passing beneath the eyes of others who are at work in general practice, in the dispensaries, and in the casualty departments of our vast metropolis.

To how many was it obvious, so early as August, 1917, that something strange was indeed "in the air"? There were a few cases that made one think of typhus, and diagnose Brill's disease, and there were many stuporose or drowsy febrile children, cases that we labelled "acidosis" or "constipation."[1] There were many odd cerebral cases too, and tuberculous meningitis was diagnosed, that got well. As the autumn came on there was some homely influenza, but one noticed a great increase in the anæmias and the purpuras, the old time "scurvies"—it could not have been the food, for official persons said

[1] Similar happenings were noticed by Lowenburg, in New York, early in 1918 (*New York Med. Journ.*, 1918, cvii, 1220).

so—and, what struck me as very odd, literally scores of children with tumid bellies, containing fluid. Tuberculous peritonitis, of course; but one should have remembered what Sydenham said concerning the swelling of the abdomen that happens to children in those years wherein there is a tendency to the production of autumnal "intermittents" epidemically. (Op. cit., pp. 68, 69.)

At any rate, so great an impression was provoked that, speaking publicly at the Hampstead Hospital, forecast was made that 1918 would be a year of pestilence. Small-pox indeed began to show itself here and there, and January brought us cases which some spoke of as typhoid, and a few hardy souls diagnosed as "gastric influenza"; while there were many odd "nuclear palsies," much facial paralysis and so forth. February showed us more, and then an epidemic of encephalitis, of Heine-Medin type, was foretold. March gave us the delusive diagnosis of botulism; in April there were alarums and excursions of various kinds, and in May the extraordinarily short, sharp and fatal outbreak of pneumonia already mentioned. It filled the beds rapidly and a friend of mine, who has lived many years in China, shook his head, and talked of pneumonic plague. Then came the "Spanish 'flu'"; the quick assurance that all was well; and warnings, almost completely disregarded, of an autumnal return in force, written out by my friend Dr. Rajchman, now directing the epidemiological work in Poland.

The events of the latter months of 1918 are well known, but this should be noted: that, during the weeks when the names botulism and encephalitis lethargica were so popular professionally, not only the general, but the special hospitals saw far more than is usual of Landry's paralysis, of ascending myelitis, of transverse myelitis, and the like. To all of this no attention was paid. Interest was focussed *on one type of nervous disease alone;* and for the first time during many years a new disease was defined on a regional basis without attempt to study the *whole* of the prevalent disorders in the hope of arriving at some idea of the scope of correlation.

The fact is that your epidemic constitution is under way long before the death returns and the notifications can be sensibly affected; and these again are not largely altered until the spread of diagnostic ideas amongst the working members of the profession influences their choice in nomenclature. A bureau of meteorology must not be content to "anticipate" rain only when reports come in that umbrellas are going up and pedestrians are getting wet. Field work is required now more

than ever, it is true; but, it is also no less true than when Darwin said so, that, without hypothesis, there is no useful observation.

* * * * *

It is not, then, as a mere essay in transcendentalism that I venture to plead now for a consideration of *principles* in epidemiology and for elucidation of the three fundamental concepts which, implicitly at least, have been recognized by the three great epidemiologists of the world. We must avoid scholastic realism no less earnestly than we would scholastic nominalism; but there is a place for sane conceptualism in our attitude towards the problems at issue. I do not say that, in abandoning the camp of Duns Scotus, we should range ourselves under the banner of Roscelin, but we may find salvation in the sanity attributed to Abélard, and preached by our own William of Ockham. Dr. Hamer has sharpened for us William of Ockham's razor: *Entia non sunt multiplicanda praeter necessitatem.* But there is another blade: *Sufficiunt singularia, et ita tales res universales omnino frustra ponuntur.* (cf. Ueberweg, "A History of Philosophy.")

It is idle to say that in medicine metaphysics has no place, and the address by Sir William Collins, on "Physic and Metaphysic" (*St. Bartholomew's Hospital Journal*, 1905) may be commended to the unwilling.

No one would wish to deprecate the value of the *a posteriori* method: that were as foolish as some of us think the neglect of the "*a priorities*," and, since the unknown stretches far away and beyond either end of the petty spectrum that represents for us the known, we need never forget the infinitely little in the contemplation of the infinitely great. Yet science must ultimately rest upon metaphysic.

May I conclude by the brief narration of a fable? Several years ago, an ingenuous police surgeon, investigating what he was told was a case of murder, found a bullet in a heart. This he decided, and so told the coroner, was the *causa vera*, the *causa causans*, of the symptoms in this case of murder. Shortly after he went abroad to a war, and, honestly believing that war is but murder on a large scale, he investigated the appearances of many bodies; again finding bullets, he declared that bullets are the cause of war, as of murder. But, in not every fatal case was the bullet of the same kind. Moreover, the occasional absence of bullets disconcerted him until he realized that he had once found gas poisoning the *causa vera*, in a case of murder, and he therefore came to the conclusion that several wars here existed, side by side; each one *sui generis*, and boasting a different *causa vera*.

He then proposed to end war by discharging other and like bullets and gases in a contrary direction, and found many who approved his plan as sensible. However, some pestilent and philosophic person told him that war was not the mere numerical exaggeration of cases of murder, brought about either by an exaltation in the virulence of bullets or gas, or by a diminution in resistance to these agencies: it was our name for a state of affairs that we conceive as brought about by the play and interplay of racial, economic, and other factors. He was told, moreover, that while undoubtedly various kinds of killing are elements of war as conceived by the historian and statesman, wars are not to be prevented, as he hoped, by avoiding persons who, in tramcars and in cinemas, carry bullets, or who project poisonous gas in public places. He was, however, unconvinced, and returned to England more settled than ever that the causal agents of war are bullets (of various kinds, no doubt) and gases (of various toxicities, certainly), while the best hope of preventing war in future lies, not in talk about vague racial, economic, or political conditions (which can only, he thought, at most be predisposing), but in devising some means of circumventing the *causae causantes*, bullets and gas!

I do not pursue this New Analogy further, but I commend to the thoughtful some words of Fuster (1840) (op. cit., p. 319): "Les médecins systématiques, ceux surtout qui ne visent au lit des malades qu'à la détermination des lésions matérielles, et à saisir la relation des symptômes avec le siège de ces lésions, s'embarrassent immanquablement dans la variété inextricable des symptômes et des lésions des épidémies, ne trouvant rien de mieux pour se tirer de ce dédale, que de nier, seuls contre tous nos prédécesseurs et nos maîtres, la multiplicité des formes de ces maladies. Le vrai médecin ne perd pas sitôt la tête."

Hear, too, Ballonius (1574): "Par contre ceux-là sont convenablement préparés aux fonctions de notre ministère, qui se sont familiarisés avec la connaissance des constitutions antérieures et la prévision des constitutions prochaines, de manière à ne pas crier à la nouveauté lorsque des maladies se présenteront avec tel ou tel génie, et à ne pas être terrifiés par toute affection nouvelle, comme s'ils étaient en face de quelque monstre inconnu, ainsi que cela arrive à ceux qui vivent au jour le jour, peu au courant des choses d'autrefois: on les voit tomber dans l'étonnement des demi-savants et dans les explications du vulgaire. Tandis qu'ils s'absorbent dans leur surprise, ils laissent échapper et se perdre une foule de faits qui, recueillis avec soin, leur eussent valu une gloire immortelle." ("Épidémies et Éphémérides," trad. par Yvaren, Paris, 1858, p. 78.)

SUMMARY.

(1) The expressions *disease, epidemic,* and *epidemic constitution* are severally used in different senses, which need to be carefully distinguished.

(2) They are used with reference to observed *phenomena:* correlated *symptoms* in individuals, *cases* in communities, and *cases* and *prevalences* amongst populations during determinate periods, respectively.

(3) They are also used with respect to concepts (or universals) of such symptoms, cases, and cases and prevalences, respectively, considered as correlated by special factors which we recognize as *the* causes.

(4) These concepts are: (*a*) generic and (*b*) specific: in the latter case receiving the names of special diseases, special epidemics (and special epidemic constitutions).

(5) Each usage of the several expressions "disease," "epidemic," and "epidemic constitution" is legitimate, so long as it is understood: but error and confusion are inevitable when specific concepts are regarded as having objective existence.

(6) Belief in the objective existence of *ideal* diseases, epidemics (and epidemic constitutions) obscures the value of these mental constructs, which are indispensable for the systematization of information acquired *a posteriori*, and do afford the *schemata* necessary for the prosecution of profitable *a posteriori* research.

DISCUSSION.

Dr. MAJOR GREENWOOD: In pleading for a philosophy of epidemics, Dr. Crookshank is handing on a torch which has been borne by many runners. Sometimes it has burned very dim, but it has never gone out. There is some pathos in the thought that it burned brightly in the hand of a runner replaced by generations of poor athletes. I mean in the hand of Galen, to whom Dr. Crookshank has hardly done justice, but who would have listened to Dr. Crookshank's paper with interest. Galen derived his general philosophy of epidemics from the Hippocratic corpus, and like Dr. Crookshank he made the notion that the unit of an epidemic is not the individual but the aggregate, a fundamental principle—i.e., that the phenomenon is indivisible, not a mere assemblage of separate units. This is why what Sydenham would have called "skiey influences" are so important in the Galenical or Hippocratic ætiology of epidemics, although Galen admitted that "it was possible that a universal disease might be produced by drinking infected water, as according to

tradition has happened in camps."[1] Although we should now recognize that the restriction of factors operating upon collectivities to these "skiey influences" was too narrow, Galen's principle is still valid, and it is interesting to inquire why the development of epidemiological philosophy has been so imperfect. I believe the clue is given by Galen himself, who concludes his general exposition with a remarkable passage, which I may condense as follows: "This at least let me impress upon those who would learn the art of medicine, this precept which I have enforced in so many books. Let them take pains by the use of particular observations within the compass of the senses, rightly to comprehend the universals. The empirics hold that only from particulars can we pass to a universal, that all true teaching is of experience. I think far otherwise, and believe that much true teaching is derivative of reason, but that the clarity of our judgment thereof is strengthened and stablished by experience. Thus in astronomy and geometry is it by experiment and observation that what has been rationally deduced is made clear and commands assent. How much more in the discipline of medicine must universals be strengthened through the trial of particulars." This golden rule to bring the universals to the test of particulars has been neglected. Its neglect, quite as much as any distaste for general principles, is responsible for the present low estate of medical philosophy. There have been so many "universals," and they have so rarely been on speaking terms with "particulars." That is why the mouths of the systematists of Hoffmann, of Stahl, of Brown, of Cullen and, *pace* Dr. Crookshank, even of Barthez, are stopped with dust, and the wild ass, laden with Petri dishes and oil immersion lens, stamps o'er their heads and certainly will not break their sleep. Since I agree with Dr. Crookshank that sound principles are required, he will not take it amiss if I express my difficulties with complete frankness. Both in what he has said and in what Dr. Hamer has often said here, I think I can detect a certain flavour of superiority, a hint that the bacteriologist is rather a poor creature in comparison with the learned and philosophical epidemiologist. I recognize that Dr. Crookshank, recalling much that has appeared in the medical press of recent years, is entitled to say: *Semper ego auditor tantum? Numquamne reponam?* I admit that for a statistician to preach controversial forbearance and charity is a little like Satan rebuking sin. Still, I do enter the plea. We need all our wits for construction, and have none to spare for the manufacture of wounding sarcasms or the revelation of others' shortcomings. The constructive work of the immediate future must be to render precise the still vague notion of an epidemic constitution. We have devoted much attention here to this concept, yet it remains obscure. We have emphasized the need of a systematic plan of forecasting epidemiological events: yet, as Dr. Crookshank so justly remarked, our epidemiological intelligence service is hardly superior to that of a Meteorological Office which only gives warning of rain when unfurled umbrellas pass along the street. To escape from this humiliating

[1] Galen on the "Epidemics of Hippocrates" (Tertia Classis, pp. 101-2, Apud Juntas, 1609).

position, something more than advice to study the works of Sydenham or even of Ballonius will be needed. To use another of Dr. Crookshank's apt metaphors, we must organize a general staff. Upon that staff all arms of the service will be represented, and all must work loyally together. The proper sequel to and the fittest reward of Dr. Crookshank's scholarly paper is the submission here of a working plan to reduce to practice the general principles expounded. All men are pragmatists in real life; the bacteriological philosophy has "worked," it remains to demonstrate that a truer and more comprehensive epidemiological philosophy will work better still.

Dr. HAMER: Dr. Crookshank has read a very interesting metaphysical paper. As a rule I find metaphysics beyond me, but Dr. Crookshank's exposition was so clear it seemed practically indistinguishable from common sense; I was wondering in fact, as I listened, whether, just as M. Jourdain, in the play, was made to realize that he had been talking prose all his life, so I myself might perchance find that after all I was a bit of a metaphysician, though I had never before suspected it. But whither does it all tend? It seems to me that, during the last thirty years or more, we have been making fairly steady uphill progress along the road which constitutes the boundary between the county of epidemiology and the county of bacteriology, and that we have at length reached, at a height considerably above sea level, the foot of the mountain, on the very top of which lies hidden the secret of an "epidemic constitution;" and now we are face to face with a parting of ways. Straight ahead is the frowning height, its summit in cloud. On the right hand there stretches away a fine road skirting the base of the mountain. Along this road we have recently seen Dr. Brownlee whirled away in his new car "periodogram." We are all hoping to hear more from him, but as he is still insisting upon the primary, if not the exclusive, importance of continuous variation in the virulence of the germ, we have to realize that for the time being Dr. Brownlee's road is going downhill. On the other hand, on the left, there swerves away, through the territory of the old epidemiology, another fine road, which has been explored more particularly by believers in "skiey influences." So far as it has been traced this road is as flat as flat can be, but of course there is always the possibility that after a while it will begin to rise, as it skirts round the mountain, and leads to a good vantage point from which to start climbing. At the risk, however, of being laughed at, I venture to bring under notice the very rough and at first sight unpromising ground directly in front of us. Along this can be seen two obscurely marked sheep tracks proceeding at any rate onwards and upwards. One diverges slightly towards the left hand and it has been followed at various times by De Schweinitz and others, naturalists bent on collecting "ultravisible viruses;" the second track, directed rather more to the right, has been explored by Reiner Müller, Massini, Penfold, and others, workers at the problem of discontinuous variation by "mutation." As a matter of fact I have reason to believe that two travellers, each of whom

follows one of these tracks, will keep in sight of and after a while will find that they are approaching one another, and will ultimately meet at a small and retired upland farm; then after passing some dogs and following the track until clear of all stone walls, they will come right out in open ground on the face of the mountain and can start straight up the steep. But, it must here be pointed out that there remains to be considered a fourth method of approach to the mountain, the most direct of all; but that is by aëroplane and is of course only open to those trained in metaphysics and statistical methods. Investigators thus equipped are able to rise in the air, to survey with careful scrutiny the whole of the ground beneath them and to make the best use of details of information obtained by scouting parties below. It is to be hoped that at no very distant date a survey of the top of the mountain will thus become an accomplished fact. Meantime, those who cannot fly may find useful employment in examining the track beyond the farm. There is the chance there of picking up facts relating to such questions as the "parasites associated with a parasite," symbiosis, and the like; take, for example, a suggestion made 14 or 15 years ago that "the influenza organism may at one time live in association with Pfeiffer's bacillus, at another with the *Micrococcus catarrhalis*, and so on; or the throat distemper organism may be yoked now with the diphtheria bacillus and now with the *Streptococcus conglomeratus*." (Is that, I wonder, now to be regarded as a "concept" or as an "occurrence or happening?") I hope in any case that Dr. Crookshank (as an epidemiologist) will agree with me that field work of this sort may have value; and I feel sure, in the light of the paper to-night, that we may rely upon Dr. Crookshank to keep the Section in touch with what the metaphysicians and statisticians are doing in the upper regions of the air.

Dr. CROOKSHANK (in reply): I confess I am gratified that there has been so little positive disagreement with anything I have said to-night. Dr. Greenwood reproaches me, it is true, that I have not done justice to Galen. Far be it from me to find fault with Galen: exigencies of time alone have prevented me from discussing the value of the work of Galen as an epidemiologist. If Hippocrates is the first of those who have described occurrences and prevalences, so Galen stands, for us, as the first and greatest of the systematists who described "universals"—ideal forms of disease. My quarrel, as I had thought my words indicated, is only with those materialists, or realists, who have confounded the ideal forms with the happenings, and have ascribed objectivity to them. I am not conscious of the "flavour of superiority" that has been detected; and, as I took care to say, there is no need for recrimination or for sarcasm. But if some of us do sometimes poke a little gentle fun, let it not be forgotten that we have for many years endured more formidable attacks when we have ventured to hint that laboratory bacteriology is not the conclusion of the whole matter. And I say this as one who ardently studied laboratory bacteriology twenty-five years ago, and who has practised it long since. If then we, who now try to take the broader view, hear a demand for

truce, we can only reply: "*Que Messieurs les assassins commencent.*" I entirely agree with the call for synthetical work—that in fact was the motive of my paper: but I am sufficiently an Irishman to say I don't like the reciprocity to be all on one side. And, if we desire a stereoscopic view it is futile to shut one eye, or to say that only one eye gives the correct image. I repeat—and this has reference to Dr. Greenwood's adoption for the moment of the obvious standpoint of the pragmatist,—tactics are not strategy. It is not derogatory to the valour of the trench fighter, or to the skill of the bacteriologist who is armed in face of an epidemic with vaccines, to declare that there must be a strategist in the background to direct the plan of campaign, and a "philosophic" epidemiologist who may collate historical and other evidence, and who may help to resolve problems in a manner that will conduce to clear thinking and useful action. All this I thought I had indicated or at least suggested. Let us remember, too, even the data of the statistician who is concerned with epidemics are data without absolute value—they are not "specimens"—and their relative value depends entirely upon the manner in which the field-worker or "reporter" employs words and names and interprets phenomena. Curves and tables are sometimes put forward, as expressing fluctuations and variations in disease-happenings, when really they are expressive only of variations in the manner in which certain kinds of happening have been regarded at different times or by different people. The need for what is called philosophical or metaphysical discussion is therefore obvious: and, to be a realist, or materialist, in this matter of specific diseases involves no less the adoption of a "metaphysical" position than it does to be a "conceptualist." It is quite possible that, in times to come, the bacteriologist will be really grateful to the epidemiologist who, from historical or statistical inquiry, finds himself in a position to give rational warning of the approach of a pandemic or to interpret occurrences: but, before observation can be collated to that end, we must come to some agreement on this question of names, notions and happenings. In this question of influenza, for example, there are two schools: some of us, observing series of phenomena, say we find reason to believe them correlated. We are at once accused, by people who won't define what they mean by influenza, of saying that something *is* influenza which really *isn't*. How is the secular periodicity of influenza, moreover, to be determined, unless the question of the exclusion, or inclusion, of the sweating sickness, and epidemic agues, is first settled? One word more. I do not understand how any one who has observed the germination of the seeds in spring, and the fall of the leaf in autumn, can refuse to admit the influence of what is extra-telluric, on all forms of life; even upon filter-passers! Indeed, the more ardently one upholds the importance of the microbe, the more convinced an adherent should one be of the importance of climatic and seasonal influences in respect of epidemics on the grand scale. And, as indicated by the last lines of the "summary," appended to my paper, I am convinced that the free discussion of fundamental and general principles will lead to better and more fruitful work, by those who have chosen certain lines of research for their own.

Section of Epidemiology and State Medicine.

President—Dr. E. W. Goodall, O.B.E.

The Epidemiology of Acute Encephalomyelitis ("X Disease") in Australia.[1]

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This paper deals with the epidemiology of a disease which occurred in parts of the State of New South Wales, Australia, during the late summer of the years 1917 and 1918—a disease which anatomically might be designated acute encephalomyelitis, but whose distinctive features make the unknown quantity designation "X Disease" also appropriate.

The disease has formed the subject of an extensive investigation at our hands, of which a full account has been published in the Eighth Report of the N.S.W. Microbiological Laboratory for the year 1917, and to which readers are referred for clinical, histological and experimental details; for the purposes of this paper it is only necessary to recapitulate the conspicuous features.

We had clinical notes of 134 cases and found that the disease was abrupt in onset and severe. Children chiefly were affected (ninety-five cases) but adults did not escape (thirty-nine cases). General signs of cerebro-spinal irritation, namely, convulsions, rigidity, increased reflex excitability, mental obfuscation and loss of consciousness, accompanied by high fever, were the dominant bedside features. Paralysis of voluntary muscles did not occur in more than one out of ten cases. Four and a half days was the average duration of the illness, and it was fatal

[1] Read at a meeting of the Section, held January 23, 1920.

in no less than 70 per cent. of cases, fatality figures which more than double those of almost any recorded epidemic of acute poliomyelitis (infantile paralysis). Exhaustive histological examination of much human and animal material showed changes in the nervous system resembling in general respects those of acute poliomyelitis, but differing inasmuch as they were always widespread and the changes in the brain were always as great as, if not greater than, those in the spinal cord. Lastly, by a long series of experiments we have shown that the disease can be communicated by intracerebral inoculation of an emulsion of brain and spinal cord not only to the monkey but to the sheep, horse and calf. Monkeys (*Macacus rhesus*) "took" with regularity; sheep, on the other hand, showed some immunity, while experiments on horses and calves were insufficiently numerous to allow judgment on the point. We regard this susceptibility of the sheep, horse and calf as of the highest importance, because so far as we can gather the kindred disease, acute poliomyelitis, has never been experimentally communicated to any of these animals, though attempts have been made to do so.

These then were the conspicuous features of this disease as discovered at the bedside, under the microscope and by experiment. We can now proceed with our thesis, the epidemiology of the disease, which is full of interest and may be discussed under the following headings:—

(1) Season and climate.

(2) Natural diseases in animals in the affected districts.

(3) The possible existence of an intermediate (invertebrate) host.

(4) The supposed means of conveyance from human being to human being.

(5) Connexions between cases.

(6) Prevalence of cerebro-spinal meningitis and acute poliomyelitis during the years 1917 and 1918.

(7) Possible connexion with influenza.

(1) Season and Climate.

The incidence of the disease in relation to season and climate in New South Wales is remarkable. As regards the former, attention was first directed to the occurrence of cases in February, 1917, that is towards the end of the hot, dry summer season. Information afterwards obtained suggested that occasional cases had occurred in the previous December and January. More cases, indeed the majority,

appeared in March and April, and during May recognizable acute cases gradually disappeared. The disease therefore had a seasonal incidence, falling in the latter part of summer and in autumn, and apparently completely disappearing with the onset of cold winter weather. In January, 1918, cases again appeared, becoming numerous during February and March and disappearing in April, in contradistinction to the disappearance in May of the year 1917.

The areas in New South Wales in which these cases occurred usually have dry summer weather followed in the autumn by clear, bright and bracing days with cold and sometimes frosty nights. In the coastal districts of the same State the temperature in summer is lower and the humidity much greater, whilst the autumn is muggier and milder. It will be seen, therefore, that as regards season the incidence of the disease was in months characterized by relative dryness and a bracing atmosphere. Ordinary "acute poliomyelitis" (the Heine-Medin disease) has also a late summer and autumnal incidence, though apparently not necessarily requiring dry and bracing climatic conditions. Cerebro-spinal meningitis on the other hand is prevalent chiefly in the spring months of the year.

Next regarding climate: New South Wales may be roughly divided into three zones—one east of the Dividing Range where the coastal climate is found, one which has a heavy rainfall, and is mild in summer, while the autumn may be described as muggy—it is in fact a sub-tropical belt. A second, comprising the highlands of the Dividing Range, possessing a climate which in summer is warm, even hot, by day, but cool by night; in autumn dry, cool and frosty; in winter, cold and bracing with snow in places. And thirdly, there is the extensive area west of the Dividing Range which has a hot and dry summer, a mild autumn with cool or cold nights, and a winter with many bright sunny days and a cool bracing atmosphere.

Along the coastal part and on the highlands of New South Wales no indigenous cases of encephalomyelitis have with certainty been recognized.[1] The definite cases have all been confined to the western part of the State, sharply differentiated from the two other climatic sectors by its relative dryness combined with high summer temperatures. This leads up to another point which we wish to emphasize, namely, the remoteness from one another of the towns or districts affected. We may help to explain this by saying that settlement in the western part

[1] A single case occurred in Sydney after the epidemic in the country had ceased.

of New South Wales is almost entirely along railway systems which radiate from Sydney as a centre and pass through the highlands to debouch on the plains. The only exception to this is in the case of Broken Hill, situated in the extreme west of the State and the terminus of a railway radius starting from Adelaide, South Australia. There are three main railway arterial trunks reaching the western portion of New South Wales—namely, the North-western, the Western and the South-western. As yet there is little cross communication between these main trunks, or even between their subsidiary branch lines. Such inter-communication as does exist is in proximity to the highlands bounding the eastern side of the western plains. As settlement is dependent so much upon railway communication, the population is chiefly distributed in proximity to railway lines, and though small townships and large individual holdings fill up the spaces between the radii of the railway lines, the population of these parts is relatively very scanty. Communication between one main railway radius and another is almost entirely brought about through transit to Sydney and thence out by the other radius. It is true that by means of motor cars, teamsters, wagons and so on, there is a certain amount of communication between the settlements lying along adjacent radii, but the number of individuals thus passing from one to the other, compared with the number going via the metropolis, is very small. Further, the connexion between settlements along these railway radii and Broken Hill is almost entirely carried out by a devious journey, first of all to Sydney and thence to Melbourne and Adelaide and so by rail to Broken Hill. The number of individuals passing directly from the western railway lines of New South Wales to Broken Hill by motor car and other means is exceedingly small, the intervening country—several hundred miles in extent—being almost uninhabited.

We now come to the extraordinary fact that in 1917 cases of encephalomyelitis appeared, almost simultaneously, in towns along the North-western system of railways—e.g., Narrabri, Moree, Mungindi, &c., along the Western system, especially at Bourke; and at Broken Hill—three centres, hundreds of miles from one another. Apparently cases also occurred in the neighbourhood of Brisbane, and Breinl reported them at Townsville. (It is to be noted that the climatic conditions of these two Queensland towns are tropical and sub-tropical —i.e., mild and muggy, in contradistinction to the districts affected in New South Wales.) Then, again, in 1918 cases occurred, almost simultaneously, in the Narrabri district along the North-western

line; at Coonamble, on a branch of the Western line; and at Broken Hill, where the incidence was severe. Apparently the same disease was also reported from the Goulburn Valley in Victoria, a district with a cooler climate than the affected parts of New South Wales. A little later cases were reported from the South-western railway system of New South Wales, more especially from the Narrandera district. During the autumn of this year the disease apparently occurred along the Western line in Queensland, which runs out from Toowoomba; also further cases occurred in Brisbane.

Reviewing the occurrence of these groups of cases it is most important to note that the disease appeared almost simultaneously in widely separated districts having little direct communication with each other but enjoying for the most part the same kind of climate. For one of these various districts to have been infected from the other, the most likely route of conveyance of the infection was undoubtedly through Sydney. In other words Brisbane, Narrabri, Bourke, Narrandera, the Goulburn Valley in Victoria and Broken Hill are so situated that almost all passengers between these towns or districts must pass through the metropolis of Sydney. This would mean that if the virus were conveyed by human "carriers" from one district to another, these "carriers," in the great majority of instances, must have passed through Sydney before reaching the other focus of dissemination. Further, there must have been in Sydney far more "carriers" than were ever redistributed back again to the western districts of New South Wales. Presumably, therefore, Sydney must have been heavily seeded by "carriers" and yet, in spite of such heavy seeding, Sydney yielded no grave or fatal typical indigenous cases of the disease. A permissible corollary is that the chief protecting influence in Sydney were the different climatic conditions existing there compared with those of the other parts affected. It is further probable, since the disease can hardly be considered as having originated *de novo* in western New South Wales, that it was introduced originally from overseas, and that its "carriers" passed through Sydney before reaching the western parts of the State to find there a favourable nidus for the spread of the disease. As far as the evidence available shows, climatic incidence is more strikingly displayed in the case of this disease than in any other of which we are aware, except those diseases whose climatic occurrence is dependent upon the existence of an intermediate host of the virus, this host being dependent upon certain climatic factors for its development. As is shown elsewhere

the balance of evidence as regards "X disease" is very strongly against the existence of any such intermediate (invertebrate) host. Some other explanation therefore must be sought, but as yet does not seem to be forthcoming, beyond the vague meaning conveyed in the expressions "climatic" and "seasonal."

It may be noted, finally, that the incidence of the disease synchronizes with the advent of the hottest part of the summer (January), making it appear that heat is in some way necessary to the propagation of the virus. Out of which the suggestion given by Sydney Smith ("Infantile Paralysis in the Wellington Health District, New Zealand") is worthy of consideration—namely, that just as certain poisonous and disease-producing moulds thrive in warm weather, so also may the organisms of acute poliomyelitis and "X disease." Indeed for all we know it may have its normal habitat in grasses and other vegetation. The proved susceptibility of the sheep, horse and calf to "X disease," and the possibility that these animals are hosts of the virus make this suggestion more plausible. At any rate it is a speculation for experimental settlement, and we regret that we have not had the opportunity of examining the nervous systems of more animals dying under suspicious circumstances in the affected districts and of making the needful search for the virus.

(2) Natural Diseases in Animals in the Affected Districts.

As has been mentioned, we have been able by intracerebral inoculation of virus-containing material unmistakably to convey the disease to monkeys (*Macacus rhesus*), sheep, a calf and a foal. But long before these results were obtained, and, as a matter of course, we made inquiries as to the occurrence of natural cases of nervous disease in animals in the affected districts, and did so because such inquiries have nearly always been made by investigators into various outbreaks of ordinary acute poliomyelitis, with the result that in some instances obscure nervous diseases have been recorded as co-existent in domestic animals.

The mammals, domestic or wild, occurring in the districts with which we are concerned comprise the following: Horses, cattle, sheep, pigs, camels (in the extreme west), dogs, cats, rats (*Epimys rattus* or *norvegicus*), mice (*Mus musculus*) and rabbits. These come more or less into contact with human beings. In addition may be mentioned such wild animals as the dingo (*Canis dingo*), foxes and various

marsupials, which rarely come into contact with man. As regards birds, domestic poultry, including fowls, turkeys, ducks and geese, may be mentioned.

Horses.—We have received information of the occurrence of a nervous disease in horses at Narrabri. Thus Mr. Charles White, Inspector of Stock, states that more than once, at about this time of year (i.e., the beginning of March), several head of horses so affected have died in this district within a few days of each other; others recovered from the same disease. This occurred in both the years 1917 and 1918; again, several years ago, on H. Station, ten or twelve young draught horses died, but others were sick and recovered. All these were said to have had "muco-enteritis." But amongst the symptoms some were suggestive of tetanus. Thus convulsions occurred and the animal was nervous if approached and very timid. There was also a tendency to fall down, and eventually the horse would fall on one side. The conjunctiva was reddened. Recently (1918) four or five horses on the Narrabri Common had been affected and two foals had died. A post-mortem examination on one of these showed "acute muco-colitis." Mr. White also mentions that cattle at different times died in the same way. He had recently heard of two head so dying, and a man near Baan Baa had had some sick animals. These cattle sometimes became paralysed in their hind quarters.

Dr. Park, of Narrabri, has kindly supplied the following information concerning a mare of his, about 3 years old, which was taken ill on February 23, 1918. The first symptom she manifested was running round and round when driven. Two hours later the animal was very nervous, seemed always to turn to the right, had an inco-ordinate gait, at times suggestive of weakness in the hind quarters, apparently could see only very indistinctly, and had great respiratory distress. The abdominal muscles were strongly contracted, and the "whites" of the eyes pink. She was able to eat hay well. When driven to water the excitement caused by the driving apparently prevented her from going actually to the water; forty-eight hours later, however, she was able to drink. During the course of the illness she appeared to be able to see sufficiently well not to run into a fence. Though usually a quiet animal it was impossible to catch her; only on the third day could she be approached sufficiently closely to be touched, when she flinched and staggered and almost fell down. She walked with her head held high and had a tendency to turn it to the right. There was no spasm of the nictitating membrane. When standing she was continually switching her tail. There was no noticeable jaw spasm. She yawned several times, the mouth being opened widely. She improved a little during the next few days and became less excitable, though the respirations were still distressed. On February 27

she was let out to water but would not go to it, though she had not drunk since the evening before. She wandered about half a mile through the paddock, and early in the afternoon was found lying on her side in an old shallow ensilage pit. Perhaps she had gone there to roll in the damp mud. She was pulled out and struggled violently, rolling right over. During the night she died. From the condition of the ground around it was evident that she must have struggled violently.

A post-mortem examination was held at 11 a.m. on February 28, the carcass being then in a good state of preservation and not as yet "blown up." The vessels of the brain were found decidedly congested, though those of the pia mater on the dependent side of the cerebral hemispheres were more distended than those on the other side. There seemed also to be slight congestion of the vessels of the spinal cord. The abdominal and thoracic organs seemed to be normal. The lungs showed no evidence of pneumonia, the liver was normal, the spleen was not engorged, and an external examination of the intestines showed no evidence of any lesions therein. The mesenteric glands were not noticeably enlarged. Histological examination of the brain and spinal cord showed no lesions definitely suggestive of encephalo-myelitis. A monkey (A 42, M. 3820) was inoculated intracerebrally, on March 6, with a glycerine emulsion of the brain and spinal cord and remained quite unaffected. It seems clear, therefore, that this horse did not suffer from the same disease as affected the human beings.

In our notes on Case 39, 1917 series, Boggabri district, there is a reference to several horses, in the neighbourhood where the case occurred, having recently been affected with a disease in which "paralysis" appeared, several of them dying. In connexion with Case 16, 1918 series, Broken Hill district, a horse which became "stiff in the hind quarters" had died five months previously.

Sheep.—Throughout the north-western districts of this State a nervous disease known as "staggers" occurs, but a definite connexion between this and the human disease cannot be traced. Moreover, the clinical and histological features of "staggers" do not agree with those of encephalomyelitis as experimentally produced by us in the sheep. Bearing on this we submit some reports on "staggers." The following description of an outbreak is taken from the "Third Report of the Bureau of Microbiology for 1912," in an article by one of us (J. B. C.) on "Plants Poisonous to Stock":—

"An outbreak of this disease occurred amongst sheep and other animals at Emmaville in 1911, and a short account of the symptoms and the results obtained was recorded in the Second Report of this Bureau. It would appear that goats, sheep, cattle and occasionally horses had been affected and also some of the native marsupials. Certain infected paddocks seemed to be

implicated, and the fact that animals had been allowed to browse on such a paddock for a period even as short as two hours seemed from the information to be sufficient to produce the disease.

"The following account of the symptoms was compiled by Mr. Max Henry, M.R.C.V.S., of the Stock Department: 'When approached, an affected sheep which was lying down tried, but was unable, to rise. It struggled convulsively and its breathing became rapid. When left alone and watched from a distance, it rose after a time and appeared to walk stiffly and without full control of the hind quarters. The head was continually shaking up and down. The first symptom usually seen was stiffness of the hind quarters, then humping of the back, a rather marked tympanites in many cases, and convulsive fits, the animal falling down, and the legs being stiff. Later it would rise, unless it fell in a difficult place, when it would die. A goat, on being approached, ran stiffly, both hind legs were brought forward together in jumps although the fore legs trotted, and there was want of control over the hind quarters. After running a few steps it collapsed, and rolled over on its side and lay there kicking, with the head somewhat drawn back. When left alone it rose after a while, but again the up and down movement of the head was noticeable. The post-mortem appearances and sections of the tissues failed to indicate the nature of the disease.'

"Local owners regard a certain plant, which Mr. Maiden has identified as the grass, *Echinopogon ovatus*, as being responsible for the disease. Specimens of the plant collected at the time and pressed as herbarium specimens were submitted a few weeks later to microscopical examination by Dr. T. Harvey Johnston, who found mycelial strands, possibly those of an *Endoconidium*, in various parts of the seed coats, but no fructifications.

"The similarity of the symptoms of the Emmaville disease with those of La Tembladera, in the Andean region of South America, is striking, and this is accentuated by the fact that in the American disease a grass is also implicated. In this grass the mycelium of a fungus has been found, which Rivas and Zanolli have named *Endoconidium tembladeræ*, and which would seem to be closely related to *Endoconidium temulentum* of the poisonous grass *Lolium temulentum*."

In 1900 Mr. (now Professor) J. D. Stewart[1] also reported on an outbreak of "staggers" in sheep at Narrabri, in New South Wales, and mentioned shivers in horses, ascribed to *Stachys arvensis* or palm weed, as being a similar disease, especially as the symptoms only become manifest on exertion. He says that the country where the "staggers" in sheep occurs is on alluvial flats and plains of rich soil, extending back from the Namoi River, between Narrabri and Wee Waa, to a distance of about 8 miles. In the district there had been a series of severe

[1] *Agricultural Gaz. of New South Wales*, xi, p. 1112.

droughts, and the land in many places was quite bare, and in others covered with a short patchy growth with little or no grass.

In November there was a fall of rain, and some herbage came up. Since then the sheep had lived mostly on marsh mallow and the seeds of trefoil and other herbs which they picked off the ground. They kept in fair condition, and were mainly merino and Lincoln sheep or crossbreds. The disease had been noted for some time, being more prevalent during the past two years and was now fairly common. It affected all sheep from four months old and upwards. Sheep in prime condition appeared to take the disease more readily than others. Out of 500 sheep in a fold three were picked out as not being healthy by the stiff manner in which they walked. On releasing the sheep all came out lively, but after being driven 200 yards three tailed out, and walked in a peculiar stiff manner with back arched and nose poked out. After driving the flock for half a mile, about 2 per cent. showed evidence of being affected. On trying to drive an affected sheep farther it would go a short distance and throw itself down exhausted. If further urged it would rise and travel a short distance and go down again. Allowed to rest thoroughly it would get up and walk a considerable distance if not interfered with. If hustled they would go down more and more quickly. When then put on their feet the trembling or shivering of the muscles of the legs occurred. In this stage the temperature would be 105° F. to 108° F., breathing hurried (about 100 per minute) and often oral; pulse weak and rapid, 140. The mucous membrane of the mouth became blanched. The legs were perfectly mobile and the muscles supple. There was no evidence of pain. No paralysis was present, as after a rest the sheep could move freely and jump logs. The condition appeared to be one of temporary prostration attended by high temperature, weakness of the heart's action, and difficulty in breathing. Post-mortems were made on two cases; some strongyles were found in the fourth stomach of one; the spleens in both were a little enlarged and darker than normal.

A similar disease occurred at Rowena in November, 1910, but a histological examination of the nervous system showed no signs of "X Disease."

Dogs.—In connexion with Cases 21 and 22 of the 1917 series, two dogs belonging to the family are said to have died from convulsions about the time of the children's sickness. One of these, which used to follow the sheep, took fits and died, and next day the second dog also took fits and died during the following night.

Cats.—A sick cat, which had not been paralysed, is mentioned as having died a week before Case 2, 1918 series, Broken Hill, became ill.

Fowls.—Spirochætosis is common in some, if not in all, of the affected districts. Fowls actually suffering from the disease were found near a house from which a human case of encephalomyelitis had just been removed. This disease and the fowl tick (*Argas persicus*) are more fully dealt with in the next section.

(3) The Possibility of the Occurrence of an Intermediate (Invertebrate) Host of the Virus.

From time to time it has been suggested that acute poliomyelitis might be transmitted from victim to victim by means of such an invertebrate host of the virus as *Stomoxys calcitrans*, the stable fly. But whilst in occasional instances transmission by such an insect seems to have been achieved, the balance of evidence, both from experiment and observation, is very strongly against such an insect transmitter being the ordinary means of conveyance. In the disease under consideration it was nevertheless necessary to hold under suspicion and seek out every such possible potential intermediate host.

An invertebrate host, if such exists, may conceivably play one or more of the following rôles: The invertebrate may transmit the virus from human case or human "carrier" to human case, or it may transmit the virus from animal reservoir to a human case; further, the invertebrate host may transmit the virus by blood-sucking, or indirectly by the ingestion of infected material from discharges; still further, the virus may conceivably multiply in the supposed invertebrate host, either by undergoing a cycle of development or merely directly increasing in number; or finally, the invertebrate may merely act as a temporary vehicle just like an inanimate object.

It was first necessary to ascertain the occurrence of all invertebrates that might conceivably play one or other of the above mentioned rôles. Careful investigation showed the presence of the following invertebrates, coming under this category, in the affected districts, namely fleas, probably *Ctenocephalus canis* and *Pulex irritans;* bed bugs, *Cimex lectularius;* the stable fly, *Stomoxys calcitrans;* mosquitoes, *Culex fatigans* and probably *Culicelsa annulirostris*, and though rarely *Nyssorhynchus annulipes*, and perhaps some other species; tabanids; head and probably body lice—all of these being known suckers of human blood; fowl ticks, *Argas persicus*, which probably occur throughout the affected

districts, but are not with certainty known to bite man in Australia; the house fly, *Musca domestica*, common in houses throughout the affected districts, and having access to the excretions of man; *Fannia canicularis*, the lesser house fly, probably chiefly outside houses; the bush fly, *Musca vetustissima*, which commonly and especially delights in sucking the secretion from the eyes; the blow flies *Anestellorhena augur* and *Pollenia stygia*, probably widely distributed throughout the district and having a special predilection for feeding on human fæces as well as blowing live sheep.

As far as we can see these comprise all the invertebrates that can with reason be considered as possibly playing the part of an intermediate host.

In reviewing these invertebrates it will be seen that they can be divided into two classes, namely, those which remain permanently in the neighbourhood in which they are produced, and those which are easily transported from one locality to another. Manifestly those that remain in the neighbourhood of their development, could not, except under very rare and accidental conditions, be responsible for the transportation of the disease to a new locality, though they might be responsible for the spread of the virus when once introduced. Thus *Stomoxys calcitrans*, house flies, blow flies, March flies, mosquitoes and fowl ticks are only rarely transported from one locality to another; they are in fact transported by man over long distances so infrequently that they could not reasonably account for the distribution of so many cases miles away from other human habitations and in country towns in so many districts.

The fleas mentioned, though occurring, cannot be considered numerous in the affected districts, and though frequently carried from locality to locality by man, cannot reasonably be held responsible for the spread of the epidemic.

Bed bugs are very frequently distributed in luggage from one centre to another. Dissemination of the disease by their means, however, would be slight; moreover cases of the disease have occurred in isolated households in which the occurrence of bugs has been denied and in which the truth of the statement was confirmed by personal and particular inquiry.

Particular attention was directed to the fowl tick, *Argas persicus*, as being the possible transmitting agent of the virus. This tick is well known as the conveyer of a specific fever of unknown nature in Persia. A short account of the fever is given in Castellani and

Chalmers' "Manual of Tropical Medicine," (second edition, p. 949). Manson has suggested the possibility of this disease being due to a spirochæte. Castellani and Chalmers state that knowledge of the symptoms is very meagre. They add that severe pain is felt at the site of the tick bite, and that this is followed by remittent fever lasting from a few days to several weeks, accompanied by delirium and convulsions, and even at times ending in death. This vague description does not fit that of "X disease," though the occurrence of convulsions in both is of interest.

Our attention was specially focused on the fowl ticks by the knowledge that these are very widely distributed in the affected districts, whilst their ravages amongst fowls appear to be little known in such an unaffected district as Sydney. These ticks, however, occur in many other parts of Australia, where cases of the human disease did not appear. It was, however, a point of interest to note that at Narrabri and Boggabri fowls were dying from the spirochætosis transmitted by fowl ticks, whilst cases of encephalomyelitis were occurring in man in the same places. As a remote possibility it was thought possible that the *Spiroschaudinnia marchouxi* of Nuttall, responsible for the disease in fowls, might have been accidentally transmitted to human beings and have been capable of multiplication in them and of producing consequent disease. In this connexion Levaditi sections were made of portions of the brain or spinal cord from two human cases, but spirochætes were not detected; also some fowl ticks were collected at Boggabri in March, 1918, close to a house in which a fatal case of "X disease" had occurred. An emulsion of these ticks was made and injected intracerebrally into a monkey (A43, M.3827) but without producing any ill effects. The human disease was later conveyed to this monkey by the inoculation of further material derived from another monkey, showing that it was not immune. It may be pointed out that the histological lesions occurring in encephalomyelitis are of a kind that might be expected to arise were a spirochæte responsible for the disease.

From this short review of all possible intermediate vectors, it will be seen that none can be looked at with even the slightest suspicion as playing such a rôle. There have probably been few investigations into diseases such as this in which such intermediate vectors can with greater likelihood be eliminated. This is due to the sparsely-populated nature of much of the affected country, and to the relative absence of noxious invertebrates in the neighbourhood of man in this part of Australia.

(4) The Supposed Means of Conveyance from Human Being to Human Being.

Though we have shown that amongst domestic animals in Australia no natural disease can, as yet, be recognized as resembling encephalomyelitis ("X disease") of man, this does not necessarily exclude the natural occurrence of the virus in some of such animals. The virus might or might not produce recognizable symptoms in such an animal host, and yet, on transference to a human being, it might be capable of producing lesions. Assessing the known facts, however, there does not seem any reasonable likelihood of such an animal reservoir for the virus. Though in the majority of the cases no apparent association, direct or indirect, with other cases can be established, yet, in a few instances, such association seems clear and distinct. Details of these are given in the next section.

Failing, therefore, any regular and definite association between new and antecedent cases, human intermediaries, if existing and necessary for the distribution of the disease, must have manifested no signs, or very slight signs of illness, and been in many cases secondary or more remote carriers. The extensive seeding of widely separated areas, connected chiefly with each other through the metropolitan district, which was unaffected, suggests that human carriers were numerous, and also, consequently, that those individuals developing the disease formed but a small portion of those who actually harboured the virus from time to time.

Our experimental work in sheep would suggest that in the case of man, even after the virus has gained entrance to the nervous system, it is possible that it may not be capable in certain individuals of producing any signs of the disease; and it may be that only here and there a susceptible person is met with who reacts to the virus in a way provocative of disease, assuming that the intense congestion and the perivenous cellular sheaths in the central nervous system are chiefly responsible for the signs and symptoms.

As the evidence seems fairly conclusive that there is no insect vector responsible for the disease, and as there is no evidence of an animal reservoir of the virus, it seems impossible to avoid coming to the conclusion that the ordinary means of spreading is by human carriers, who are often secondary or tertiary, or perhaps still further removed from the previous case.

(5) Possible Connexions between Cases.

1917.

Cases 5, 12, *and* 13, occurring at Menindie, show a remarkable association with each other through a fourth person, who would seem to have been an intermediary, and who, it is stated, eventually developed the disease himself.

Cases 19, 20, 21, 22, 23, 24 *and* 41 (Cases 19 and 20 were brother and sister, as were Cases 21 and 22).—In connexion with this group of cases, the following is suggestive of conveyance of the disease by the intermediation of a carrier. The information was supplied by a patient who had recovered from the disease, and was apparently well-known in the locality in which the incident occurred. It appears that after the death of one of the cases a religious service was held at which two boys were in attendance. Shortly afterwards one of these two boys developed the disease. There was no suggestion that this boy had seen the patient whilst alive or had actually come in contact with the dead body in its coffin. It is more probable that the infection was brought about by the intermediation of a carrier amongst members of the bereaved family in attendance.

As regards Cases 21 and 22 (N. family), the second child of which took ill on March 6 and died on March 10, a woman who had been living at N.'s house when the first of the two children became ill called later at West Narrabri on Mrs. M., the mother of Case 24. Case 24 became ill on March 23 and died on April 1.

Members of the N. family visited the family D. in whose house the second case of the N. family became ill. The G. family (Case 23) were friends of the D. family, and Case 23 became ill on March 23 and died on March 28.

Cases 44 *and* 46.—These two children had been closely associated in play at Moree. The first became ill about February 19, and the second about March 3 or 4.

Cases 55 *and* 56.—These again were brother and sister. They became ill apparently almost simultaneously.

The following information is also available. The body of a child, M., was seen by C. just after death had occurred. Whilst at the house he was called for and taken in a motor car to another house. The youth who drove the car became ill about a week later and died apparently from this disease. He had not gone into the house in which the dead body was lying.

1918.

Cases 1, 11, *and* 22.—Case 1 was connected by intermediaries with Cases 11 and 22.

Case 4.—This patient appears to have been brought into close contact with another presumed case. A sister also apparently developed the disease.

Cases 5 *and* 25.—There is possible connexion between these two cases, the latter developing the disease a fortnight after the former.

Cases 5, 9 *and* 19.—There seems to have been some connexion between these three cases.

Case 14.—A significant point in this case is that three or four weeks before the child became ill another child had frequently played and even shared fruit with him. This child had left Broken Hill sixteen days before Case 14 became ill, and apparently developed the same disease as Case 14 at about the same time at Mildura.

Cases 20 *and* 21.—These were sister and brother, and developed the disease almost simultaneously.

Case 22.—Case 17, an earlier case, had lived in a hut beside the house occupied by Case 22.

Case 23.—There is a possibility of this case being indirectly connected with Cases 1, 11 and 13.

Case 26.—There is possible connexion between this case and two previous cases, perhaps of encephalomyelitis, in the family J., through the intermediation of a 'bus-driver at Narrabri.

Cases 26 *and* 31.—The mother of Case 31 helped to nurse Case 26, who died on January 13, Case 31 becoming ill on March 2.

Cases 27 *and* 28.—These two families were very friendly with each other.

Cases 32 *and* 33.—These children had possibly been closely associated some days before they became ill.

Cases 36 *and* 37.—The father of Case 37 was a groom at the hotel where the mother of Case 36 stayed during her child's illness. Case 37 became ill about five weeks after Case 36.

Case 47.—There is an interesting comment attached to this case, showing its possible connexion with two previous cases, presumably of this disease. After Easter, 1917, a child died from a complaint very suggestive of encephalomyelitis. Shortly afterwards the family, with the three other children, stayed at Warren at the house belonging to the parents of Case 47. While staying there another child of the

same family became ill, but recovered quickly. Case 47 did not become ill until March 16 of the following year. These facts suggest the possibility of the visiting family having left a legacy of carriers at Warren, perhaps actually in the family of Case 47.

Case 57.—There is a possibility that by means of intermediaries this child, living in the Hay district, was infected from the disease at Boggabri where the parents had relatives.

(6) The Prevalence of Cerebro-spinal Meningitis and of Acute Poliomyelitis (Infantile Paralysis) during the years 1917 and 1918.

In New South Wales both cerebro-spinal meningitis and infantile paralysis are notifiable diseases. The occurrence and distribution for the whole State of cases, considered by the practitioner as one or the other, is therefore well-known during the course of these two epidemics of "X disease" in the period preceding the first, in the interval between them, and in the months succeeding the second.

Table showing the Weekly Incidence of Notifications of Cases of Cerebro-spinal Meningitis and Infantile Paralysis in New South Wales from October, 1915, to November, 1918.

Week ended		Cerebro-spinal meningitis (notifiable since October 2, 1915)	Infantile paralysis (notifiable since 1912)	
		1915		
October	2	—		
,,	9	—		
,,	16	1		5
,,	23	3		
,,	30	2		
November	6	2		
,,	13	5		9
,,	20	4		
,,	27	4		
December	4	2		
,,	11	2		25
,,	18	1		
,,	25	3		
		1916		
January	1	1		—
,,	8	1		7
,,	15	1		1
,,	22	2		16
,,	29	2		18
February	5	1		12
,,	12	1		28
,,	19	2		8
,,	26	—		17
March	4	1		15
,,	11	1		20
,,	18	—		25
,,	25	2		21
April	1	1		14
,,	8	1		12
,,	15	5		17
,,	22	2		7
,,	29	2		13
May	6	5		8
,,	13	1		5
,,	20	2		4
,,	27	7		5
June	3	3		11
,,	10	4		2
,,	17	6		3
,,	24	4		1
July	1	1		1
,,	8	5		2
,,	15	4		—
,,	22	8		1
,,	29	13		1
August	5	15		—
,,	12	23		1
,,	19	9		1
,,	26	16		—

TABLE—(*continued*).

Week ended	Cerebro-spinal meningitis (notifiable since October 2, 1915)	Infantile paralysis (notifiable since 1912)
September 2	14	2
,, 9	14	—
,, 16	14	—
,, 23	8	—
,, 30	20	1
October 7	9	—
,, 14	7	—
,, 21	6	—
,, 28	10	—
November 4	3	1
,, 11	12	1
,, 18	5	2
,, 25	5	1
December 2	10	1
,, 9	6	1
,, 16	5	—
,, 23	5	1
,, 30	2	—
1917		
January 6	7	—
,, 13	5	—
,, 20	2	2
,, 27	4	1
February 3	3	3
,, 10	3	—
,, 17	1	1
,, 24	2	1
March 3	6	—
,, 10	9	—
,, 17	16	—
,, 24	7	1
,, 31	20	—
April 7	7	—
,, 14	11	1
,, 21	8	1
,, 28	6	—
May 5	3	1
,, 12	4	1
,, 19	2	—
,, 26	4	—
June 2	5	—
,, 9	—	—
,, 16	2	—
,, 23	1	—
,, 30	—	—
July 7	1	—
,, 14	1	—
,, 21	1	—
,, 28	2	—
August 4	3	—
,, 11	6	—
,, 18	1	—
,, 25	4	—
September 1	1	1
,, 8	7	—
,, 15	2	—
,, 22	2	—
,, 29	1	—
October 6	2	—
,, 13	4	—
,, 20	—	—
,, 27	—	—
November 3	—	—
,, 10	1	—
,, 17	2	—
,, 24	1	—
December 1	2	—
,, 8	2	—
,, 15	3	—
,, 22	—	—
,, 29	1	1
1918		
January 5	1	—
,, 12	4	—
,, 19	2	1
,, 26	1	1
February 2	2	—
,, 9	10	—
,, 16	6	2
,, 23	4	2
March 2	6	—
,, 9	3	1
,, 16	4	1
,, 23	2	—
,, 30	2	1
April 6	2	2
,, 13	1	3
,, 20	2	3
,, 27	1	2
May 4	1	1
,, 11	1	1
,, 18	1	2
,, 25	2	2
June 1	—	3
,, 8	1	1
,, 15	5	1
,, 22	2	—
,, 29	3	2
July 6	2	—
,, 13	3	1
,, 20	4	—
,, 27	4	—
August 3	1	2
,, 10	3	1
,, 17	—	4
,, 24	2	—
,, 31	5	1
September 7	—	—
,, 14	3	—
,, 21	7	1
,, 28	5	3
October 5	3	—
,, 12	3	—
,, 19	2	2
,, 26	1	—
November 2	—	—
,, 9	1	—

The preceding compound table has been prepared to show the official notifications of cases considered by the attending practitioners to be either cerebro-spinal meningitis or infantile paralysis. A list has also been prepared (*vide* official report [1]) of those cases which we have either proved by histological examination to be encephalomyelitis ("X disease") or which from the clinical history and surroundings were almost certainly instances of this disease. This table will bear some discussion. First, as regards the notifications of cases of cerebro-spinal meningitis, it will be seen that in the year 1917, the Metropolitan district and Hunter River district both being excluded, those districts in which there were an unusual number of notifications were the districts affected by encephalomyelitis. Thus the relatively heavy incidence in the North-western district is a striking example, and it will be seen that many of the cases reported originally as cerebro-spinal meningitis were afterwards considered to be undoubted instances of "X disease." It is probable that the majority of those about which we have been unable to obtain further information were also similarly instances of this disease. The reason that in 1917 many cases were notified as cerebro-spinal meningitis was the inability at that time to place them in any other category. The clinical course of "X-disease" is almost identical with that of cerebro-spinal meningitis, the only point of difference being that on lumbar puncture clear fluid, instead of turbid fluid containing meningococci, is obtained. Practitioners therefore adopted the safe course of notifying them as cerebro-spinal meningitis, and it was only later when it was clearly established that the disease was not cerebro-spinal meningitis that such notifications ceased. It will be seen as regards this district that whilst the notifications of supposed cases of cerebro-spinal meningitis were numerous during 1917, they were very few during 1918, and yet cases of encephalomyelitis occurred in some of the same towns. Turning to the year 1918, it will be noticed that there was a slight increase in reported cases of cerebro-spinal meningitis in Yanko Shire, and it is very significant that during this year undoubted cases of "X disease" occurred in this district for the first time. The inference is that a certain number of notifications of cerebro-spinal meningitis during 1918 were really instances of "X disease," as had been the case in the North-western district in the previous year.

Taken as a whole, it will be seen that undoubted cases of cerebro-spinal meningitis, probably due to the meningococcus, were comparatively

[1] Referred to on p. 185.

few in New South Wales during the years 1917 and 1918 when the outbreaks of "X disease" occurred.

As regards acute poliomyelitis (infantile paralysis), the official notifications bring out a point of very great importance. It will be seen that during the two years, 1917 and 1918, cases recognized by the general practitioner as being instances of this disease were remarkably few and were more or less uniformly distributed throughout the whole State. No more cases were reported during either year in the districts affected with "X disease" than in any other portion of the State. In other words, in the districts affected by encephalomyelitis ("X disease") there was no undue prevalence of cases of ordinary infantile paralysis. Nothing can be more striking than the experience in the North-western district, where, during 1917, there were at least fifty-one cases of "X disease" and absolutely no cases of infantile paralysis. In 1918, again there were fifteen cases of "X disease" and only two notifications of infantile paralysis. The inference that might be drawn from these notifications is that the two diseases were distinct and not merely different focal manifestations of one and the same virus.

(7) The Possibility of a Connexion with Influenza.

We should not have thought it necessary to write under this caption had it not been suggested by others in discussing the British epidemic (1918) of "epidemic stupor" (encephalitis lethargica) that not only this but also the "mysterious disease" of Australia may be identical with the mysterious "Nona," described during the great influenza epidemic of the year 1890: in other words, that they all represent special varieties of influenza. In regard to this suggestion we can only say that the "mysterious disease" (encephalomyelitis, "X,") can be coupled with neither the "epidemic stupor" nor influenza in Australia. We have not seen cases of "epidemic stupor," but gather from various publications that it is a form of infection which has a special tendency to attack the oculo-motor nuclei, and is specially associated with lethargy and stupor. Now, while somnolence is admittedly present in some cases of "X disease," oculo-motor paralysis is remarkable for its complete absence, and there are numerous other differences, while, as concerns influenza during the years 1917 and 1918, there was no epidemic of this disease in Australia. We may add that at the time of writing (March, 1919), influenza is prevalent, but cases of "X disease" are not numerous.

SUMMARY.

(1) The paper deals with the epidemiology of outbreaks of acute encephalomyelitis ("X disease") occurring in New South Wales, Australia, during the year 1917 and 1918.

(2) The outbreaks occurred during the late summer and early autumn of both years and were confined to "outback" districts where the climate is hot and dry.

(3) Although the districts which suffered are remote from each other, and only connected with one another by long radiary lines from the metropolis (Sydney) they were stricken simultaneously. And, since there were no cases in Sydney, the "carrier" theory will hardly explain the initial dissemination, but regarding the conveyance of the disease from case to case in the affected districts, some evidence is produced of the responsibility of human carriers.

(4) A careful study of natural diseases in animals in the affected districts did not assist in tracing the disease to its origin. But since the disease was proved to be communicable to sheep, horse and calf, there is the bare possibility that these animals as well as the human beings may be hosts of the virus.

(5) An intermediate vector in the shape of an invertebrate host (flies, mosquitoes, lice, &c.) could be almost certainly excluded.

(6) During the outbreaks of "X disease" there were remarkably few cases of cerebro-spinal meningitis and infantile paralysis (both of them notifiable diseases in New South Wales) and the State was free from influenza.

Section of Epidemiology and State Medicine.

President—Dr. E. W. Goodall, O.B.E.

An Investigation into the Periodicity of Epidemics of Whooping-cough from 1870-1910 by Means of the Periodogram.[1]

By Matthew Young, M.D.

(Statistical Department, Medical Research Committee.)

It has been known for many years that epidemics of whooping-cough, like those of many other infectious diseases, show a tendency to recur at more or less regular intervals. These intervals in whooping-cough are generally stated to vary from two to three years. Comparatively recently another method of investigation has become available, making it possible to determine this periodicity more accurately than hitherto. The method is known as that of the "periodogram," and was devised and developed by Professor Schuster [1], who, with others, made use of it in the investigation of periodicity in the recurrence of various physical phenomena—viz., sunspots, &c. The application of the method to the elucidation of the problems of periodicity in the field of epidemiology we owe to Dr. Brownlee [2], who was the first to realize its possibilities in this direction, and who has contributed several papers on the subject, including an exhaustive investigation by the method into the periodicity of measles. For a completely detailed account of the principle and numerous illustrations of its application the reader is referred to these papers. Briefly, the method is based essentially on the fact that the existence of a hidden periodicity in any long series of figures or statistics can be demonstrated by writing the figures consecutively in rows or periods of gradually increasing length and summing the columns of each set of figures

[1] Read at a meeting of the Section, held February 27, 1920.

thus obtained. On a comparison of the totals of the columns in the different groups of figures for the periods of varying length, it is seen that where the number of columns coincides approximately with the length of a definite periodicity in the series of figures, the range between maximum and minimum is great, whereas if the number of columns diverges from the length of the period the totals may be of nearly equal value. By fitting a Fourier series to the totals of the columns for each of the different sets of summations amplitudes are calculated. A study of the values of these amplitudes determines the lengths of the probable periodicities.

The data for the present investigation are taken from the weekly returns, issued by the Registrar-General of England and Wales, of the deaths from whooping-cough in eleven large towns, for the years 1870-1910. It was not till 1870 that weekly returns of deaths from individual infectious diseases for the large towns were first made, so that the investigation cannot be continued further back than this year. In the case of London, however, statistics are available from the year 1840. The towns selected for investigation were London, Bristol, Liverpool, Manchester, Salford, Sheffield, Newcastle and Birmingham in England, Glasgow and Edinburgh in Scotland, and Dublin in Ireland, towns which show considerable variety in climatic conditions and in industrial activities. The figures made use of in the investigation are those of the actual number of deaths that occurred. Except in the case of London, no correction has been made for the gradual increase that has taken place in the population of the various towns from year to year.

It might be contended that as there is less than a 5 per cent. case-mortality from whooping-cough, the data from deaths may not give results comparable with those obtainable from a consideration of the number of cases. According to Hirsch [4] it is impossible to make out how far the maxima and minima of the death-rate from whooping-cough may depend upon the larger or smaller number of the sick, inasmuch as there is a lack of data as to the number of cases for nearly all the localities; where there are such returns it is impossible to find any correspondence between the maxima and minima of the sickness and those of the mortality, on comparing the figures for each. Statistics of the number of cases that occurred weekly in any of the towns chosen are, moreover, not available. In the city of Aberdeen notification existed, however, for the twenty years 1883-1902. It is found as for measles before that also in the case of

whooping-cough the number of deaths may be accepted as a fairly reliable index of the prevalence of the disease. The following figures (Table I) taken from the paper by Dr. Laing [3] show the number of cases and deaths in the different months of the year, taking the average of twelve years 1889-1900.

TABLE I.

	Number of cases	Number of deaths
January	1,628	82
February	1,524	106
March	2,028	102
April	2,092	133
May	1,582	103
June	1,390	67
July	1,407	67
August	1,361	68
September	758	41
October	787	31
November	956	37
December	1,210	49
Total	16,723	886

A chart showing the death-rate per 100,000 of population in each month, given by Dr. Laing, shows a close resemblance to that which exhibits the prevalence of the disease.

The method of investigation has been as follows. The figures representing the weekly number of deaths given in the returns of the Registrar-General for each of the eleven towns selected were transferred to Power's tabulating cards by the punching machine, each card recording the figures for an individual week for the whole of the towns named. The cards were first arranged in groups of 80 weeks, and suitably distributed by "space and total" cards. They were then passed through the tabulating machine. By this means the totals of the columns for the period of 41 years for each of the eleven towns were automatically printed. The process was repeated to obtain the totals for the weekly figures arranged in 81 columns, and was continued till the totals obtained by summing the numbers of deaths at intervals of one week from 80 to 100 weeks, and at two-weekly or four-weekly intervals from 120 to 160 weeks had been recorded for each town. By utilizing the tabulating machine the labour of writing out the figures again and again to obtain the totals for each period from 80 to 160 weeks was avoided. All that was necessary was to re-arrange the punched cards into groups of whatever period was being summed. By having a duplicate set of punched cards much time was saved.

One set was arranged and subdivided by "total" cards while the other set was passing through the tabulating machine.

The series of totals obtained for the different periods were now reduced to twenty terms (*see* Dr. Brownlee "Periodicities of Epidemics

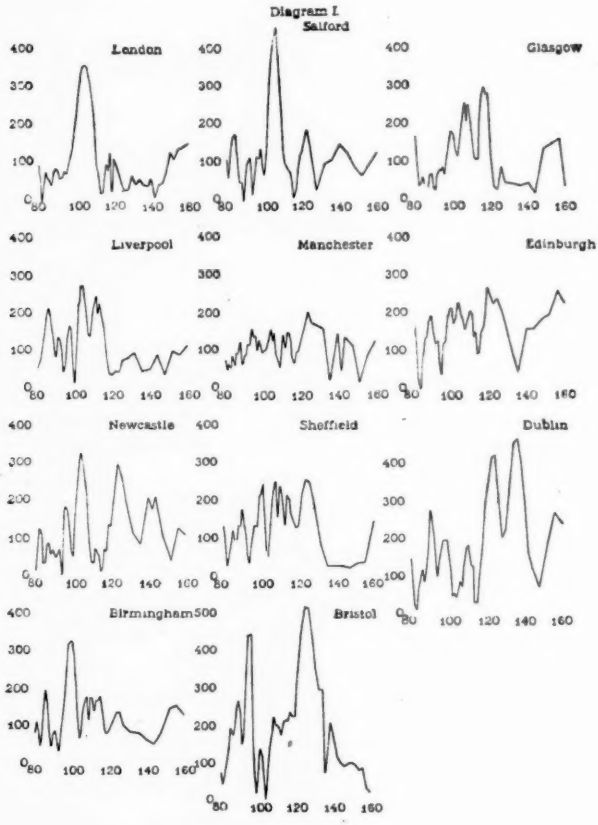


DIAGRAM I.

This diagram shows the graphs of amplitudes constructed for the eleven large towns named in the text. For each town the amplitudes of the first harmonic were calculated at intervals of one week from 80 to 120 weeks, and at two-weekly and four-weekly intervals from 120 to 160 weeks. The figures from which the graphs have been drawn are shown in Table II.

of Measles," Appendix IV, p. 38), and from these the co-efficients of the sine and cosine of the first harmonic with the corresponding amplitudes were calculated. In the case of some of the towns these

TABLE II.—SHOWING THE AMPLITUDES IN THE PERIODOGRAM ANALYSIS OF EPIDEMICS OF MEASLES IN THE GREAT TOWNS OF ENGLAND, SCOTLAND AND IRELAND.

Weeks	1870-1910 London Amplitude	1870-1910 Liverpool Amplitude	1870-1910 Newcastle Amplitude	1870-1910 Salford Amplitude	1870-1910 Manchester Amplitude	1870-1910 Sheffield Amplitude	1870-1910 Birmingham Amplitude	1870-1910 Bristol Amplitude	1870-1910 Glasgow Amplitude	1870-1910 Edinburgh Amplitude	1870-1910 Dublin Amplitude
80	100·1	61·9	24·3	111·9	81·5	144·9	92·1	83·8	178·0	182·9	160·3
81	62·8	77·6	87·3	64·7	59·5	116·4	120·5	52·1	110·4	167·4	109·7
82	2·8	93 4	138·8	135·4	74·6	38·2	96·3	72·6	43·2	120·5	32·6
83	45·2	128·8	120·3	171·9	63·3	67·0	57·1	109·9	53·6	40·0	20·3
84	81·4	158·2	40·6	181·6	95·0	92·4	85·7	154·4	72·5	7·9	76·8
85	68·8	208·7	46·1	131·3	69·6	132·6	153·4	204·7	52·2	69 2	99·3
86	61·4	221·4	93·1	102·9	101·3	101·4	203·4	182·5	51·9	132·9	129·1
87	49·9	205·9	99·3	58·4	104·0	105·0	172·8	208·4	42·1	145·5	94·4
88	83·1	161·0	69·8	57·8	130 9	141·2	98·3	258·7	77·8	185·1	118·9
89	90·6	116·5	80·9	5 7	71·8	143 8	52 7	279 9	79 6	200·8	175·1
90	86·5	94·9	71·7	60·3	90·3	183·4	89·1	243·5	43 6	173·5	282·7
91	65·0	146·3	56 5	110·0	99·3	140·0	97·4	160·5	36·8	152·1	260·2
92	66·9	137·1	72 6	119·3	124·2	124·8	77·3	188 9	72·5	126·7	226·9
93	70·5	100·1	84·7	90·8	122·6	70·2	42·8	346·2	90·6	131·4	158·3
94	84·4	51·1	13·8	22·5	165·6	43·8	84·4	450·8	87·1	84 0	109·5
95	80·1	78·3	180·4	70·6	146·3	93·8	138·7	456·4	98·6	45·8	154·7
96	102 8	148·2	193·2	123·4	148·4	113·4	194·0	407·4	78·8	125 2	176·8
97	128·2	178·2	183·6	120·3	108·6	144·2	252·3	276·4	117·7	130·5	205·0
98	151·6	167·3	144·7	141·2	138·1	142 0	324·2	94·2	168·9	189·9	205·5
99	196·6	81·8	80·0	89 8	111·5	221·4	337·1	29·4	189·2	220·0	205·8
100	236·3	21·1	62·2	73·4	104·5	228·8	336·5	110·5	182·1	220·2	170·3
101	280·7	117·3	155·3	144·4	110·6	255 0	302·0	149·3	146·6	180·9	124·6
102	335·3	234·8	244·7	263·1	114·3	168·6	226·5	126·0	124·0	196·9	56 6
103	360·4	280 0	320·9	357·5	137·5	75·8	139·1	62 7	153·6	234·0	63·1
104	365·3	282·1	338 6	418 3	164·7	60·4	78·4	11·1	186·2	226·2	54·9
105	364·7	254·4	311·9	446·8	135·9	119·0	98·0	87·2	232·2	205·0	71·5
106	355·6	215·2	277·8	468·1	155·3	207·6	147·0	152·4	265·5	192·7	96·7
107	327·7	151·5	227·5	394·6	98·7	244·2	174·0	186·9	218·4	166 1	80·6
108	295·3	146·6	145·6	322 6	80·4	259·4	186 5	234·5	262·4	180·9	134·6
109	253·4	193·8	96·0	242 3	63·4	168·0	126·9	210·9	217·8	195·1	183·2
110	186·4	225·1	42·5	143 3	106·5	245 0	183·4	212·4	172·7	217 2	195·2
111	100·8	254·4	42·8	107·5	150·5	225·2	181·0	205·0	121·8	208·0	163·0
112	73·3	206·1	84·6	101·1	114·3	203·0	149·8	183·8	114·0	148·7	139·4
113	28·9	236·3	70·5	86·9	157 9	147·6	178·6	220·1	118·8	155·9	137·9
114	27·5	213·8	65·5	81·6	157·7	222 6	178·1	222·0	209 3	100·0	40·9
115	80·4	184·0	19·1	55·1	133·2	211·2	189·0	227·4	282·3	106·7	40·6
116	102·3	148·3	33·0	13·6	95·3	177·0	162·9	222 7	307·2	158 8	101·5
117	84·5	90·6	82·4	34·4	76 3	168·4	123·9	249·0	289·9	171 2	150·8
118	133·9	56·1	72·7	88 3	101·3	145·0	100·3	238·9	291·0	203·0	218·2
119	30·0	46·7	149·3	129·0	108·0	139·2	90·2	238·8	245·1	276·5	307·0
120	117·8	44·2	144·6	138·4	129 6	144·2	95·2	325·6	143·3	261·6	344·3
122	92·9	57·0	223·6	191·9	174·6	224·0	121·9	455·4	47·0	237·4	424·1
124	59·1	54·9	308·6	158·2	213·1	266·5	147·5	530 8	36·3	247·9	429·7
126	32·8	81·9	276·7	102·3	183·4	259·1	148·1	528·6	97·2	222·3	323·0
128	37·8	89·0	211·9	31·0	180·4	212·2	110·0	455·9	57·8	194·8	211·6
130	72·7	—	—	—	—	—	—	360·9	—	—	234·8
132	50·9	101·0	124·4	102·7	167·3	100·0	95·8	308·2	51·9	114·8	366·8
134	61·3	—	—	—	—	—	—	308·1	—	—	464·5
136	50·0	54·1	98 6	115·9	32·6	36·4	93·5	86·0	48·9	51·1	473·4
138	47·7	—	—	—	—	—	—	220·3	—	—	406·7
140	62·9	59·2	216·0	159 8	153·1	37·4	77·7	182·8	53·4	167·5	301·4
142	15·5	—	188·2	—	53·3	—	—	133·1	—	—	179·2
144	48·5	98·4	220·5	136·4	144·4	37·8	64·5	111·0	26·8	169·6	130·6
146	54·6	—	—	—	—	—	—	103·3	—	—	—
148	89·5	47 7	113·7	98·1	121·3	33·9	91·1	109·4	147·2	191·1	81·2
150	135·5	—	—	—	—	—	—	111·1	—	—	—
152	115·7	107 4	55·6	75·3	28·4	43·1	155·8	108·6	159·6	203·8	197·2
154	140 9	—	—	—	—	—	—	94·7	—	—	—
156	145·4	98·9	139·3	102·0	97·5	49·1	165·6	99·0	171·4	268·1	280·7
158	153·7	—	—	—	—	—	—	46·0	—	—	—
160	159·5	121·5	119·3	137 8	137·1	156·8	139·9	34·3	45·0	237·1	249·8

values were obtained by arithmetic, but in others they were estimated graphically by the aid of the planimeter from specially prepared sine paper, on which the quantities had been graphed. A close correspondence was found to exist between the results obtained by the arithmetical and the graphical methods, the difference being no more than 3 or 4 per cent.

The amplitudes having been calculated in the manner indicated, a graph of amplitudes was drawn for each of the towns under investigation showing the varying range of this measure from 80 to 160 weeks. These graphs are shown in Diagram I and the amplitudes in Table II. It is considered best to describe in detail in the first place the graph of amplitudes plotted for London, because of the greater length of time for which statistics exist, then to give an account of the graphs for the other towns under investigation, and to compare the features in which the graphs of these resemble and those in which they diverge from that of London.

The graph of amplitudes constructed from the data for London is divided into two parts: the first part for the epoch 1840-69, the second for the epoch 1870-1910. The outstanding feature in the second section of the graph of amplitudes is the large amplitude, the maximum point on the graph, at 104 weeks, or two years exactly. The value of this amplitude is 365. A period of approximately two years seems to be the main one in the range of the graph from 80 to 160 weeks, connoting a periodicity of about 104 weeks in the occurrence of epidemics of whooping-cough in London. At no other point in the graph is there an amplitude in any degree comparable to this, the next greatest amplitude being that shown at 160 weeks with a value of 155. From the second harmonic the amplitudes from 40 to 80 weeks were graphed. In this section of the graph the only large amplitude is that at 52 weeks. The value is 485. This measures the seasonal periodicity of the death-rate from whooping-cough which attains, as is well known, its maximum annual prevalence in March and April of each year.

As a preliminary proceeding to the interpretation of the graph it is necessary and important to apply two criteria to the elevations shown on it. In one test the square of the amplitude at each apparent period is compared with the mean square of the amplitudes at another section of the graph where no period is probable, and the value of the ratio between the two determines whether the first amplitude is of sufficient magnitude to indicate a real or significant periodicity or whether it is such as may

be produced by chance or accidental circumstances. By reference to the table constructed by Professor Schuster [1] the exact probability of the independent existence of the period is obtained. The value of the ratio in this case is over 40, indicating immeasurably great odds in favour of the existence of a true periodicity. The second test enables us to distinguish whether the range of amplitudes found in the

Diagram II.

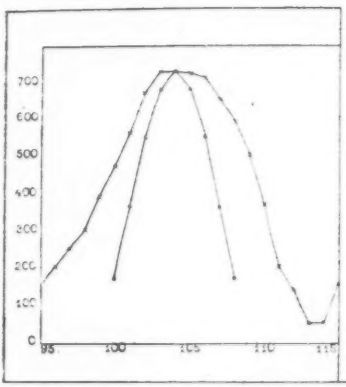


DIAGRAM II.

Diagram III.

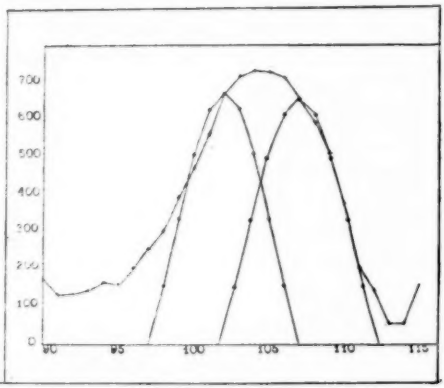


DIAGRAM III.

Diagram II.—This diagram illustrates the method of determining graphically whether the elevation on the London graph of amplitudes is due to a single or a compound period. The actual observations are indicated by a continuous line with crosses, the theoretical by a line with solid circles. The residual area existing after the area mapped out by the range of amplitudes that might be expected with the maximum ordinate at 104 weeks is very obvious.

Diagram III.—This diagram shows the graph of amplitudes in the vicinity of 104 weeks constructed from the data for London. It is indicated by the continuous line with crosses on it. On this graph are superposed graphs delimiting the range of amplitudes due to theoretical periods at 102 and 107 weeks, and the respective areas of the whole elevation due to these are indicated by continuous lines with solid circles thereon.

graphs of the amplitudes is produced by a single period or is due to the presence of two or more periods of nearly identical length. It is comparatively easy [2 (3)], to superpose on the graph the theoretical values calculated for the amplitudes adjacent to the principal amplitude, taking account of the magnitude of the latter and of the number of rows in the statistics. A comparison of the curve thus obtained with that already constructed for the amplitudes derived from

the original data is shown in Diagram II. It is apparent from a comparison of the data with the formula that a period of 104 weeks alone cannot account for the phenomena observed. The apparently uniform course of the graph of amplitudes is not accounted for by a single period, but may be due to the presence of two or more. With such statistics it is impossible to make any complete analysis. Certain assumptions may, however, be made and their results compared with the facts. The first assumption made was that one period is in the neighbourhood of 102 weeks. Performing the necessary calculations by the method described by Mr. Trachtenberg in a paper to be published soon, it is found that if a period exist at 102 weeks the residue of the elevation in the graph is accounted for almost in its entirety by a second period of 107 weeks. Similar calculations were made for periods of 101 and 106 weeks but the fit is not so good. It may be assumed with a certain degree of probability that the two periods of 102 and 107 weeks respectively, or periods very similar to these, may account for the features of the London graph (Diagram III). In the earlier period, however, that referring to the years 1840-69, the statistics seem to indicate that the principal periods then existent were of lengths of 101 and 106 weeks respectively.

To test the matter further a graph has been constructed for London showing the deaths that occurred in this city in lunar months for the epoch 1870-1910 (*see* Diagram IV). Under the base line of this graph are inserted definite markings, solid circles and crosses, to denote the points at which the height of the epidemics should occur in accordance with periodicities of 102 and 107 weeks respectively. A brief study of the diagram is sufficient to demonstrate that from 1876 to 1891 marked coincidence between the maxima of the epidemics shown in the graph and the periodogram periods exists. In this section of the graph the points of occurrence of the 102 and 107 weeks' periods are practically coincident and harmonize with the maxima of the epidemics graphed from the original data. In the last section of the graph the periods seem to have little if any marked relation to the incidence of the epidemics. From 1891 onwards the points of occurrence of the theoretical maxima of the periods at 102 and 107 weeks which have progressively diverged from each other coincide with the troughs rather than the elevations of the graph. It is to be noted that this divergence of the assumed maxima from the real is associated with a great lessening in the number of the deaths. It must be remembered that all this is guess-work, as the data are not sufficient to make any proper determination.

Diagram IV.

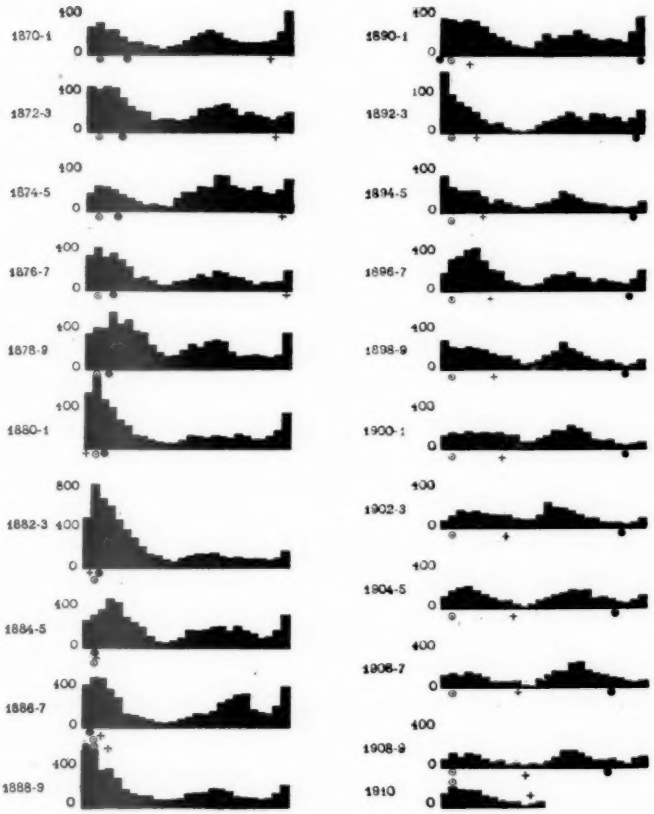


DIAGRAM IV.

In this diagram are shown the deaths from whooping-cough in lunar months in London for the epoch 1870-1910. Each section represents two years. In this town the existence of two periods, viz., 102 and 107 weeks in the vicinity of two years has been tentatively suggested. The points where such epidemics are theoretically expected are indicated by solid circles and crosses respectively. The closer correspondence of the theoretical epidemics with the actual data in the middle of the epoch than in the remainder is well illustrated. The points where elevations due to an epidemic of 104 weeks might be expected are shown by the following sign ⊙.

The phenomena for measles are quite different from those found in whooping-cough. In the former an epidemic corresponding to a period determined by the periodogram may be absent or of little moment for a time and again turn up in the expected site. Sometimes this appears due to the existence of circumstances, seasonal or other, which are unfavourable to the development of an epidemic. In whooping-cough, however, the data do not seem sufficiently homogeneous to permit of this phenomenon being observed. The explanation may be that the predominating periodicity has been changing at intervals. A change from 101 and 106 weeks' to 102 and 107 weeks' period seems to be indicated though it cannot be held in the least proved. It seems even possible that a regular periodicity may be entirely in abeyance for a section of the epoch under consideration, although the analysis of the statistics as a whole shows a well marked periodicity. Under the base line of the London graph marks (⊙) have been placed indicating the points where a 104 weeks' period should occur. These points seem to coincide more closely with the maxima of the graph than do the 102 and 107 weeks' periods already inserted, as of course would be expected from the presence of the seasonal variation, but this does not explain the whole phenomenon. It can only be said definitely that periods constant or inconstant in the neighbourhood of 104 weeks are the determining causes of the phenomena observed in the London statistics.

The statistics of London have also been systematically and carefully investigated for any longer periods between 160 weeks and 260 weeks (five years). This was done by writing out the monthly figures consecutively in periods at intervals of one month from 40 to 65 months and performing the necessary Fourier's analysis in each case. No period of any real significance, however, was found to be present.

The data for the other large towns already mentioned were investigated in a similar manner to those of London in the search for confirmation or refutation of what was found there and the inferences that have been drawn from these findings.

When the graph of amplitudes constructed for Bristol is examined, it is seen to differ in a very important feature from that of London in that the period in the vicinity of 104 weeks is completely absent. Two principal periods are evident, one at 95 weeks with an amplitude of 455, and a second at 124 weeks with a value of 530. In the section of the graph from 40 to 80 weeks, plotted from the second harmonic, there is a very large amplitude at 52 weeks which has a value of 545. As

there is practically a negligible amplitude at 104 weeks this 52 weeks' amplitude must be wholly expressive of the seasonal periodicity of whooping-cough in Bristol. Besides the two main periods which occur at 95 and 124 weeks respectively, the graph shows several elevations with a greater amplitude than 200. These are located at 89 weeks, 108 weeks and 138 weeks respectively, the amplitude of the 89 weeks' period being as high as 279, but they are quite overshadowed and appear relatively insignificant in comparison with the two main periods already mentioned.

The graph for Newcastle shows the principal period at 104 weeks exactly. This amplitude has a value of 338·6 and is the outstanding feature of the graph. There is another period at 124 weeks where the amplitude is 305. At 96 weeks, 140 weeks, and 144 weeks there are present three elevations on the curve with amplitudes of 190, 250 and 220 respectively.

The predominant period in the Liverpool graph is that at 104 weeks with a value of 282. Less well-marked elevations on the curve attain their maxima at 86, 96 and 111 weeks, where the values of the amplitudes are 220, 180 and 255 respectively.

In the graph of amplitudes for Birmingham the main period is shown at 99½ weeks approximately with a value of 337, which is considerably greater than any other amplitude visible in the graph. There is a well-defined subsidiary elevation at 86 weeks with an amplitude of 205. Near the middle of the graph there is a plateau surmounted by several elevations which show amplitudes varying from 185 to 190. The amplitude for 104 weeks is almost the lowest point on the curve, exhibiting a value of 75 only.

When the graph constructed for Salford is examined it is seen that there is one very evident period, the most prominent characteristic of the curve, at 105 weeks approximately with a value of 465. That this period is not exactly two years but slightly in excess of that is well illustrated in Diagram VI (p. 222). At 84 weeks there is a well-defined subsidiary elevation with an amplitude of 185, and at 122 weeks another with an amplitude of 190. At no other point on the curve does the amplitude rise above a level of 160. The longest predominant period of all those considered is found in the Dublin graph, which reaches its highest level at 135 weeks, where the amplitude is 473. There is another well-marked elevation, reaching its maximum at 124 weeks, with a value of 430. Less notable elevations on the graph have their maxima at 90 weeks, 98 weeks, 110 weeks, and 156 weeks, with values

of 285, 205, 195 and 280 respectively. At 104 weeks the graph shows a value of 50 only.

The principal period in the graph of amplitudes for Glasgow is one of 116 weeks approximately where the value is 305. Amplitudes of 265 and 262 are shown at 106 and 108 weeks respectively while at 99 weeks there is an amplitude of 187.

The graph for Edinburgh attains its highest level at 119 weeks where the value is 278. The amplitude at 156 weeks is 165. There is a subsidiary elevation with its maximum at 89 weeks, the value at this point being 200. At 99½, 103 and 110 weeks three elevations with values of 220, 235 and 215 respectively are seen surmounting a plateau.

TABLE III.

Amplitudes: 104 weeks.

Town	First harmonic	Second harmonic	Third harmonic	Fourth harmonic
London	365·3	484·8	112·8	84·6
Liverpool	282·1	303·2	44·8	81·0
Manchester	143·3	459·7	85·0	62·5
Salford	418·3	468·0	47·0	83·2
Bristol	11·1	548·2	49·4	103·0
Sheffield	59·6	339·7	55·8	12·7
Birmingham	79·1	400·1	73·0	70·4
Newcastle	338·6	312·4	68·8	55·9
Glasgow	181·2	396·4	64·9	25·2
Edinburgh	226·2	462·8	62·7	73·5
Dublin	54·9	340·6	57·4	118·2

Amplitudes: 156 weeks.

London	145·4	76·8	473·9	67·5
Liverpool	98·9	32·7	283·7	27·0
Manchester	97·5	170·8	439·5	38·5
Salford	102·0	227·7	459·5	37·8
Bristol	99·0	158·6	562·2	40·7
Sheffield	49·1	35·9	325·2	37·3
Birmingham	165·6	13·2	391·6	25·4
Newcastle	139·3	155·4	289·3	24·3
Glasgow	171·4	115·3	370·5	25·4
Edinburgh	268·1	62·9	470·0	38·3
Dublin	280·7	239·0	334·6	85·3

The Sheffield graph exhibits its principal maximum at 125 weeks where the amplitude is 265. At 101 and 108 weeks there are elevations with values in each case of 255. At 90 weeks the graph shows an elevation with a value of 185 while at 110 and 114 weeks the amplitudes are 245 and 220 respectively. The value of the amplitude at 104 weeks is only 60.

The graph for the City of Manchester, unlike those for most of the other towns, shows no outstanding amplitude. The highest level attained is 210 at 124 weeks. On the curve several elevations are shown varying in value from 150 to 160 but there is no definite predominant periodicity apparent.

It has already been shown for London that no period of more than two years existed. The longest period demonstrated has been that of 135 weeks in Dublin. It remains to consider how far long periods are absent from the other towns. Dr. Laing [3] came to the conclusion that epidemics of whooping-cough occurred in Glasgow and Edinburgh every three or four years and in Aberdeen every two years. While the latter is correct the investigation of the Glasgow and Edinburgh statistics shows no special tendency to a three years' period. The amplitude in Glasgow is small, only 170; in Edinburgh larger, 265, and possibly significant. The most marked example of a three-yearly period is that of Dublin where the amplitude is 280. In the first two cases, however, the curve in the periodogram shows a plateau rather than the type of elevation which corresponds to a period and suggests the presence of periods in the neighbourhood of three years which are irregularly present. The elevation in the Dublin graph which attains its maximum at 156 weeks when tested in the manner already described seems, however, to be wholly due to a simple period. An examination of the original data does not show any evidence that these relatively high amplitudes at long periods in Edinburgh and Dublin are due to the accidental coincidence of the totals of large epidemics in writing out the figures.

The apparent periodicities shown on the graphs of the provincial towns have been subjected to the two tests already described for London. In some of the graphs the predominant amplitudes are so outstanding in comparison with the others and with the section of the graph where no periodicity is probable, and are so evidently indicative of a real periodicity that the application of the test is unnecessary, but in others, e.g., Edinburgh and Sheffield, the principal amplitudes seem to fall within a range which indicates that they may be due to accidental variations in the figures. The second test to determine the range of amplitudes due to a period and to differentiate simple from compound periods when applied to the predominant elevations on the graphs, shows that the 104 weeks' period in Liverpool and Newcastle, the 94½ weeks period in Bristol, and the 123 and 135 weeks periods in Dublin are undoubtedly simple. The elevation in the Salford graph with its

maximum at approximately 105 weeks, that of Birmingham at 99½ weeks and that of Bristol at 125 weeks show appreciable residues after the areas due to the apparently principal periods have been mapped out. This may indicate that these periods are not simple, but it is even more impossible to analyse them with any certainty into their component periods than in the case of the London graph.

An examination of the graphs for the various towns under investigation shows that in most cases (the exceptions being Manchester, Edinburgh and Sheffield) conclusive evidence is furnished of definite periodicities in the epidemics of whooping-cough. The average intervals between the epidemics vary considerably in length. That most frequently found is approximately 104 weeks. This period is exactly 104 weeks in Newcastle and Liverpool, in the neighbourhood of 104 weeks in London, and almost exactly 105 weeks in Salford. The latter value is apparently not due to chance, as during the last forty years the progress of the height of the epidemic into the spring has been regular throughout.

Of periods differing from 104 weeks in excess or defect there are numerous examples, some with amplitudes which make their existence particularly probable, others with smaller amplitudes, which when tested, in the usual manner, are found not to have a probability of separate existence greater than 1,000 to one. There is, however, no necessity to reject all these. If in different towns very nearly equal periods are found to exist, and if in some of these towns the periods come into the range of high probability, then it is also probable that the periods, though not specially important, may exist elsewhere. Thus an undoubted period of 99½ weeks is found in Birmingham and one of 94½ weeks in Bristol. The amplitude at 99 weeks in Glasgow, though small, is verified from Birmingham. It may be remembered that the periodicities of measles in these two cities are almost identical. The period of 90 weeks in Dublin is, however, neither of sufficient relative magnitude to indicate high probability nor is it found elsewhere. In some cases the average period is greater than 104 weeks. The best examples of this are one of 111 weeks in Liverpool, two of 107 and of 116 weeks respectively in Glasgow, one of 124 weeks in Newcastle, one of 125 weeks in Bristol, and two in Dublin, one of 123 and another of 135 weeks. There is evidently a period of extensive geographical distribution in the neighbourhood of 124 weeks. With regard now to the exceptions: in the Sheffield graph amplitudes at 101, 108 and 125 weeks are prominent but none of these is of great magnitude. It seems

more than a coincidence that the graph should attain a high level at 108 weeks and its highest level at 125 weeks considering that the former period occurs to some extent in Glasgow and the latter is very

Diagram V.

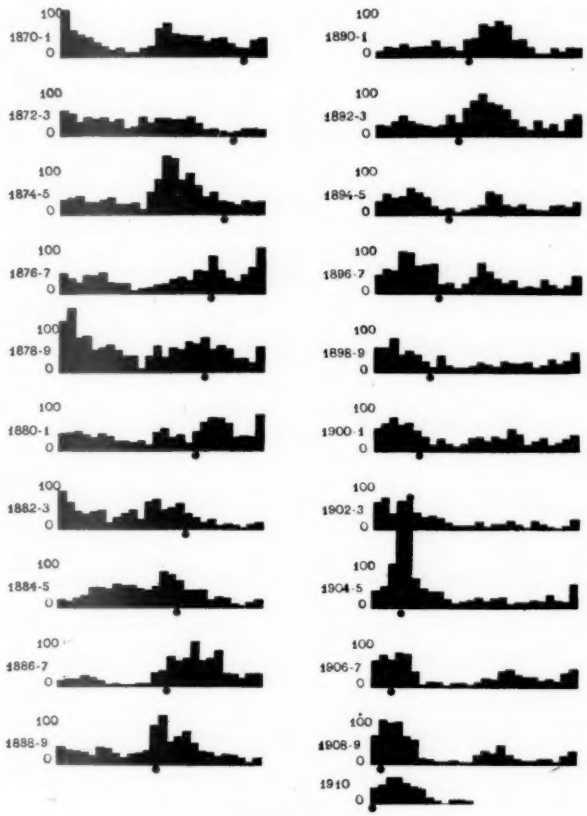


DIAGRAM V.

In this diagram the deaths from whooping-cough in lunar months in Birmingham from 1870 to 1910 are shown. Each section corresponds to two years. In this town only one predominant period has been demonstrated by the periodogram, and the points where the epidemics are expected are indicated by black circles under the base line.

well marked in the graphs of Newcastle, Bristol and Dublin. This same remark applies to Manchester. This city, as has already been found in the case of measles, shows no definite periodicities but the graph attains

its highest level at 124 weeks. For diagrammatic illustration graphs have been constructed in which the deaths occurring in each lunar month in the individual towns for the epoch 1870-1910 are shown and

Diagram VI.

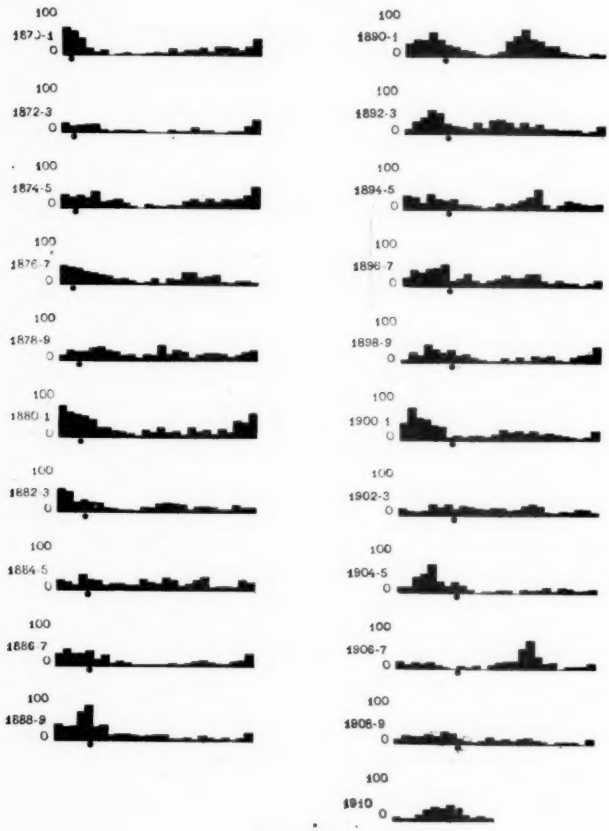


DIAGRAM VI.

This diagram shows the deaths from whooping-cough in lunar months in Salford from 1870 to 1910. Each section of the graph represents two years. A predominant period of approximately 105 weeks has been shown to occur in this town, and the points where the maxima of these epidemics are expected are indicated by solid black circles under the base line.

the position of the theoretical maxima indicated by placing solid circles and crosses under the base lines. A selection has been made from these for publication. Diagram V shows the graph constructed for

Birmingham. In this city only one period is found. It is to be noted how closely the average period determined to exist by the periodogram describes the position of the epidemics as found from the figures.

Diagram VII.

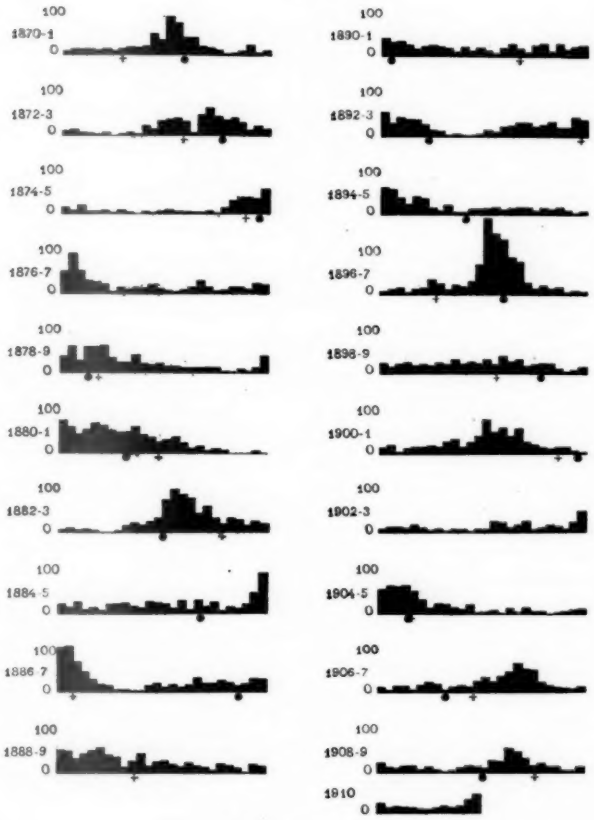


DIAGRAM VII.

This diagram shows the deaths from whooping-cough in lunar months in Dublin from 1870 to 1910. Each section of the graph represents two years. Two predominant periodicities, viz., of 123 and 135 weeks, have been demonstrated in this town by the periodogram analysis, and the points where the maxima of these epidemics are theoretically expected are shown by solid circles and crosses.

The next graph, that for Salford (Diagram VI) is specially interesting. The chief period here is 105 weeks and the regular manner in which the epidemics are shown to recur is really a fact of importance. In the

cases of Dublin and Bristol multiple periods have been demonstrated and the correspondence of the epidemics with their different periods are well illustrated (Diagrams VII and VIII). It will be noted that

Diagram VIII.

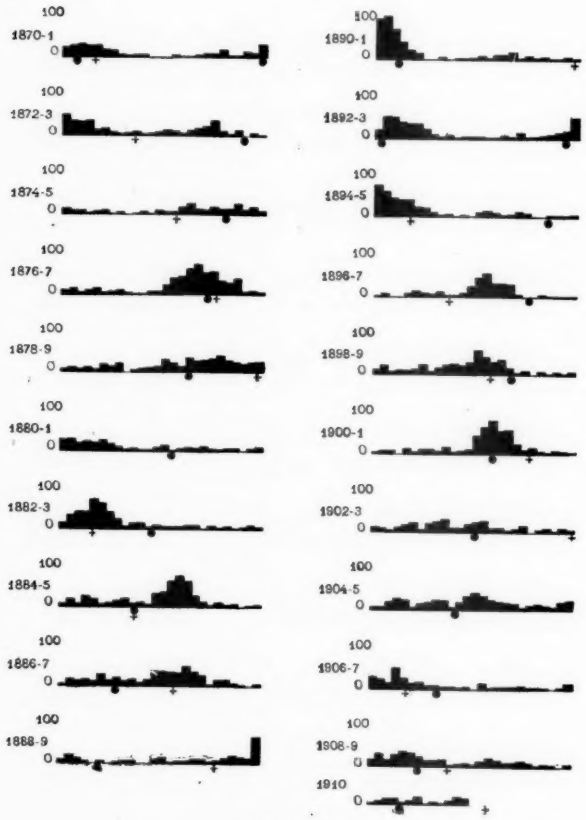


DIAGRAM VIII.

This diagram shows the deaths from whooping-cough in Bristol in lunar months from 1870 to 1910. Each section of the graph represents two years. As in Dublin, two principal periods have been demonstrated by the periodogram. These occur at 94½ and 123 weeks respectively, and their expected points of occurrence are indicated by solid circles and crosses under the base line.

occasional epidemics of some size occur in the graphs which are not included in the system or systems found by the method; on the other hand that the points at which epidemics are expected are not invariably

represented by epidemic elevations on the graph. The latter feature is probably to be explained in most cases, certainly in some, by the fact that some seasonal conditions are unfavourable to the development of

Diagram IX.

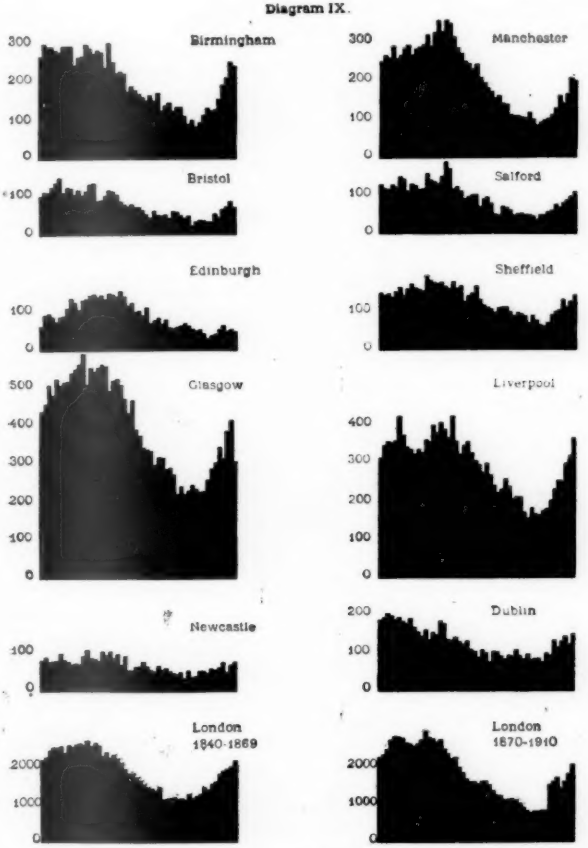


DIAGRAM IX.

This diagram consists of graphs for each of the eleven towns named, showing the total numbers of deaths from whooping-cough that occurred in each week of the year for the period of forty-one years from 1870 to 1910. The graph for London is represented in two sections—one section representing the epoch from 1840 to 1869, for which figures are available; the second representing the epoch 1870-1910 as in the other towns.

an epidemic. Thus though due in reality as apart from theory it may be found either not to materialize or to be much diminished in extent.

To investigate the seasonal variation of the prevalence of and

mortality from whooping-cough graphs have been constructed for each individual town showing the total number of deaths for each week for the period of 41 years from 1870-1910. In the case of London, as already described, the data have been graphed in two sections, the first for the epoch 1840-69, and the second for the epoch 1870-1910

Diagram X.

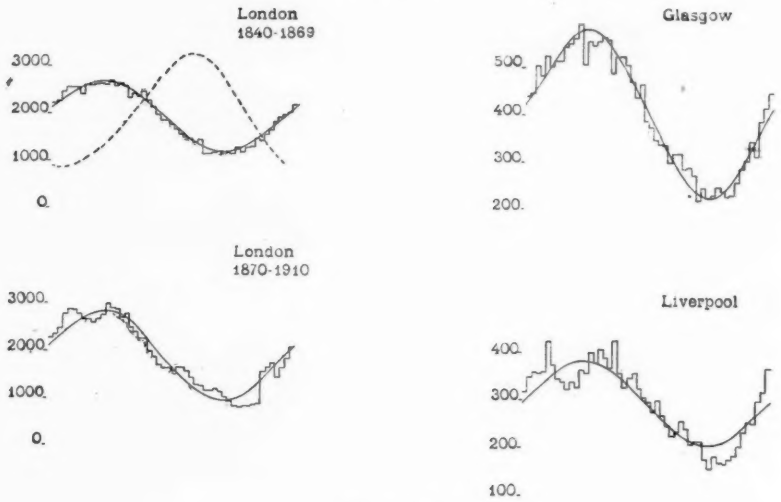


DIAGRAM X.

This diagram comprises four graphs showing the numbers of deaths from whooping-cough that occurred in each week of the year for both London epochs —1840-1869 and 1870-1910—and for the period of forty-one years, 1870-1910, in Glasgow and Liverpool. To each of these graphs a simple harmonic curve represented by a continuous line has been fitted, and the better fit shown by the first London epoch than in the second, Glasgow and Liverpool, is very obvious. There has been also superimposed, in the form of a discontinuous line on the graph for the first London epoch, the curve of the seasonal variation of the mean monthly temperature in London, and the relationship of this to the curve illustrating the mortality from whooping-cough is to be noted.

(Diagram IX, p. 226). The seasonal variation in the prevalence of the disease shows in all cases a maximum in the early months of the year and a minimum in the late autumn. A simple harmonic curve has been fitted to the data in each case. The amplitudes are all large. This curve gives a very close fit in most places. Two graphs, as already said, have been constructed for London for the epochs 1840-69 and 1870-1910

respectively, showing the total number of deaths which occurred in each individual week of the year during the periods and comparing the graphs thus obtained with the curve of the first harmonic. In the case of the first epoch the correspondence is almost absolute, in the case of the second somewhat less accurate. The harmonic curve fitted to the data for Liverpool shows a degree of correspondence with the latter which is in close agreement with that shown in the second London epoch. No such close correspondence to a simple harmonic curve is found either in the case of pneumonia or bronchitis. Further the maximum in these diseases corresponds better with the minimum temperature. With regard to whooping-cough the phenomena seem to be special. In the first epoch the maximum occurs on the fifty-seventh day of the year, in the second epoch on the sixty-fourth day. It may be said that the weather conditions of the winter produce a diminishing vitality lasting for a considerable number of weeks after the temperature has begun its upward course, while the higher vitality gathered in the summer withstands the fall of temperature for some time. This seems more likely than that the virulence or infectivity of the organism varies so as to produce this effect. The position of the maximum of fatality from whooping-cough varies, however, considerably from town to town. The following short table (Table IV) gives the day of the year at which the maximum number of deaths occurs :—

TABLE IV.

Town	Day of maximum fatality	Town	Day of maximum fatality
Liverpool	79	Bristol	59
Manchester	79	London (1840-69) ...	57
Salford	72	,, (1870-1910) ...	64
Sheffield	82	Glasgow	74
Birmingham	60	Edinburgh	78
Newcastle	74	Dublin	43

The day of maximum fatality is seen to be practically identical in Liverpool, Manchester and Sheffield, to be somewhat earlier in Salford and considerably earlier in Birmingham. Bristol and Birmingham correspond closely with London, while Edinburgh and Glasgow show maxima which are very similar and correspond with that found in Newcastle. Dublin is remarkable for the early date of the maximum fatality, which occurs forty-three days from the commencement of the year or little over half that observed in Liverpool, Manchester and Sheffield. The interpretation of the significance of this variation in

the day of maximum fatality presents some difficulty. It has been suggested already that the date of minimum temperature has perhaps some association with the date of the maximum prevalence and to investigate this point the following table (Table V) has been constructed.

TABLE V.

Town	Day of minimum temperature	Day of maximum fatality from whooping-cough
London	36·6	64
Glasgow	38·4	74
Liverpool	40·1	79
Birmingham	38·2	60
Sheffield	39·2	82
Newcastle	43·0	74
Edinburgh	40·5	78
Dublin	40·5	43

This table, however, is disappointing in affording no interpretation of the facts; the minimum temperature in Dublin occurring very little later than the minimum temperature in Sheffield. If then the factor is climatic it cannot be dependent on temperature alone. Dublin has essentially an insular as opposed to a continental climate such as may exist to some extent at Sheffield, and this might be taken as open to the interpretation that the continental temperature was more depressant to the vitality of the young than the insular temperature, but if this is the case the maximum in Edinburgh should be later than the maximum in Glasgow, whereas such difference as there is is scarcely appreciable. With reference to this conclusion it is worthy of note that Hirsch [4], from a study of 495 epidemics of whooping-cough, came to the conclusion that the season of the year has no material influence upon the epidemic occurrence of the malady, and further adds that "experience within the higher latitudes themselves in all that relates to the prevalence of the disease and the death-rate from it in the several seasons and under the sort of weather characteristic of them, serves to show that this ætiological factor is always of secondary importance in producing the malady and in determining its type."

In his investigation into the periodicity of measles Dr. Brownlee [2 (3)] found that in five large towns, viz., Bristol, Birmingham, Glasgow, Edinburgh and Sheffield, the periodicities found resembled those in London. In whooping-cough, on the other hand, the graphs of the above-named towns present no features in which they resemble that of London, unless it be that the period of approximately 107

weeks in Glasgow is represented by what has been tentatively determined to be a component of the London compound period.

It has also been found in measles that the principal periodicities, or those of any moment, shown in the towns under investigation are all more or less well represented in the London graph. This is not so in whooping-cough, as, with the exception of the elevation in the vicinity of 104 weeks, the graph of London maintains an almost uniformly low level and shows no periods corresponding to the longer and shorter periods which have been demonstrated in the other towns.

Although the graphs of amplitudes under consideration do not present such definite features as the corresponding graphs for measles for the same group of towns they undoubtedly show that there exists in some of the latter at least a more or less definite periodicity in the recurrence of epidemics of whooping-cough. In this disease also, as has been mentioned, there is a considerable variation in the epidemic periodicity in the different towns.

Similar periodicities in an epidemic disease such as whooping-cough occurring in different places may be assumed to indicate essentially similar causes taking effect in their production. The most reasonable explanation that has been offered to account for the periods of different duration that have been shown to occur in whooping-cough (which periods are, in some cases, so far as can be seen, independent of or without any obvious relation to the solar year or climatic variations, although in others they may be the result of, or modified by interference phenomena due to the interaction of varying weather conditions and the special organism causing the disease) seems to be that given to account for similar phenomena which have been found to occur in measles and small-pox. With regard to these it has been advanced that the length of intervals between the epidemics is in all probability associated with some vital property of the organism responsible for the disease; it may be that it acquires new potency in some way, and that the difference in the range of the periods is to be ascribed to the circumstance that there are different strains of the infective organism which exhibit differences in the duration of the cycle of their life-history. The evidence, such as it is, furnished in regard to whooping-cough in the foregoing pages, is not wholly in accord with the theory but it is probable from what is known of the life history of the whooping-cough organism, which is a bacillus, that it is very different from that of the probably protozoal form of the organisms of measles and small-pox.

Now that the principal periodicities of epidemics in both measles and whooping-cough have been determined for the same group of large towns it is possible to discuss the question of concurrent or sequent epidemics of measles and whooping-cough. Whooping-cough is usually said to occur frequently in association with measles, but it appears probable that this association has been exaggerated and there seems to be a difference of opinion as to the order of sequence with respect to one another in which these two diseases occur. It has been generally supposed that measles predisposes to, or in some way prepares the way for, whooping-cough. Hirsch [4] states that in 495 epidemics of whooping-cough a coincidence with an epidemic of measles was noted ninety-four times. In fifty-six of these the diseases occurred together, in eleven whooping-cough preceded measles and in twenty-five it came after. According to Dr. Laing [3] and Dr. Wilson [5] epidemics of measles and whooping-cough alternate in Aberdeen with considerable regularity, measles following whooping-cough rather than the latter the former. The following table (Table VI) gives in juxtaposition the principal periods found for measles and whooping-cough in six of the large towns that have been under investigation, viz., London, Birmingham, Bristol, Glasgow, Liverpool and Salford.

Table VI.

Town	Disease	Period						
		Weeks	Weeks	Weeks	Weeks	Weeks	Weeks	Weeks
London	Measles	87	89½	97	105½	109½	114	—
,,	Whooping-cough	—	—	—	102(?) 104(?)	107(?)	—	—
Birmingham	Measles	86	—	99	—	109	—	—
,,	Whooping-cough	86	—	99	—	—	—	—
Bristol	Measles	—	—	98½	—	—	116	—
,,	Whooping-cough	—	—	94½	—	—	—	125
Glasgow	Measles	—	89	98	—	109	—	—
,,	Whooping-cough	—	—	—	—	107	116	—
Liverpool	Measles	—	—	92	100	—	—	—
,,	Whooping-cough	86	—	97(?)	104	—	111(?)	—
Salford	Measles	—	—	90	104	—	114 or 115	—
,,	Whooping-cough	—	—	—	105	—	—	—

It will be seen from the table that in Birmingham and Salford approximately similar periods occur in the two diseases, but that in some of the other towns there is considerable divergence in the length of intervals between epidemics. It would thus appear, if the influence of the principal periodicity due to the organism alone be considered, that if in any one town of the latter group in any one year the epidemics of the two diseases are concurrent a certain number of years must elapse before concurrence again takes place, and in the interval the epidemic of whooping-cough will for a series of years alternately precede and succeed the epidemic of measles or vice versa. In a town such as Birmingham or Salford, on the other hand, if the epidemics are concurrent in any one year, they will exhibit a tendency to concurrence in succeeding years. The existence of multiple periodicities in one or both diseases in certain of the towns and the difference in the maximum seasonal prevalence of the two diseases—whooping-cough being most prevalent in the early months of the year and epidemics of measles showing a tendency to develop in the early winter and early summer—make the time relationship more complicated. The concurrence or otherwise of epidemics of measles and whooping-cough is, nevertheless, apparently dependent on the combined action of the periodicity due to the organism and that due to the season of the year, and from the varying periodicity found in different localities it is clear that the proportionate degree and order of sequence, or concurrence, may vary widely in these.

Conclusions.

From the foregoing account it seems possible to draw some conclusions, of which the principal are the following:—

(1) That, while the periodicities do not appear to be so notable or so well-defined in whooping-cough as in measles in the corresponding towns, there is conclusive evidence of a more or less regular periodicity in the majority of those considered, and that the cycles are too definite to be regarded as due to accident.

(2) That whooping-cough in London is a less complex infection than measles, in the latter of which there are many variants of the organism causing multiple periodicities.

(3) That the periodicities or cycles found seem characteristic of whooping-cough, and that, unlike what occurs in measles, well-defined

periodicities occur in whooping-cough in the other great towns which are totally unrepresented in London.

(4) That the variable periodicities found in different localities in whooping-cough are to a certain extent confirmatory evidence of the causal condition, being, as has been suggested in measles, connected primarily with variations in the quality of the contagium, or with different strains of the organism causing the disease having a different duration in their life-history, and it is probable that the differences found in the two diseases are to be explained by the presumed different nature of the causal organisms and consequent differences in their life-histories.

(5) That the concurrence or relative order of sequence of the epidemics of whooping-cough and measles is a function of the periodicities due to the organism and that due to seasonal influence, and that as the former vary in different localities so also will the proportion of cases vary in which the epidemics are coincident, and in which whooping-cough precedes measles, or vice versa.

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[3] Laing, Dr. Jas. "Whooping-cough in Aberdeen," *Public Health*, 1902, xiv, p, 584.

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APPENDIX.

Tabulation of Statistics.

Reference has been made in the foregoing paper to the use of a mechanical system for the purpose of recording and arranging the data for the various towns, and to reduce the arithmetical work required for their analysis. This was desirable to obviate the necessity of rewriting the figures denoting the numbers of weekly deaths for the periods of different length preparatory to making the required summations by the adding machine. It has been thought advisable to write a short account of the scheme adopted, as it may be of interest to those engaged upon somewhat similar work. In the method, cards of a type called the "tabulating card" are necessary, and electrically-propelled machines to deal with these. The tabulating cards are of oblong shape, $7\frac{3}{8}$ in. by $3\frac{1}{4}$ in., and have printed on them forty-five vertical columns of figures running from 0 at the head of the column to 9 at its foot. These columns may be divided by lines into "fields." The number of columns in a field depends upon the varying number of items to be recorded; thus a two-column field provides for the record of all numbers from 0 to 99, a three-column field for recording all numbers up to 999, and so on. In the present case all the information to be recorded on the card is in numerical form, but where this is not so the details require to be coded. The machines are of three kinds: (1) A punching machine for perforating the cards, (2) an automatic sorting and counting machine, and (3) an automatic tabulating machine. The punching machine is fitted with a keyboard of parallel keys, numbered in exact correspondence with the tabulating card. The cards pass automatically through the machine, and the keys are pulled into positions which depend upon the numbers required. When the keys are in the desired position, the depression of a lever punches holes in the card in the positions assigned to the numbers indicated at the lower margin of the keyboard, and the punched card is automatically thrown out into a receptacle. The depression of the lever causes the keys to revert to the neutral position, and a fresh card passes into the machine. Temporary pressure on a second lever retains the keys in their acquired position, so that cards may be punched in duplicate or triplicate if desired.

In the whooping-cough investigation four columns of the card were allotted to the record of the number of the week, as the period of forty-one years necessitated provision for the representation of 2,132 weeks. Subsequent "fields" were set apart for recording the weekly deaths from whooping-cough in the various towns, a "field" of three columns being reserved for the

London figures where the weekly returns were large and ran occasionally into three figures, and fields of two columns for each of the remaining towns. The tabulating cards were punched in the manner described, each card ultimately containing a record of the numerical order of a certain week as well as the number of the deaths from whooping-cough in that week in each of the eleven towns. Cards were punched for the whole 2,132 weeks of the forty-one years. In an ordinary investigation the cards which have had the requisite holes punched in the different fields are now passed on to the automatic sorting and counting machine. The punched holes in the card control the mechanism of the sorting machine in a manner roughly analogous to that by which the mechanism of a pianola is controlled by the punched holes in the music roll. Electrical connexion, established through the punched hole in the card by means of pins on a movable framework, moves a lever which automatically raises the lid of the compartment into which a card with the hole punched in that position should be deposited. This machine both sorts the cards into any groups required and counts them as they pass into the receptacles allotted to the different groups. The process is very rapid, the cards going through at the rate of 14,000 an hour. In the present investigation the sorting machine was not utilized, but the cards were sorted by hand into the required groups preparatory to their passage through the third machine—viz., the automatic tabulating machine. The cards having been retained in serial order, were first hand-sorted into groups of eighty, the first group containing the first, eighty-first, one hundred and sixty-first, two hundred and forty-first cards, &c., in the series; the second containing the second, eighty-second, one hundred and sixty-second, two hundred and forty-second, and so on. When the cards were arranged in this manner, a "space" card and a "total" card were placed in the intervals between the successive groups, and the whole series was passed through the tabulating machine. The tabulating machine is so devised that it can print the individual totals on the tabulating cards that pass through it as well as their sum or the sum of these totals alone. In this case the totals of the columns of the figures arranged in periods of eighty rows for all the eleven towns were first printed automatically on a roll of paper in parallel vertical columns. Time was saved by having a duplicate set of punched tabulating cards. While one set, which had been arranged in groups of a certain length, was passing through the machine the other set of cards was being hand-sorted preparatory to tabulation. Hand-sorting was adopted in the present instance because, after the cards had been suitably distributed in groups for any one period, all that was necessary to rearrange them preparatory to the tabulation of a subsequent period was, the cards having been retained in serial groups, to take one card from the beginning of the second group and place it at the end of the previous one, to take two from the beginning of the third group, to be added to the second group, three from the beginning of the fourth group to be added to the end of the third group, and to continue this procedure systematically throughout the series.

DISCUSSION.

Dr. BROWNLEE: There are a number of points of very considerable interest in Dr. Young's paper. The results are, perhaps, not so easy to interpret as in the case of measles, but they show on the whole the same class of phenomena. The high amplitude found for the period in the neighbourhood of two years in London is of special interest. In this case the phenomenon has persisted without much alteration during the whole seventy-two years. The form of the periodogram shows, however, that a two-year period alone will not account for the fact, but that there must be some mixture of periods of which the length is only approximately two years. Whether these periods are permanent or vary during the epoch in question does not seem capable of immediate solution. The long periods found in Dublin especially are of great interest. There does not seem, therefore, much doubt that these periods are real. Further, the fact that the amplitudes of these periods are about equal seems to preclude the explanation that the phenomena may be due to the recurring production of a susceptible population but that the cause must be found in the organism. Dr. Young's paper is an important contribution to the determination of the facts regarding infectious disease. It is only by this kind of laborious work that the actual phenomena requiring explanation can be ascertained. More interest should be taken in this side of the subject than is manifested at present.

Dr. MAJOR GREENWOOD: I agree with Dr. Brownlee in attaching importance to such researches as these. I also regret that they have not so far attracted many students. But I do not think this is surprising. It is not many years since much less recondite branches of mathematical statistics attracted even less attention amongst epidemiologists, and we must not expect that a knowledge of or interest in very difficult methods of analysis will be soon disseminated. Further, I should be loth to think that this method, or any other one method, is the only one by means of which epidemiological truth can be attained. The claim of the statistician to be admitted to the full privileges and franchises of the company of epidemiogists, once disputed, has now been conceded. No claim to a dictatorship will, I hope, ever be either made or recognized.

Dr. HAMER: The paper is very interesting, but difficult to understand on a first hearing. I am exceedingly grateful to the author, who has afforded me real consolation, for the general outcome of the inquiry in the case of whooping-cough is much less alarming than in that of measles recently presented to the Section by Dr. Brownlee. It is true that the conclusion is reached that in London whooping-cough exhibits a "compound curve," but

there is no suggestion that the components of the curve are associated, the one with the north side and the other with the south side of the river, as Dr. Brownlee holds is the case in measles. This last named suggestion, indeed, bewilders me, inasmuch as I work every day in sight of Waterloo Bridge, and am continually seeing and meeting children coming from South London, possible hosts of paramœcia of eighty-seven weeks' periodicity, and children from North London, possible hosts of paramœcia of ninety-seven weeks' periodicity. I confess that the more I think about it the less am I able to comprehend how two sets of children can possibly continue to live as it were cheek by jowl without one set influencing the other so far as measles is concerned.

Section of Epidemiology and State Medicine.

President—Dr. E. W. Goodall, O.B.E.

The Relationship of Small-pox and Alastrim.[1]

By S. Monckton Copeman, F.R.S., M.D., F.R.C.P.

"Variations in type which communicable diseases are liable from time to time to assume are eminently interesting from the point of view of the epidemiologist, but for the administrator such vagaries are not unlikely to give rise to awkward predicaments, resulting in some instances in unfounded charges and undeserved rebukes. The public are so accustomed to dogmatic statements that they are prone to attribute hesitation on the part of the medical man to ignorance rather than to accumulated wisdom."

This quotation from an annotation in the *Lancet* for August 1, 1903 (p. 326), referring to difficulties which had been experienced at Trinidad, in the West Indies, in determining the true nature of a disease in some respects resembling small-pox, was recalled to my memory, when, during the summer of 1919, I had occasion to investigate, on behalf of the Ministry of Health, an outbreak of varioloid disease of anomalous character which broke out in a limited area on the borders of Norfolk and Suffolk. In view especially of the difficulties experienced in arriving at a definite diagnosis and of the considerable divergency of opinion as to the true character of the disease among those—both the medical officers of health, and medical practitioners—concerned in the care and treatment of patients, some account of this particular outbreak may be of interest to the members of this Section; as also some comparison of the circumstances arising from this investigation when compared with those of an outbreak at Cambridge in 1903, also investigated by myself, and of yet another which prevailed so extensively

[1] At a meeting of the Section, held March 26, 1920.

in Trinidad at about the same period, and to which special attention was directed by the editor of the *Lancet* in the words quoted at the head of this paper.

Dealing first with the recent outbreak of disease extending over a period of rather more than a couple of months in the summer of 1919, confined for the most part to *Lowestoft and Beccles*, it is of interest to note that all the cases reported from these towns, as well as others which were notified at a later date, appear, as the result of investigation, to have had definite relationship with one another. They were distributed over the various localities on the borders of Norfolk and Suffolk as follows:—

Locality	County	Number of cases	
Lowestoft	Suffolk	8	31
Beccles	,,	17	
Bungay	,,	2	
Gillingham	Norfolk	2	
Geldeston	,,	2	

Origin and Course of Infection.

The outbreak originated with a man named Percy B., aged 32, the skipper of H.M.T. *Glenroy*, a steam-trawler which, during the war, had been engaged in mine-sweeping operations in the Eastern Mediterranean. The vessel, coming originally from Alexandria, stopped at Malta from March 5 to April 12, 1919, and at Gibraltar from April 16 to April 26; arriving at Weymouth, Portland, on May 6. Having obtained leave shortly after arrival he proceeded to Gillingham, near Beccles, where live his wife's parents, Mr. and Mrs. H., at Riverside Cottage. B. arrived at Gillingham on May 16, and, on the following day, May 17, he visited his brother, Jack B., at 144, Raglan Street, Lowestoft, where he stayed for one night, leaving again on the following day for Gillingham, whence he returned to Portland on May 25. I have since learnt from Dr. Lauder, medical officer of health for Southampton, where B. subsequently stayed at the Sailors' Home, that B. informed him that he felt ill for a few days at Portland before leaving for Gillingham, and that on the journey thither had noticed some spots on his face. On June 16 he proceeded, so he informed Dr. Lauder, on another ten days' leave, to Beccles, where he found his father-in-law affected in a similar manner to that which he had himself experienced. (He added that his washing had been sent to Beccles.) Dr. Lauder further stated in his letter that there were eight members of the crew aboard the ship,

none of whom complained of being sick, but that four of the crew had left at Portland.

From his wife, Mrs. B., who was living with her parents, additional information however was obtained to the effect that her husband had first felt ill on Sunday, May 11, 1919, and had not left his ship until May 15; that the rash came out in groups on his body and face, subsequent to which he felt quite well; and further that the mate of the vessel had also suffered in similar fashion. Mrs. B. did not herself contract the disease, in which connexion it may be mentioned that she had had chicken-pox at the age of about 10, that she had been successfully vaccinated as a baby and revaccinated in 1891 or 1892. But, as previously stated, Mrs. H., the mother of Mrs. B., and her husband both subsequently became infected, Mrs. H. first falling ill on June 5. On June 8 she was seen by Dr. Wood-Hill, of Beccles, who, in the absence of any definite symptoms, diagnosed gastritis. On the afternoon of the same day however a rash appeared, but as she was not seen again by a medical man until June 4, no satisfactory account of the appearance of the rash is obtainable. She was never at all seriously ill. She was apparently vaccinated in childhood, but never revaccinated. Twelve days later, her husband, Mr. H. fell ill, being attacked with vomiting which caused him to think that he was suffering from indigestion. He therefore went to bed for two or three days although not feeling seriously ill. But on June 23 spots began to appear, first on the nose and later on the forehead. On July 5, when I saw him, together with Dr. Wood-Hill and Dr. Fox, the scabs had all come off; and, on examination, we found that he had three good vaccination marks on the left arm. As Mr. H. is exceptionally deaf it was practically impossible to obtain any information from him, but Mrs. H., communicating with him by means of signs, stated that she understood from him that, so far as he was aware, he had never had chicken-pox.

Gillingham, although actually in Norfolk, is situated just on the opposite side of the River Waveney at Beccles, and the H.s' cottage is close to a bridge over which passes the main road from Beccles to Loddon and Norwich. This is important in view of the facts that will subsequently be described bearing on the question as to how infection subsequently spread from this family to Beccles, where no less than seventeen cases eventually came under observation.

Reverting to Percy B.'s visit to Raglan Street, Lowestoft, it is of interest to note that his brother, Jack B., aged 43, fell ill on

May 29, *exactly twelve days* from the date of arrival of Percy B. On the following day, May 30, he was seen in consultation with Dr. Boswell, of Lowestoft, by Dr. Stewart, C.M.O.H., and his assistant Dr. Forbes, to the latter of whom I am indebted for the statement as to his condition on that date: "Although in bed he was then feeling better and had a profuse papular rash over the abdomen, a few papules on the flanks and back and also a few on the forehead. There was however no sign of vesiculation or pustulation. The papules were red and surrounded by a red areola—in appearance very similar to the papules in an acne."

Dr. Stewart wrote to Dr. Boswell the following day to say that in his opinion the case was neither small-pox nor chicken-pox, but that it might be acne. But on seeing the patient again, on June 5, he revised his opinion to the extent of informing Dr. Boswell that the case looked much more like small-pox or possibly chicken-pox than it had before. Thus far all the cases known to have occurred were all of a very indefinite nature, but in those that subsequently occurred the symptoms were much more definite.

The next Lowestoft case was that of Mrs. F., aged 28, living at 146, Raglan Street, next door to Jack B. It may be mentioned that these two houses have a back yard in common, and that the occupiers of both of them are on very friendly terms, which probably accounts for the infection spreading from one household to the other. As regards the occupants of the house on the opposite side of the B.'s cottage, these people were, as I was informed, practically unknown to B. and his wife.

Mrs. F. was taken ill on June 19 or possibly the day previous. She states that she felt weak and shaky on waking up, and, on looking at herself in the glass noticed some spots on her chest. She is quite clear that the spots which subsequently appeared on her face did not come out until June 22, after which the eruption spread practically all over her body. She was seen in consultation with her medical attendant, Dr. Perry, by Dr. Stewart, C.M.O.H., on June 25. Dr. Stewart subsequently wrote to me saying that Mrs. F. had herself informed him that the rash developed in the first instance upon her chest, spreading to the abdomen, while at the time of his visit he found that it practically covered the whole of the face, arms and legs. On July 5 I saw Mrs. F. in the Lowestoft Small-pox Hospital, when, although her appearance indicated a serious condition, she was quite clear in her intellect, and stated that she felt fairly comfortable.

The whole surface of the body was covered with an eruption of apparently varying age, and confluent in many places. On the backs of her hands, knees, and elbows, and on either side of the chest, the skin was flaking off over areas of several square inches, leaving a raw surface, a condition which seemed likely to lead to septic trouble. Fortunately however this did not supervene, and, on my next visit to the hospital, about a fortnight later, I found her out of bed, and feeling, as she said, quite well, although her face was badly marked. A special point of interest in her case is to be found in the fact that although she was not admitted to Hospital until June 29, ten days after being taken ill, the nurse informed me that further spots had come out *after* her admission, some, newly developed, having been noticed as late as July 1. These, according to the nurse's description, had the form of "little white pimples resembling pearls."

Mrs. F. has three young children, a boy aged 5½, and twins aged 2, who, as soon as she was known to be ill, were removed from her home to the care of a relative living some distance away in Lowestoft. Unfortunately these children were not vaccinated immediately, with the result that the elder one contracted the infection. Happening to be in Lowestoft on the day after this child was taken ill, I had, in this instance, the opportunity of myself observing the rash in its earliest stage. On examining the child I found about a dozen tiny, more or less "shotty" red papules on the chest and abdomen but *none on the face or head.* But the photograph of this child, taken in the Lowestoft Small-pox Hospital a few days later, showed that the eruption, now profuse and definitely vesicular, had spread over the face and head, as well as over the entire surface of the extremities and trunk.

Four further cases of small-pox were subsequently notified in Lowestoft, the first three of which were directly connected with those previously mentioned. Joe A., aged 28, of 172, Raglan Street, in whom the rash developed on July 14, had helped to nurse Mrs. F., and finally to carry her to the ambulance when she was removed to hospital. This he did entirely out of good nature, as other neighbours were afraid to do anything for her. He is a recently demobilized soldier, who has been serving in Mesopotamia, and was in a more or less poor state of health at the time he became infected. He had been vaccinated in infancy and again in 1914 on joining the Army, and had been revaccinated with success about a week before his illness disclosed itself. Fred D. F., aged 15, also of 172, Raglan Street, who became ill on July 23, with rash on July 25, had been sleeping with the previous patient. F.

also had been vaccinated with success about a week before his illness commenced. The third patient, Sarah B., aged 54, lived with the A.'s, at 172, Raglan Street. She became ill about the end of July, the rash appearing on August 2. She had been vaccinated in infancy, and was offered revaccination with the other occupants of the house and on subsequent occasions, but persistently refused. The last Lowestoft case was that of a girl who had come on holiday from Walthamstow and had taken lodgings at a boarding-house in Lowestoft. On August 9 she was taken ill, a rash on the face and forehead appearing on the following day. As in her case some doubt was felt as to the correct diagnosis she was at once vaccinated, together with a friend of hers who had been sleeping in the same bed with her at Lowestoft. She was then removed to the Carlton Small-pox Hospital, but isolated in a room by herself. It subsequently appeared that a brother and sister of this patient had been ill at Walthamstow about fourteen days previously with what was there diagnosed as chicken-pox, and it is not improbable that it was chicken-pox rather than small-pox from which this patient was suffering. In view, however, of the apparently definite incubation period of fourteen days only, and of the similarity, in the early stages, of her symptoms with those of the other Lowestoft cases, it is obvious that the administrative action taken in her case was quite justifiable. It may be mentioned that so soon as it became known to the other occupiers of the lodging-house that this patient had been notified as a case of small-pox, they all, to the number of twenty-eight, at once left Lowestoft. Dr. Marshall, the M.O.H., however, was able to obtain from the boarding-house proprietor the name and home address of all these people, with one exception, and he at once notified the medical officers of health of the districts concerned in each instance.

Beccles Cases.

The cases of Mr. and Mrs. H. have to be regarded as Beccles cases seeing that their cottage is actually within the boundaries of the town, although—being on the west side of the river Waveney—it is included in the Gillingham Rural District (Norfolk).

In Beccles itself the first case of infection occurred in Stone Yard, a small enclosed court, containing seven two-roomed cottages, for the most part in so bad a condition that the medical officer of health is endeavouring to get this property demolished. The patient was a Mrs. H., aged 66, who fell ill on June 12, 1919, and developed a rash

on June 15. A couple of days later she was attended by Dr. Wood-Hill, who, being doubtful as to the diagnosis, asked Dr. Stewart, the C.M.O.H., to see her in consultation. On that date nothing was known as to the source of her infection, and, in the absence of any information on this point, Dr. Stewart came to the conclusion that she was suffering from chicken-pox. Some days later, on July 5, I saw her, together with Dr. Wood-Hill, his partner (Dr. Fox), and Dr. Marshall, Medical Officer of Health for Lowestoft, with whom I had motored over from the Small-pox Hospital at Carlton. We then discovered the interesting fact that a diminutive boy, aged 12½, but looking much younger, whom we saw in the yard, was a grandson of Mrs. H. and also a grandson of Mrs. H., the mother-in-law of Percy B., by whom infection was originally introduced into the district. Although the H.'s live on the other side of the river the actual distance of their cottage from Stone Yard is less than half a mile. Mrs. H. denied any intercourse with the H.'s, but, on inquiry, it was found that the grandson, who lives with Mrs. H., was accustomed to visit the H.'s cottage from time to time. Furthermore, we learnt that on some date, which cannot be accurately determined, but apparently before Mrs. H. took to her bed, she had given this boy three pennies, which he on returning to Stone Yard had handed over for safe-keeping to Mrs. H. When mentioned to her, Mrs. H. said she remembered the occurrence, and stated that, being busy at the time, she had kept the pennies in her hand for some time before depositing them in the money-box. The boy, it may be mentioned, had been successfully vaccinated in infancy; and Mrs. H. informed us that she herself had been vaccinated as a child of 8, but not since. She also told us that she had learnt from her mother that, at some time in her childhood, she had suffered from "water-pox."

As regards the possibility of Mrs. H. having contracted the infection in such indirect manner as suggested, it is perhaps noteworthy that McCombie, in his article on "Small-pox" (Clifford Allbutt's "System of Medicine"),[1] makes special mention of the fact that, in small-pox, contagion may be indirect, and that the disease may be communicated from the sick to the healthy by a healthy third person, or by various articles among which *coins* are specially mentioned.

The fact that the nature of the disease from which Mrs. H. was suffering was not definitely diagnosed for some days accounts, in part, for the fact that she was the starting-point of a number of

[1] Allbutt and Rolleston, "System of Medicine," vol. ii, part i, 1906, p. 489.

other cases in Stone Yard, where, out of a population of twenty-four individuals who inhabited the seven cottages, no less than ten eventually contracted the disease. These included her daughter Caroline H., aged 15, who lived in the same cottage with her; Mrs. Emma R., aged 55 (who, in neighbourly fashion, helped to nurse Mrs. H.); Mrs. H.'s son, aged 43; and six other persons. Either Mrs. H. or one of the other Stone Yard cases appear to have been responsible for transmitting the infection to other patients in New Road and at Mill Side, Beccles, respectively; while Mrs. H. was herself doubtless responsible for the illness of her married daughter, Elizabeth F., aged 35, and her husband, Percy Charles F., aged 30, who lived in a cottage distant about 200 yards from the entrance to Stone Yard.

Bungay and Geldeston Cases.

The four cases, of which two occurred at Bungay and two at Geldeston—a village in Norfolk, although only a few miles distant from Bungay and Beccles—also appear to have contracted infection in somewhat indirect but interesting fashion from the Beccles Stone Yard centre.

Alice Maude S., aged 27, an unmarried girl, living in a cottage at Geldeston with seven other relatives, and her aunt Mrs. Ann W., aged 48, of Flixton Road, Bungay, journeyed together by rail to Beccles, on June 30, to clean out an empty cottage in Stone Yard in which Miss S.—who was engaged and shortly to be married to a son of Mrs. H.—was about to live. On this date she visited Mrs. H. and, not improbably, her fiancé also, but there appears to be some question as to whether Mrs. W. also went into Mrs. H.'s house. However, both Alice S. and Mrs. W. subsequently contracted small-pox; Mrs. W. being taken ill on July 12, while Alice S. was attacked about thirty-six hours later. They each developed a rash three days after being taken ill and were both removed to the Small-pox Hospital, at Ipswich, which has been provided for the requirements of the county of Suffolk with the exception of the Borough of Lowestoft and the Urban District of Oulton; but by special arrangement the Geldeston cases were also admitted. Both in Bungay and Geldeston the primary case was followed by one other only. At Bungay, C. W. S., aged 20, a possible contact of Mrs. W., was subsequently notified; while at Geldeston, Robert S., aged 38, a brother of Alice S., fell ill somewhere about July 29, developing a rash on July 31, in which con-

dition he was found by Dr. Wood-Hill in the waiting room of his surgery at Beccles. This occurrence was doubtless the indirect cause of the last case known to have occurred in Beccles, that of Percy C. M., aged 11 months, an *unvaccinated* child, whose parents live at 120, Denmark Road, Beccles. For, as the result of subsequent inquiries, it appears that this child's father, while waiting to be vaccinated, had been sitting in Dr. Wood-Hill's surgery next to Robert S. for about a quarter of an hour on July 31. He states that on going home, in order to relieve his wife, he took the baby up in his arms and nursed it for some time as it was ill and fretful. On August 8 or 9 the child is stated to have become very quiet and to have slept for a great part of the day, although apparently feverish and perspiring a great deal. On August 9 a number of little white spots, like blisters, were noticed on the child's legs, and on August 10 similar spots also appeared on the face and neck. On August 12, the date on which I last visited Beccles, I learnt that the child had been removed to hospital that same evening, when it was found that he had a temperature of 103° F., with a pulse of 120, and respirations 52 to the minute. On visiting the hospital with Dr. Fox about 9 p.m. and examining the child we found several well-marked vesicles on the abdomen and neck, and also one on the sole of the right foot. The child was then more or less unconscious and evidently seriously ill; and, as a matter of fact, it died a couple of days later, on August 14, a termination not unexpected in view of the history as to its previous condition of health, and of the further fact that it had never been vaccinated.

As at quite an early stage of my inquiry, both at Lowestoft and Beccles, it became apparent that the cases of the disease under investigation were, in certain respects, not by any means typical of small-pox as usually met with in this country, it is desirable to give some account of the various phases of the disease as witnessed by myself and by the medical officers of health in the affected districts, as well as by the various medical practitioners in charge of the cases.

Incubation Period.—In certain instances it proved possible to determine, fairly accurately, the period that elapsed between exposure to infection and the onset of the first symptoms of the disease, when it was found to vary between twelve and fourteen days.

Initial Symptoms.—Occasionally the onset was not well marked, the patient possibly complaining of an indefinite feeling of being "out of sorts," while in other cases the illness commenced suddenly

with extreme prostration and severe pain, particularly in the head, together with nausea not infrequently resulting in actual vomiting. In a few cases these prodromal symptoms were entirely absent. Backache was by no means a common symptom, although it was occasionally met with. In some cases indefinite pains, stated to be confined to the muscles or bones, were complained of, in this respect somewhat resembling the onset of an attack of influenza in the epidemics of former years. Not infrequently the patient thought he was suffering from a severe bilious attack and consequently went to bed and perhaps dosed himself with castor oil, salts, or some other medicine of an aperient character. In one instance, in which, as previously stated, the patient was seen by a medical man a few hours before the rash appeared, he diagnosed the illness as gastritis. Rigors have not been observed, nor did the patients usually complain of feeling feverish, although at a somewhat later period when temperatures were taken as a matter of routine, it became apparent that the onset of the disease was apt to be accompanied by high temperature, amounting in some instances to 103° F. or 104° F. Several of the patients complained of pain or irritation in the back of the throat, accompanied by cough, or of soreness of the mouth. In two cases delirium was marked on the second or third day. Coincident with the first appearance of the eruption on the second or third day, the initial symptoms often disappeared so completely that the patients thought themselves quite well, got out of bed, and were even found to be following their usual vocations. In the majority of cases, indeed, no further illness was complained of throughout the whole course of the disease other than the irritation due to the presence of the eruption, the patients feeling and looking well and taking food quite in their usual manner. Of the whole series of cases only four were at any time seriously ill, and although it appeared probable in the case of one of these that the attack would prove fatal he presently recovered. The only instance in which the patient succumbed to the disease was that of a puny unvaccinated baby, aged 10 months.

Initial Rashes.—In a few cases the initial symptoms were accompanied or followed by a more or less generalized rash of an urticarial or measly character. One patient attacked with the former kind of rash had been vaccinated in infancy, and again at the age of 15, a year or two before her attack developed. In her case no further symptoms supervened, although a relative, living in the same house, passed through the disease in its usual form. When visiting Beccles on the first

occasion, I found the occupant of one of the cottages in Stone Yard and her daughter both in bed, suffering from what seemed to be prodromal symptoms of the disease, but the daughter, a girl aged about 12, showed a rash apparently typical of measles. A few days later, however, I heard from Dr. Wood-Hill that they had both developed a well-marked vesicular eruption. Two of the men, referred to above as having contracted the disease in specially serious form, both showed, at an early stage, a profuse rash of a reddish-purple colour on the lower part of the trunk and a more intense band on the upper and inner aspect of the thighs, about a couple of inches in width, bounded above by the line of the groin. One of the men complained of the intolerable itching of the skin in that region. With occasional exceptions, a papular eruption shortly passing into a vesicular stage, first made its appearance on the second or third day of the disease, although in some instances this continued to develop in definite crops at intervals of several days—an extreme instance of which was seen in the case of Mrs. F., who, although she was not admitted to hospital (at Lowestoft) until what was probably the tenth day of the disease, was stated by the nurse-in-charge to have developed a further crop of vesicles on the upper portion of her arms on the day following her admission. This patient, curiously enough, had no prodromal symptoms before the appearance of the rash.

As regards the *position* of the eruption, it is of special interest to note that as in the case of Mrs. F., the rash in the greater number of instances was noticed on the chest, trunk, or limbs, at different intervals of time, extending from some hours to several days, before it appeared upon the face. In two instances, at least, vesicles developed inside the mouth or on the mucous membrane of the cheek. The eruption, in most instances, gradually spread over the entire surface of the body, although it was never so marked on the upper arms as on the forearms, nor on the flexor as on the extensor surfaces of the limbs. Actual confluence of the eruption on the face was not uncommon, only occurring, however, on the trunk and limbs in a few instances—a somewhat remarkable fact in view of the density of the eruption. The eruption also occurred on the palms of the hands and the soles of the feet, these, in certain instances, being almost covered with "seeds" as in the case from which one of the photographs was taken. In Mrs. F.'s case, as previously stated, at an early stage of the eruption, the skin, especially in the flexures of the elbows and knees, on the bend of the shoulders, and on either side of

the upper part of the chest, became raised in areas of several inches in extent and eventually became rubbed off leaving exposed the moist surface of the underlying dermis. This condition existed on the day on which I first saw her in hospital, but, extraordinary to relate, when I saw her again five days later, these areas, which of course had been carefully dressed by the nurse, had practically healed up without causing any rise in temperature or other unpleasant symptoms.

The individual vesicles did not conform as regards their appearance and structure, to the description of those usually met with in ordinary small-pox. They were more or less dome-shaped, not unbilicated, or septate, while owing to the thickness of the raised layer of epithelium they usually did not collapse when pricked, as is the case with the vesicles of chicken-pox. In colour they were, at first, a dull white, gradually changing to a more or less yellowish tinge, and passing into a reddish-purple as they became pustular. In some instances, however, they had more or less dried up by this stage, and no purulent fluid could be detected in them. They gradually dried up completely, and finally became detached in about three weeks. Several of the patients, subsequent to the falling off of the "crusts," were left with considerable superficial pitting of the skin, particularly of the face, but it is too early as yet to say whether this is likely to be of a permanent character.

Diagnosis.—From what has already been stated in reference to the symptoms, it will be obvious that the average intensity of the disease, after the first few days, at any rate, was less than is usually met with in even a comparatively mild outbreak of small-pox. Other outstanding points of difference are the nature of the eruption, more especially in regard to (*a*) the site of its first appearance; (*b*) the fact that it comes out in definite and successive crops—in both these respects resembling chicken-pox rather than small-pox. In point of fact, several of the first cases, as previously mentioned, were originally diagnosed as chicken-pox, mainly for these reasons, coupled with the point that the patients were obviously not seriously ill. Furthermore, it was the exception to meet with any secondary rise in temperature about the time at which the vesicle was passing into the pustular stage, although I am informed that, in one or two of the latter cases in which the temperatures were carefully taken in hospital, some rise of temperature was noted at this stage. In all these respects, the disease under investigation, apparently introduced in the first instance from some locality in the Mediterranean area, although undoubtedly of the nature of small-pox, would seem more closely to resemble that variety that has been so thoroughly studied in Brazil,

and which is known as alastrim, rather than the form of the disease usually met with in this country. An excellent account of the considerable amount of literature dealing with alastrim is to be found in Kolle and Wassermann's "Handbuch" (*see* list of references) in which a special article by Tomarken and Carrière is devoted to the subject.[1] Reference to this account will show that the description of this variety of the disease conforms, in practically every detail, to what has been observed in the course of the disease recently under investigation at Lowestoft and other places. The photographs shown[2] indicate certain points of difference between the cases from which they were taken and ordinary small-pox, although by the time that the eruption had become fully developed, at which period most of the photographs were taken, the appearances more nearly resemble those of ordinary small-pox than was the case in the earlier stages of the disease. In point of fact the photographs of Mrs. H. (taken by Dr. Fox) and of the boy F., taken some days after his admission to the Lowestoft Small-pox Hospital, would probably be regarded by anyone acquainted with the peculiar distribution of the eruption of small-pox as being completely typical of that disease. One further point to which attention has not previously been directed, but which requires mention, bears on the effect of vaccination in this variant of small-pox. This has reference to the fact that although vaccination, especially if reinforced with recent re-vaccination, confers protection against the disease, its protective power does not appear to be of so definite a character as in ordinary small-pox, while, further, in some instances, it has been found possible to vaccinate the patient with success *during the period of convalescence.*

Considering the number of children in the various localities affected who have escaped vaccination in past years, it is interesting to note that the majority of cases of the disease occurred among young adults, or as in a few instances, persons of advanced age; of children below the age of 10, one only contracted the infection.

Mortality.—It is noteworthy that the mortality from this disease was extremely low, the only case in which death occurred being that of an infant previously mentioned, which, just before its attack had been discharged from Norfolk and Norwich Hospital, where, for several weeks previously, it had been under treatment for intractable eczema and rickets. This special point, among others, characteristic of the disease,

[1] Kolle u. Wassermann, "Handbuch der pathogenen Mikro-organismen," 2te Aufl. 1913, Bd. VIII, pp. 708-709 (by E. Tomarken and H. Carrière).

[2] Not reproduced.

must apparently be regarded as having relation to its probable locality of origin. As matter of fact, on the occasion of my first visits to the affected districts, I expressed to the various medical men and other officials concerned, the opinion that, as the disease gradually became more acclimatized, it was not improbable that it would tend to approach more nearly to the type of small-pox as ordinarily met with in this country. Such indeed would appear to have been the case, since among the later cases notified there have been several in which the severity of the symptoms has been distinctly greater than in the earlier ones, while the general course of the disease in other respects has been less abnormal than was observed in the first instance. Thus, in writing, on July 17, as to the condition of certain of the later cases in the Beccles Small-pox Hospital, Dr. Fox stated that "the latter cases were more typical of ordinary small-pox as they not only suffered from headache but from pronounced backache," adding that one of the men was so extremely ill that he would probably die. So certain apparently were both Dr. Fox and the staff nurse at the hospital that this case would terminate fatally, that a coffin was actually procured and taken to the hospital in order that, in the event of the man dying, there should be no delay in burial. Extraordinary to relate, however, when I again visited the hospital a few days subsequently, I found this man sitting up in bed and feeling, as he stated, "quite comfortable except for the irritation of his hands and feet" resulting from the innumerable "seeds" which he was endeavouring to pick out with a pen-knife. Practically all the other patients began to feel quite well within a few hours after the eruption had come out, notwithstanding the comparative severity of the prodromal symptoms in some cases.

Nature of the Disease.—From the account of this disease given in the preceding pages it will be realized that, although undoubtedly a variant of small-pox, its characteristics differ in many respects from the usual type of that disease. As previously mentioned, the disease under investigation appears to bear close relationship with the form of small-pox termed *alastrim*, numerous outbreaks of which have been reported in various parts of South America. The resemblance of alastrim to the disease which is the subject of the present paper, and the respects in which it differs from normal small-pox, may perhaps best be shown by a résumé of the description of alastrim to be found in Kolle and Wassermann's handbook to which reference has previously been made. The authors of the article in question state that among the initial symptoms most usually noted are: headache; sacral pain, but not so

acute as in variola; gastric distress; bronchitis; often high initial temperature; no exanthem in femoral triangle; fever drops on appearance of rash; patient feels persistently well afterwards. As regards the appearance and locality of the rash, the exanthem appears first as small spots, and in regular sequence (*successive crops*); usually first on forehead, then on the abdomen, back, breast and extremities.

The vesicles, which are usually dome-shaped and which contain a scanty amount of watery fluid, are of a dull whitish colour looking as if the skin were "bespattered with spots of chalk." The vesicles are *not* septate, i.e., they are unilocular. There is no umbilication. The stage of pustulation is unaccompanied by increase of temperature. "Drying up" commences about the ninth day; complete by the fourteenth or fifteenth day. Scars disappear in a few weeks. Mortality low, usually from 0·5 to 1 per cent. The disease is stated to be *very infectious*, but not communicable to animals. Recent vaccination is said to afford protection but *not* vice versa.

As was however perhaps to be expected, the cases under investigation have not all entirely conformed to this classical description of alastrim, although instances have occurred illustrating every one of the points in which this disease may be regarded as exhibiting deviation from a typical case of small-pox, even when modified by previous vaccination. Such deviation in cases of the disease under investigation are to be found in the fact that the eruption not only developed as a rule on *the chest and abdomen* before appearing on the face, but also came out in *successive crops*, extending in some instances over a considerable number of days; severe lumbar pain was absent; while the gastric distress specially referred to in the article on alastrim in Kolle and Wassermann's handbook, was a noteworthy symptom, not infrequently resulting in actual vomiting. In some cases a high initial temperature, up to 104°F., has been recorded; but in the majority of cases, directly the eruption appeared the temperature not only fell to normal but the patient felt practically well. In only one or two of the later cases was there any evidence of a secondary fever; and with the exception of two of the more recent Beccles cases there has been entire absence of the peculiar intense "mousy" smell so typical of ordinary small-pox.

The appearance of the vesicles, as described for alastrim, and the rapid further evolution of the eruption in this disease are also distinctive points. Moreover, in the case of alastrim, vaccination, unless recent, does not appear to afford as efficient protection as in the case of small-

pox; while it has proved possible to revaccinate several of the patients during the period of convalescence.

With the object of obtaining further evidence as to the true nature of the disease it was decided to obtain material from vesicles at an appropriate stage, for the purpose of animal inoculation on monkeys and rabbits. I had already shown that the monkey invariably reacted, with the production of a typical eruption, to the inoculation both of small-pox and of vaccinia. The experimental work was entrusted to Dr. Scott, of the Ministry's Pathological Laboratories at Carlisle Place. To him I sent a number of capillary tubes of lymph obtained from several different patients at Lowestoft and at Beccles. From a certain number of the earlier cases which had arrived at the final stages of the eruption "crusts" were also obtained for the purpose of inoculation; and, in the case of each patient, a blood smear was also obtained for microscopic examination. From Dr. Scott's report it is obvious that no very definite results followed the inoculation experiments, this again apparently indicating the relationship of the disease rather to alastrim than to ordinary small-pox.

It is unnecessary on the present occasion to deal with all the administrative measures taken or advised with the object of preventing extension of the disease in the various localities in which it made its appearance. Here, however, reference is perhaps desirable to two points dealing with (*a*) the question of vaccination and re-vaccination, and (*b*) the segregation of "contacts."

In view of the observation that vaccination, unless of comparatively recent occurrence, apparently does not afford so great a degree of protection in the case of alastrim as in that of small-pox, it appeared essential, under these circumstances, that vaccination should be performed by the public vaccinators on all persons applying to them, even though a period of ten years from previous vaccination might not have elapsed, as required in the memorandum of the Local Government Board on small-pox.

As regards "contacts" also, it appeared necessary to adopt a somewhat generous interpretation of the term, as it was practically impossible in certain instances to determine who should actually be regarded as "contacts." As an illustration of the difficulty referred to may be mentioned the case of a man at Bungay attacked by the disease. In view of the facts (1) that there was apparently no case of small-pox in the neighbourhood other than Mrs. W.; (2) that the man had not left the town for some weeks previously; and (3) that the

symptoms and course of the disease in his instance were almost identical with those of the only previously known case, it is practically certain that he was, in some way, a "contact" of Mrs. W. although it could not be definitely proved that he had come into contact with that case, in the ordinary sense of the word.

As regards segregation of "contacts" the departmental view is that this procedure is usually unnecessary provided that the cases are all vaccinated and that they are visited day by day by the Medical Officer of Health or his representative, and immediately removed to hospital in the event of their showing any symptoms of the disease. The rationale of this procedure is, of course, that small-pox is believed not to be definitely infectious until the appearance of the eruption, whereas definite symptoms of the onset of the disease will usually be apparent two or three days previously. In the present outbreak, however, certain instances have occurred, noticeably in the case of Mrs. F., at Lowestoft (a somewhat severe case of the disease), where the prodromal symptoms have been conspicuous by their absence. Thus Mrs. F. definitely asserted that, prior to appearance of the eruption, she did not feel otherwise than in her usual health, and that the first she knew of anything wrong with her was on seeing a number of pimples on her chest one morning on getting up to dress. As soon as she was seen (the same day) by a medical man, her three children were removed to the house of a relative at some distance, but, nevertheless, the eldest one after the usual incubation period suffered attack from the disease.

Also, in the case occurring at Stone Yard, Beccles, the difficulties were greatly increased by the fact that the first cases to develop there were in the first instance definitely diagnosed as chicken-pox; that the dwellers in the Yard were of the poorest class, but very neighbourly, so that when the true nature of the disease in the case of Mrs. H. became apparent there was a likelihood, as indeed was subsequently proved by the event, that infection was already rife in this crowded area. Consequently it appeared desirable that all the dwellers in the Yard should be kept within these boundaries for a sufficient time to cover the probable incubation period of the disease; that owing to their poverty they should be supplied with food at the public expense; and that in the case of those earning wages they should be compensated to an adequate extent, also from public funds. These precautionary measures seemed to be specially necessary owing to the fact that the Medical Officer of Health, who is also in private practice, was almost overwhelmed by pressure of work, official and otherwise, and could not,

therefore, himself be certain of making daily visits to the area to examine the persons for possible symptoms of the disease. Moreover the police thought it highly probable that if not definitely confined to the area in question certain of the inhabitants might have clandestinely escaped from the neighbourhood, under which circumstances much difficulty would be likely to have been experienced in subsequently tracing them. A similar statement may be regarded as holding with reference to the family at Bungay in which the first case in that town developed; and it actually occurred in connexion with the extension of the outbreak to Geldeston, a brother of the woman first affected being subsequently found at Beccles in the eruptive stage of the disease.

Cambridge Disease.

A somewhat similar outbreak at Cambridge in 1903 gave rise to no little controversy, in the local press especially, owing to the fact that in its having been impossible, for a considerable time, to determine the true nature of the disease, no efficient steps were taken in the first instance to limit the spread of the infection. The first case in this outbreak was apparently reported to the Medical Officer of Health for Cambridge on May 20, 1903, but it was not until more than three weeks later that he came to know of two further cases which were notified to him as variola, while two additional cases were reported to him a couple of days later. As none of these cases appeared to be at all seriously ill, although two of them, as stated, had been notified as small-pox, the Medical Officer of Health arranged for them to be seen in consultation with Professor (now Sir Clifford) Allbutt, who had had considerable experience in small-pox outbreaks while in medical practice at Leeds, before his appointment at Cambridge. After careful examination he expressed the opinion that all four were cases of chicken-pox. In view, however, of the diagnosis on the notification form of the second and third cases, these were removed to the small-pox hospital, while the remaining three were isolated in their own homes. Between June 15 and July 2 twenty-seven additional cases, including five children occurred, the majority in a somewhat restricted area of the town originally affected.

Information of the occurrence of so many cases in the adult having reached the Medical Department of the then Local Government Board, I made a brief visit to Cambridge on July 25 and again on July 30 in order to discuss with the Medical Officer of Health the question as to

what should be done in the way of isolation and other sanitary precautions. While in Cambridge on the second occasion I saw a number of the cases with the Medical Officer of Health, and succeeded, in some instances, in obtaining photographs. Before leaving the town I called on Professor Allbutt and expressed to him the opinion that the cases would probably turn out to be small-pox rather than chicken-pox, and that it was not improbable that as the outbreak progressed cases more typical of the disease would be likely to develop. That the disease was in reality small-pox he at this time utterly refused to admit, but nevertheless I arranged with the Medical Officer of Health that all cases, so far as possible, should be removed to hospital and all other precautions taken as if in the presence of a definite outbreak of small-pox.

More than a fortnight later, Dr. Wanklyn, the L.C.C. small-pox expert, was asked to visit Cambridge and give his opinion as to the true nature of the outbreak. By this time as many as sixty-two cases had occurred, several of which more nearly resembled typical small-pox than had those which were previously under investigation. Nevertheless, even at this stage, only eight had been actually notified as small-pox and only sixteen isolated. At the end of his visit Dr. Wanklyn attended a meeting of medical practitioners of the town, at which he advised that all cases of this anomalous character should be regarded as variola.

Briefly, to finish the history of the outbreak, it should be stated that up to August 22, the date on which the last case appeared, 154 cases in all had been reported, fifteen of which died, a somewhat high rate of mortality in view of the exceptional mildness of the attack, as it appeared in the first instance. Possibly, however, the number of deaths was in part due to the fact that so many of them were not afforded the advantage of hospital treatment, having instead been isolated in their own homes, where proper facilities were for the most part wanting.

As regards the number of deaths that occurred, it will be observed that this outbreak at Cambridge was of a more serious nature than the outbreak recently investigated at Lowestoft and the neighbourhood, although the cases which first appeared, in practically all respects, especially as regards the appearance and development of the eruption, resembled those of last year's outbreak, of which I have given an account.

To sum up briefly the conditions of similarity between the two outbreaks, it may be mentioned that on June 30, 1903, when I first saw a number of the Cambridge cases in consultation, I specially noted the following circumstances :—

(1) In no instance had backache been experienced in the early stages of the attack.

(2) In none had there been any secondary rise of temperature.

(3) The eruption had appeared, for the most part, in definitely successive crops.

(4) The rash was about equally distributed on extensor and flexor surfaces.

(5) The vesicles were more or less globular in form, dull white in colour, not umbilicated, and unilocular.

In the Cambridge outbreak the mildness of the earlier cases of the disease, and of cases in vaccinated persons under the age of twenty was especially noticeable. The vaccinal condition of all the patients, together with the deaths and the case-mortality per cent. in two groups, vaccinated and unvaccinated, was as follows:—

	Number of cases	Number of deaths	Case mortality
Vaccinated ...	138 ...	11 ...	7·97 per cent.
Unvaccinated ...	16 ...	4 ...	25·0 ,,
	154 ...	15 ...	9·76 ,,

In conclusion it may be mentioned that, unfortunately, the original source of infection could not be traced.

Varioloid Varicella in Trinidad.

About the same time that Cambridge was suffering from the outbreak just described, epidemics, in some instances described as small-pox, and in others by some modified term, were reported from various localities in Australia, America, and the West Indies. Time does not permit of description of these various outbreaks so that the remainder of my paper must be limited to a description of one only of these—an extensive epidemic which prevailed in the island of Trinidad in 1902, during the course of which over 4,000 individuals were attacked.[1]

This particular outbreak is here cited, partly for the reason that it was carefully investigated at the time by a number of special investigators sent to Trinidad for that purpose, and partly for the reason that according to the description published by these investigators at the time, it would appear more closely to resemble the particular outbreak in Lowestoft and elsewhere, which I have already described, than

[1] *See* also paper by R. Seheult "On an Epidemic of Small-pox of Irregular Type in Trinidad during 1902-4." *Proceedings*, 1908, i (Epid. Sect.), pp. 229-302.

any outbreaks other than those more recently reported from South America under the name of alastrim, to which I have already referred.

The Trinidad disease was at first considered to be varicella, and although this opinion was subsequently modified, in a large measure, the actual nature of the disease was very difficult to determine. Although, as at Cambridge and elsewhere, severer symptoms occurred at a later period of the outbreak than were met with among the first cases reported, the general opinion at the time tended to the view that the disease failed in many respects to conform to the usual picture of small-pox. For this reason a way out of the difficulty was attempted by the invention of the hybrid term of "varioloid varicella."

As regards the *period of incubation*, considerable difficulty was experienced in the attempt to obtain anything like definite information on this point, especially as the greater number of individuals affected belonged to the unskilled labouring classes. Any history obtained from these people usually tended to be vague and indefinite, but in a certain number of cases in which more accurate details were obtainable the duration of the incubation period seems to have been about a fortnight.

As regards the symptoms appearing during the period of invasion there was a striking similarity to those usually met with in small-pox during this period, including fever, headache, and especially backache, in this respect differing distinctly, as will be noted, from the usual experiences at Cambridge. Vomiting is said to have occurred in about one-third of the cases and occasionally delirium was present. Many others merely experienced a feeling of malaise for one or two days. The first definite symptom of the disease was usually the appearance of the rash, which in every instance was the signal for a fall of the temperature to normal.

The most interesting feature of the *eruption* was seen in the fact that the rash invaded the body in *successive crops* which were definite and distinct, and not merely due to hastened or retarded development.

In a paper by Dr. Dickson and Dr. Lassalle,[1] describing the Trinidad epidemic, they stated that in cases observed in the wards of the Colonial Hospital from the first onset of fever, the rash frequently appeared first on the forearms and hands, or on the legs. Other observers state that the rash not infrequently appeared simultaneously on the face, forearms and front of trunk. The appearance and condition of the vesicles, so far as could be judged from the descriptions, much more nearly resembled the description of the eruption in alastrim than

[1] *Brit. Med. Journ.*, 1903, ii, p. 711.

in what is ordinarily regarded as small-pox. The vesicles were invariably unilocular and probably never umbilicated. They were firm in texture and contained a slight amount of more or less mucoid material which exuded with difficulty when the vesicles were pricked. Both in vaccinated and unvaccinated persons it was observed that the vesicles not infrequently dried up without becoming pustules. Occasionally bullæ formed, which ruptured easily, liberating their contents and allowing the cuticle to assume the appearance of a piece of thin wrinkled parchment, a description which coincides somewhat closely with that of the condition of affairs to which I have already referred in one of the Lowestoft cases. As occurred in the Beccles and Lowestoft cases the soles of the feet and the palms of the hands were frequently affected, in addition to the extremities, the face and the back of trunk.

Judged from the amount of eruption often present the cases might have been expected to have been of a serious nature. Nevertheless constitutional symptoms in the large majority of cases were marked by their absence, so much so, that mention is made of the fact that difficulty was not infrequently met with in persuading the patients to remain indoors.

As previously stated, fever, even though fairly high, disappeared with the onset of the eruption, while the secondary fever, typical of normal small-pox was almost entirely absent, this having been met with in thirteen cases only out of 406 treated in the isolation wards of the Colonial Hospital.

As regards *age-incidence* the malady was practically confined to the period of middle life, the largest percentage of cases, 35·5, occurring in the age-period 20 to 30.

The *influence of vaccination on the disease* appeared somewhat doubtful seeing that a large proportion of mild cases occurred in unvaccinated people, while, on the other hand, a large number of serious cases appeared in people who had been vaccinated and showed good marks. Nevertheless, vaccination as late as within the first five days of the incubation period appeared to modify the effect, while, when the operation was done at a later period vaccinia and the disease in question ran concurrently, without apparently modifying one another in any way. As with cases of alastrim, in a certain number of instances (fifteen out of 185), vaccination proved successful during later convalescence, or subsequent to complete recovery.

The fatality of the disease was remarkably low, eighteen deaths only having occurred among 4,029 cases investigated; these figures contrasting

noticeably with the experience of the years 1871-72, during which, in the course of an epidemic of undoubted small-pox in the island, nearly 2,500 deaths occurred among about 12,500 cases, giving a mortality of 19·5 per cent., as compared with a mortality of 0·44 per cent. in the cases of the outbreak to which somewhat detailed reference has been made.

These outbreaks of anomalous varioloid disease at Cambridge and in the island of Trinidad respectively, appear to constitute stages of divergence from the normal between the recent outbreak in Norfolk and the South American disease known as alastrim, on the one hand, and what would ordinarily be regarded as typical small-pox on the other. All three of the outbreaks to which I have referred appear however to resemble one another more nearly than any of them resemble normal small-pox, and it is an interesting point for discussion as to whether there actually exist distinct types of varioloid diseases other than variola itself.

As regards the protective power of vaccination it may be noted that this is apparently somewhat less marked in alastrim and the other milder forms of varioloid disease, although its influence, especially when the vaccination has been fairly recent, is still obvious—in this respect supplying a definite contrast to the entire want of protection afforded against varicella, the disease for which, in its earlier stages at any rate, cases of these anomalous varioloid diseases have not infrequently been mistaken.

When the diagnosis is in doubt as between small-pox and varicella, vaccination is usually considered to be capable of affording definite information. In accordance with the usual statement, if the disease is varicella, vaccination, especially in previously unprotected persons will "take," whereas, if the disease is small-pox, the operation will not prove successful. As a general rule, and in presence of normal small-pox, this statement may be considered to hold good. But it may be quite otherwise when the disease is of one of the anomalous types to which I have referred, and in any case the result will depend on the period at which the operation is performed. Even in the case of ordinary small-pox, indeed, this test does not necessarily afford the information desired. Thus the late Dr. Washbourn who, in a paper published in 1902, had advised the performance of this test with the

object of arriving at a diagnosis between variola and varicella, wrote to me shortly afterwards to say that Dr. Ricketts, who had previously held the same view as himself, had since heard of a case in which vaccination had been successful after variola had "come out." And as regards the anomalous types of varioloid disease which we have been considering, it will be obvious, from what has been said, that the use of this test cannot be regarded as of any real value from the point of view of diagnosis.

Personally, I think that in all probability there may be found various stages of varioloid disease between that form to which the name of alastrim (or in South Africa, amaas) has been given, and that which, in this country, is ordinarily regarded as typical small-pox. But whether or not this be the case, it is obvious that, from the practical point of view and in the present state of our knowledge, it is only by the taking of all precautionary measures as if in presence of an outbreak of ordinary small-pox, that we can hope effectively to prevent the spread of the disease.

Section of Epidemiology and State Medicine.

President—Dr. E. W. Goodall, O.B.E.

Typhus Fever in Poland, 1916 to 1919.[1]

By E. W. Goodall, O.B.E., M.D. (President).

In the summer of 1919, in response to an appeal from the Ministry of Public Health of the Polish Republic, the War Victims' Relief Committee of the Society of Friends decided to send a small unit to Poland in order to work under the Ministry in the campaign against the epidemic of typhus which the Ministry was setting in motion, and I was asked to take charge of the unit. I reached Warsaw on July 26, and remained in Poland till September 27. I spent the nine weeks I was in that country partly in Warsaw, partly and mostly in Zawiercie, a town in the south of Congress Poland, and paid visits to places in the neighbourhood of the last-named town. Such knowledge of the prevalence of typhus as I gained was derived chiefly from information given me by various officials of the Public Health Service, and partly from personal inquiry. But although I received great assistance from every official with whom I came in contact, from the Minister himself downwards, I wish to make it quite clear that in the disordered state of administration into which Poland was thrown by the events of the war, accurate information was by no means easily obtained, even by the authorities themselves, still less, therefore, by a stranger and foreigner. Consequently the figures I shall bring forward are to be regarded as being approximate only. I believe, however, that even a general statement will be interesting to the members of this Section, for I doubt if the medical profession in the west of Europe has the smallest knowledge of the extent to which typhus and other epidemic diseases have been devastating the unfortunate country of Poland. Moreover, there

[1] At a meeting of the Section, held April 23, 1920.

is no large body of statistics of notified cases of typhus in existence so far as I am aware, as by the time the notification of infectious diseases was introduced in the United Kingdom typhus had ceased to be prevalent. The *Transactions* of the Epidemiological Society and of the Epidemiological Section of the Royal Society of Medicine contain no statistical accounts of typhus epidemics, with the exception of Colonel Hunter's paper of last December, which dealt chiefly with the outbreak in the Serbian army in 1915.

The figures I shall put before you have been obtained from the following sources: (1) "A Memorandum on the Sanitary Conditions of Poland," by Dr. Tomasz Janiszewski, Minister of Public Health, published in March, 1919; (2) "A Memorandum on the Plan of Campaign against the Epidemic of Typhus in Poland," also by Dr. Janiszewski, published in June, 1919; (3) No. 4, vol. i (October, 1919) of the *Bulletin of the League of Red Cross Societies*, containing the Report of the Interallied Commission in Poland, and (4) information given me by Dr. Trenkner and Dr. Ryder, the Medical Officers of Health respectively of Warsaw and Zawiercie, and by Dr. Rajchmann, of the Epidemiological Institute, Warsaw, to all of whom I am under deep obligations.

Typhus fever was endemic in Poland and certain of the neighbouring countries for many years before the war. By Poland I mean what is now known as Congress Poland, which corresponds to what was known as Russian Poland. The neighbouring countries referred to are South-eastern German Silesia, Austrian Silesia, Galicia, and, in Western Russia, Lithuania, Polesia, Volhynia, and the Ukraine; and the countries to the east were sources of a never-failing supply, as indeed they still are. In the memorandum by Dr. Janiszewski on the plan of campaign against typhus, it is stated that the mortality from typhus in Congress Poland amounted during the last years before the war to an average of six per 100,000, a maximum of twenty-four per 100,000 having been reached in 1892-3 and in 1909; and that the number of cases varied between 6,000 and 7,000 yearly. Congress Poland has an area of about 50,000 square miles; its population in 1914 was about 12,800,000. As the Russian government did little, if anything, towards attempting to eradicate the disease, it was not, apparently, diminishing to any great extent in Russian Poland. In Galicia, however, under the Austrian government, the disease was systematically declining. The area of Galicia is about 30,000 square miles, and its population in 1914 was about 8,000,000. Then came the war, and, for reasons which will

be mentioned later, typhus increased enormously. In consequence of the disordered state of the machinery of the civil administration of the country brought about by the war, no figures that can be regarded as being even approximately accurate are available up to the end of 1915, except for the city of Warsaw; but there can be no doubt that an unusual prevalence began in 1915, especially in connexion with the movements of the Russian armies. Thus it is stated that in that year already there were in that small part of Galicia which was not invaded by the Russians, upwards of 8,000 cases of typhus, with much larger numbers of other infectious diseases.

As regards 1916 and after, for the parts of Congress Poland which were occupied by the Germans and Austro-Hungarians respectively, and for Galicia, the official figures given by the Germans and Austrians are as follows:—

CONGRESS POLAND.

German Occupation.

Year	Cases	Deaths	Fatalities per cent.
1916	16,460	1,269	7·7
1917	29,618	2,328	7·8
1918 (To November 1)	73,380	5,670	7·7

Austro-Hungarian Occupation.

1916	11,294	1,328	11·7
1917	10,446	1,175	11·2
1918 (To November 1)	15,645	(?)	—

GALICIA.

1916	6,784	883	13·0
1917	3,776	273	7·2
1918 (To November 1)	8,057	814	10·1

TOTAL NUMBER OF CASES FOR THE THREE YEARS IN CONGRESS POLAND AND GALICIA.

1916	34,538	3,480	10·0
1917	47,840	3,776	8·6
1918 (To November 1)	97,082	—	—

It has been estimated that for Galicia and Congress Poland there were, during the whole year 1918, about 122,000 cases of typhus. The figures for 1919 are more accurately known, having been obtained by the Ministry of Public Health; they are as follows, up to December 31:—

	Cases	Deaths	Fatalities per cent.
Congress Poland ...	161,042	12,632	7·8
Galicia	70,158	7,250	10·3
Total ...	231,200	19,882	8·5

Thus, for the period January 1, 1916 to December 31, 1919, there were at least 431,200 cases of typhus fever in Congress Poland and Galicia combined. The number is probably higher, for there can be little doubt that not a few cases escaped being recorded, in consequence of difficulties brought about by the conditions of war; probably also the figures given by the Austrians are under-estimated, as the Austrian administration was not so efficient as the German. The disease still continues to be prevalent; during January and February of the present year 12,098 cases have been notified in Congress Poland, and 34,476 in Galicia.

It will be seen from these figures that there has been a great and progressive increase of typhus during the war, and also and especially since the armistice. There can hardly be any doubt about this, even when all allowances are made, including those for errors of diagnosis.

From a list of affected counties which is given in one of the memoranda referred to above it appears that the epidemic was widely spread and prevailed at one time or another, with varying attack rate, all over Poland in 1918. Early in 1919 the whole country was more or less invaded, but the districts that suffered most were those in the east, south-east, and south-west (county of Bendzin, which contains the mining districts). Galicia and Volhynia were also severely affected.

The figures just given are mass figures and afford no indication as to the progress of the epidemic, nor of the seasonal and regional prevalence, but I have detailed figures for the periods of the epidemics in Warsaw and Zawiercie, thanks to Dr. Trenkner and Dr. Ryder.

Warsaw.

Typhus has always been prevalent to a slight extent in Warsaw, as in Poland generally, but of recent years there had been no epidemic till 1917, during the German occupation. In that year the number of cases rose from seventy-five per week during the first week of January, steadily though not evenly, to about 410 in the first week in June. There was then a fall to 225 during the first week of July, and after that a gradual rise with, however, interruptions, to 550 in the second week in January, 1918. In consequence of a strike amongst

the subordinate public health officials of the city from January 13 to 19, the notifications were very irregularly recorded during that and the two following weeks, and there can be little doubt that a large number of the 809 cases recorded during the week ending February 2, should have been recorded and really occurred earlier. Probably the acme of the epidemic was reached in the week ending February 23, when 775 cases were recorded. The number of cases then fell somewhat abruptly, with a moderate rise at the beginning of April, to less than fifty cases a week early in July and twenty-five a week in August. The weekly notifications remained at about twenty-five till the week ending November 21, when they rose to seventy. They then fell, but gradually rose again early in January, 1919, and again the maximum, 200 to 225, was attained in February. From the middle of April there was a gradual fall to less than twenty-five a week in July. During the whole of the first epidemic, from January, 1917, to July, 1918, there were between 24,000 and 25,000 cases.

The city of Warsaw had at the time of the epidemic a population of about 700,000 persons. I understood that this figure did not include any of the German troops, but represented the civil, Polish, population only. The epidemic started in the Jewish quarter of the city, and at first spread chiefly amongst the Jews. According to Dr. Trenkner the same thing happened at Lodz, of which city he was medical officer before he was appointed to Warsaw in 1917, and in many other places in Poland. Dr. Janiszewski confirms this statement. In the Warsaw epidemic, 73 per cent. of the cases occurred amongst the Jews, and 23 per cent. of these in one particular part of the Jewish quarter where the population was most dense. In the other quarters the number of cases was in proportion to the number of Jews amongst the inhabitants. The Jews form 30 per cent. of the population of Warsaw. Roughly, the number of cases in the different districts was in proportion to the density of population, and the density is highest in the parts of the city inhabited by Jews.

Since the epidemic of 1917-18 typhus has become more widely diffused through Warsaw, but the 1919 epidemic, if it can be called such, was comparatively slight. Lately (1919) the Christians have been attacked in larger numbers than the Jews. The attack-rate of the 1917-18 epidemic was between 3 per cent. and 4 per cent., and the fatality was about 9 per cent. It is a curious fact that the fatality amongst the Jews was half that of the Christians, 7 per cent. as against 14 per cent. Dr. Trenkner accounted for this difference by the greater

care and attention the Jews bestowed upon their sick. They also called in medical advice earlier than did the Christians, so that their patients came under treatment sooner.

As regards age-incidence I was supplied with the following figures relating to 5,747 consecutive cases occurring at the end of 1917 :—

Age		Cases		Deaths		Fatalities per cent.
0—10	...	908	...	7	...	0·7
—20	...	2,407	...	29	...	1·2
—30	...	1,035	...	43	...	4·1
—40	...	717	...	71	...	10·0
—50	...	513	...	86	...	16·7
—60	...	112	...	59	...	52·6
—70	...	50	...	19	...	38·0
—80	...	5	...	3	...	60·0
	Total	5,747		317		5·5

It is evident that these figures relate to a period of the epidemic when the fatality was below the mean. The low fatality in children will be noticed. Dr. Trenkner told me that, amongst the 908 patients under 10 years of age, there were fifty-four infants under 18 months, and not one of them died. He had reason for thinking that a considerable number of cases in children escaped notification because of their mildness.

As regards the age-incidence these figures correspond fairly closely with those of the London Fever Hospital as given by Murchison, and also of the hospitals of the Metropolitan Asylums Board, in the days when typhus was met with in London. They are the only extensive statistics bearing on this point to which I have had access. In respect of the fatality at different ages, however, the Warsaw figures differ from Murchison's, for in the London Fever Hospital cases the fatality under 10 was higher than that between 10 and 20. But the Asylums Board figures agree with those of Warsaw.

Zawiercie.

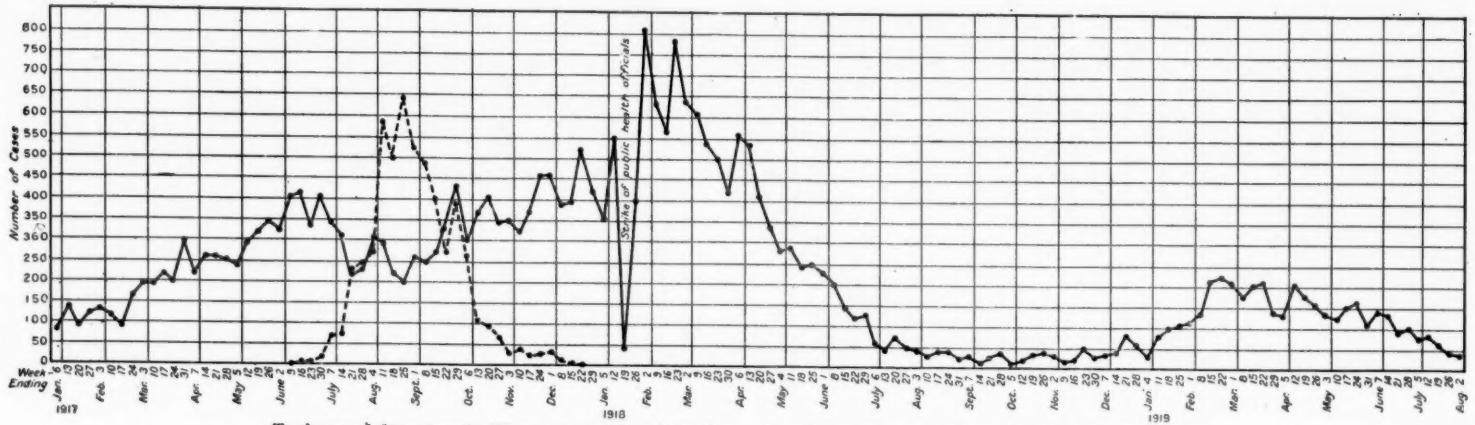
The daily figures for typhus from January, 1916, in this town have been given me, and I have charted the weekly total since that date. From these it appears that there was very little typhus during 1916 and 1917. There were slight rises in the number of cases in March, April, and October, 1916, and June, 1917. In 1918 there was a small

epidemic which, having started in November, 1917, attained its height in the following February and March, when there were nineteen cases for the week ending March 2, after which the numbers remained slightly raised, but with considerable weekly fluctuation, until August. The total number of cases from November, 1917, to August, 1918, were 198. Then followed a sharp epidemic which began in October, eleven cases for the week ending October 19. The rise in the number of cases continued with considerable weekly variations, and culminated in the week ending April 12, 1919, during which eighty cases were notified. The epidemic declined slightly more rapidly than it rose, though not evenly, to the end of July. During August and September the cases were being notified at the rate of one a day. The total number of cases from the beginning of October, 1918, to the end of July, 1919, was 1,340.

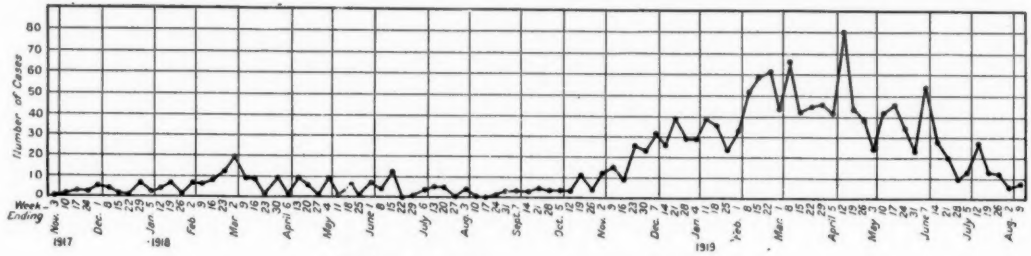
At the time of this epidemic the population of Zawiercie was about 44,600, so that the attack-rate was about 3 per cent. From official figures which were given to me it appears that the Jews formed 19 per cent. of the population. According to Dr. Ryder the Christians were attacked in a larger proportion than the Jews, as shown in the following table, which deals with about three-quarters of the epidemic and with the first six months of 1919 :—

	Males	Females	Total	Deaths	Per cent.	Christians		Jews	
						Cases	Per cent.	Cases	Per cent.
January ...	71	68	139	15	10·7	129	92·8	10	7·2
February ...	100	113	213	16	7·5	163	76·5	50	23·5
March ...	100	111	211	18	8·5	198	93·5	13	6·2
April ...	97	111	208	29	13·9	195	93·7	13	6·3
May ...	60	89	149	19	12·8	129	86·5	20	13·5
June ...	46	66	112	23	20·5	100	89·3	12	10·7
Totals ...	474	558	1,032	120	11·6	914	88·6	118	11·4

The Jews were said to be less cleanly than the Christians, and from what I saw of them I should say that this was true. But there were reasons for thinking that there was more concealment of cases amongst the Jews; the authorities had had some trouble in getting certain of the Jewish medical attendants to notify.



Typhus and dysentery in Warsaw: weekly notifications. Continuous line = typhus; dotted line = dysentery.



Typhus in Zawiercie: weekly notifications.

There are one or two points in these epidemics to which I wish to draw attention. So far as I know, there are few, if any, published figures of the number of cases week by week of a typhus epidemic. Our knowledge of the prevalence of the disease is derived partly from the returns of fatal cases and partly from records of admissions to hospitals. Hence I venture to think the figures of the Warsaw and Zawiercie epidemics are of interest. From his observations at the London Fever Hospital during the twenty-three years 1848-70, Murchison came to the conclusion that typhus epidemics occurred chiefly in cold weather, and therefore most frequently in winter and spring. The seasonal prevalence both of the Warsaw and Zawiercie epidemics were the winter and spring, and I have been given to understand that during the war, at any rate, those have been the seasons for the typhus prevalence in Poland and Galicia. It will be noticed that both the typhus epidemics in Zawiercie culminated in the early spring and persisted though less severely during the summer. In the 1917-18 epidemic of Warsaw the height of the epidemic was reached in February, rather earlier than in the case of the second Zawiercie epidemic. It did not persist into the summer to such an extent as did that Zawiercie epidemic, and declined steadily during April, May and June. This decline is attributed by the local authorities to the vigorous campaign against the disease which was set on foot in April and carried on for three months, and to which I shall refer later. It is to be observed however, that the Zawiercie epidemic of 1919 declined rather more abruptly than the Warsaw epidemic, and no such special measures were undertaken to combat the epidemic as was the case in Warsaw. It is also to be noticed that the Warsaw epidemic began in the spring of 1917 and continued fairly steadily through the subsequent months of the year. The Zawiercie epidemic of 1919, attributed by the local authorities to the laxity in observing sanitary regulations, which set in on the departure of the Germans, was preceded by a smaller one the previous year, while the Germans were in full possession, and, indeed, showed signs of commencing even before the Germans left.

There is now absolutely no doubt that typhus is spread almost entirely by means of the body or clothes louse. There are some authorities who believe that it can be spread also by the head louse, but if that be the case, it is difficult to account for the diminution of the disease in Great Britain, for I am sorry to say that in London the proportion of lousy heads amongst certain classes of the population is still rather high. Amongst patients of all ages admitted to the North-

western Hospital, Hampstead, at the present time the percentage of lousy heads is about 59. Twenty years ago at the Eastern Hospital, Homerton, it was 60; so that there has been no improvement in that matter of cleanliness. Detailed observations as to the connexion between the prevalence of typhus, meteorological conditions and the degree of lousiness of the population, are much wanted, if we are to explain correctly several questions which arise in respect of typhus. For example, what connexion is there, if any, between the age-incidence of typhus and the age-incidence of louse infestation?

The case mortality both for Congress Poland and Galicia, and in the Warsaw and Zawiercie epidemics, was not high when we consider the conditions under which the population were living. In consequence of typhus not having been prevalent to any large extent in the United Kingdom since the introduction of the notification of infectious diseases, there are no figures relating to large numbers of cases except for patients in hospital; and as regards such patients there can be no doubt that a large proportion of the worst cases were taken to hospital, so that hospital statistics do not represent the truth as regards case mortality. Murchison, in discussing this question, came to the conclusion that the case mortality of typhus was probably not more than 10 per cent. In the gross figures for Poland and Galicia, given above, it will be noticed that the case mortality varied from 7·2 to 13·0 per cent. It will be noticed also in the Zawiercie figures for the first six months of 1919 the monthly case mortality varied very considerably, from 7·5 to 20·5 per cent.

Causes of the Prevalence of Typhus.

It is not difficult to account for the wide prevalence of typhus in Poland since the beginning of the war on general grounds. Constant warfare, the movements of troops, the influx of refugees from the districts which were the actual scenes of fighting, the return of prisoners of war, especially since the armistice, in both directions across the country, the lack of soap and clothing and of medical and surgical necessities in the country districts and in many of the towns the difficulty of obtaining sufficient water, would be factors conducing to the prevalence and dissemination of lice, that is to say of typhus, in a country where the disease had been endemic before the war. Medical men and nurses have been very scarce, and there has been a deficiency of food for the poorer classes, especially in the East and South-east.

The figures I gave at the commencement of this paper showed that typhus had been especially prevalent since the armistice. There is no doubt that when the Germans and Austrians established themselves in Poland in 1915, they both, and especially the former, used their utmost endeavours to keep infectious diseases under control, not from any love they bore to the Poles, but with the object of keeping their armies free from sickness. There can also be little doubt that to a certain extent, especially in the country and smaller towns, they succeeded. In spite however of their efforts there was the large epidemic in Warsaw in 1917-18. Dr. Trenkner attributed the epidemic chiefly to the action of the Jews. Much smuggling, especially of food, went on from outside into the city, the smugglers, who were chiefly Jews, hid and slept together in little groups in sheds and barns. Members of the groups became infected with typhus and carried the disease into the city. Dr. Trenkner on various occasions traced fresh cases to group infection in this way. Overcrowding and want of cleanliness did the rest. In Zawiercie the action by the Germans seems to have had more effect, and there was not any great prevalence of disease there before they left. In that part of Poland which I visited—viz., the county of Bendzin, typhus had become especially rampant since the armistice, as was exemplified in the Zawiercie epidemic. Directly the Germans left there was an unrestrained movement of population to and fro between the town and surrounding country; released and escaped prisoners of war began to return, especially from the East; and refugees flocked to the West from the devastated Eastern districts. Thus between November 1, 1918, and January 1, 1920, 652,604 prisoners of war returned westwards from Russia, and 627,088 emigrants came back from the same country to Poland. All these people were registered at the various points in Eastern Poland through which they passed. The number of the refugees from the devastated regions was estimated at 200,000. Besides these, 553,845 Russian prisoners of war were registered as having returned eastwards from Germany and Austria. The Germans had been severely thorough in their sanitary measures. They set up de-lousing stations and forced the inhabitants to be de-loused at the point of the bayonet. When they left compulsion ceased and personal cleanliness diminished.

In Warsaw, after the Germans left, there was a slight increase in the number of cases of typhus, but the rise was not very noticeable until January, and the epidemic reached its height in February and March. Considering, however, the greater freedom of movement

amongst the people and the influx of repatriated prisoners of war and refugees from the devastated regions, it is rather to be wondered at that a more severe epidemic did not arise. By the following July the population of Warsaw had increased by another 100,000 persons, and the city was very much overcrowded; besides that, the scarcity of certain necessaries, and especially soap, clothes, and linen, became rather great. This scarcity exists to a larger extent in small towns in the country than it does in the greater cities, though it is bad enough in some of these. Even in some of the best hospitals in Warsaw the want of linen was noticeable even to the most casual observer, and in the smaller towns and villages the scarcity was appalling. Some faint idea of the dearth of soap may be obtained from knowledge of the fact that quite a small piece of the commonest kind of soap cost 4 marks, and for tablets of better quality anything up to 20 marks a tablet was demanded. At that time the value of the mark was about 190 to the English pound. Since that time the mark has still further depreciated in value and has now reached 650 to the pound. The price of war articles has gone up greatly, and I have been recently informed on authority I know I can rely upon, that the articles I have mentioned are still more scarce than they were last year.

Another very potent factor has been overcrowding, especially since the armistice, and especially in winter time. In other countries and in former years typhus has always been a disease of the winter, doubtless because in that season people of the poorest classes herd together more densely, and change their clothing and wash less frequently, than at other times of the year. These causes would be especially present in such a country as Poland when the winter is as a rule severe, and in smaller towns and country places water is obtained with difficulty. During and since the war fuel, especially coal, has also been scarce, so that warm water cannot be provided in large quantities. I have alluded to the greater freedom of movement among the people since the armistice. At the same time the train service has been limited and therefore the trains are almost always crowded to excess. Under these circumstances it is not difficult to understand how easily lice are conveyed from one person to another and one place to another.

I happened to be in Poland at the end of the summer and the beginning of the autumn, and that is the season of least prevalence of typhus. The diminished prevalence at that season is due to the fact that there is less overcrowding than in the winter, because the people are scattered in the country for the work of the harvest,

also water is more easily obtained and bathing more pleasant than in the winter. Although in Warsaw and other places the Jews suffered more severely than the Christians, it is doubtful, in my opinion, that they so suffered because they were Jews: the more probable reason is because they were more densely crowded together, for, as has been mentioned, the Jews were less attacked in Zawiercie than the Christians, and as far as I could see from inspection of houses in different quarters of the town, amongst the poorer classes, there was as much overcrowding amongst Christians as Jews.

Adverse, however, as the circumstances have been in Poland, during and since the war, it must not be supposed that the authorities have not attempted to deal with the epidemic. As far back as April, 1918, that is to say, six months before the Germans quitted Warsaw, Dr. Trenkner made a great effort to cleanse the houses and their inhabitants in the worst and most crowded parts of the city, a proceeding to which the Germans offered no objections, as of course such a measure was conducive to keeping their army free from infection. But the task was a very difficult one as the people were by no means anxious to help the authorities. If the inhabitants of a certain square for instance got wind that their houses were going to be visited by the sanitary squad, they cleared out and locked their rooms up. However, this obstacle was overcome by making unexpected visits very early in the morning, taking the passports away from the inhabitants, who were sent off to the de-lousing station, with the instruction that they would not receive their passports back again until they produced the certificate that they had been de-loused. Meanwhile, their homes were disinfected and cleaned. A movable disinfector was taken round for the purpose. This campaign was carried on very vigorously for three months, and to it must, I think, be attributed, at any rate partly, the smallness of the epidemic which occurred not long after the Germans vacated the city. It should be added that both before and after this special campaign a considerable amount of preventive sanitary work was carried on.

As regards Poland generally the Ministry of Public Health had planned a most comprehensive scheme to fight the epidemic. The proposals were, in the first place, that every effort should be made to prevent fresh infection entering the country from beyond the Eastern and South-eastern frontiers, that is from regions where typhus is more rampant than in Poland itself, by setting up a cordon of housing barracks, epidemic hospitals and de-lousing stations on the principal railway lines; and, secondly, that the foci already existing

in the country should be dealt with somewhat in the same manner as in Warsaw.

In order to carry out these measures the country was divided into districts, and the necessary staff laid down for each district. Estimates were also made of the machinery and material that would be required. Besides these measures a great effort was to be made to deal with the general insanitary condition of the country, which is especially bad in Congress Poland. Unfortunately, however, the Ministry was met by a great dearth of both personnel and material, and an appeal was made to the Governments of the Allies, and to private organizations, for help. I am afraid, however, the response to this appeal was at that time but feeble. In order to get any preventive work done with a view to stopping an epidemic in the winter, the measures indicated should be commenced not later than the beginning of the summer. I understand that neither Warsaw nor Zawiercie have suffered from an epidemic during the past winter, but that the disease has been extremely prevalent in Eastern and South-eastern Poland and very severe in Eastern Galicia.

Although this paper deals only with typhus, it should be mentioned that large numbers of other communicable and epidemic diseases had occurred and were continuing to occur in some parts of the country—namely, relapsing fever, dysentery, cholera, enteric fever, small-pox, and malaria. Tuberculosis in its various forms is also rife. There was a severe epidemic of dysentery in Warsaw from the beginning of July to the middle of October, 1917, just when the typhus epidemic was on the rise, and there were 2,434 fatal cases. There was also a virulent epidemic of influenza in Poland at the end of 1918 and beginning of 1919.

Clinically, the disease, typhus, presented the same appearances as I have observed in cases in London some years ago; but complications were infrequent: pulmonary complications and parotid buboes were the most common.

DISCUSSION.

Lieutenant-Colonel W. BYAM: I think we must be particularly careful before we accept epidemiological evidence that the head louse cannot convey typhus. Both the head louse and the body louse are now considered to belong to one species, *Pediculus humanus*, being simply varieties (*capitis* and *corporis*), and from the evidence we possess it is extremely probable that they are equally

capable of transmitting infection. That both varieties do not transmit disease with equal facility is possible, but the explanation should, in my opinion, be sought in the methods by which the insects are disseminated. The body louse spreads as the result of close contact between the infested person and his neighbour, especially in warm surroundings, for stray lice but rarely find a fresh host. Only the larvæ and adult insects are transferred in this way, though considerable numbers may pass from man to man on any suitable occasion. On the other hand, the head louse may often be transmitted as an egg while still adhering to a detached hair, the hair being conveyed by means of brushes, combs and articles of head gear, or by being picked up, as when a clean head rests against the back of an upholstered seat recently vacated by one who is lousy. It is seldom that lousy heads come directly in contact, or that adult insects are transferred. As a result, the percentage of lice which are infected becomes a factor of importance. In the case of typhus, exact knowledge of the percentage of lice which are infected is not available, but the greater the number of lice transferred the greater is the risk of infection being transmitted. If body lice are transferred in greater numbers than head lice the more marked will be their power of transmitting disease, even though the eventual degree of infestation may be the same in both cases. From our knowledge of the allied disease, trench fever, we know that infection is not transmitted hereditarily in the louse and, from the evidence at present available, it is more than likely that the same holds good for typhus. Should this be true and the dissemination of head lice be mainly by the transference of eggs, it would follow that head lice would play little part in the spread of either typhus or trench fever. The second aspect of typhus on which I would like to speak is its prophylaxis as aimed at by disinfestation. By the kind permission of Lieutenant-Colonel P. S. Lelean, C.B., Professor of Hygiene at the Royal Army Medical College, I am able to show you some pictures[1] of a disinfestor designed by him which will be largely employed in the Army for field work in future. The advantages of this apparatus are its efficiency, portability, cheapness, and the ease with which it can be worked. And for these reasons I think it should be of extreme value in the campaign against typhus which Dr. Buchanan has told us is about to be undertaken by the League of Nations. The pictures, I think, are self-explanatory, but I may add that each of the sacks you see is capable of dealing with the kits of ten soldiers every half hour; and that, with the exception of the sack, the boiler and the chimney, the apparatus is built of mud and, therefore, does not require to be transported. The efficiency of disinfestation depends on the fact that the steam is introduced from *above*, thereby displacing all the air from the clothing before the steam can escape below from the mouth of the sack.

Sir WILLIAM MACPHERSON: May I suggest that the future as regards epidemics of typhus in Poland may not be so dismal as it appears from the

[1] *See* Byam and Archibald's "Practice of Medicine in the Tropics," Oxford, 1920.

extensive prevalence of these epidemics during 1916 and 1919, of which Dr. Goodall has given us so clear an account? For this reason: If, as I understand, an attack of typhus confers immunity, the time must come when the non-immune material will be exhausted, and I would be inclined to anticipate a much less incidence of typhus fever on that account in the future among the slum population of the towns which Dr. Goodall described. In other words, in the future we should have in these slums an immune population, and, therefore, a less incidence of typhus. Perhaps Dr. Goodall would let us have his views on this point.

Section of Epidemiology and State Medicine.

President—Dr. E. W. Goodall, O.B.E.

The Apparent Rate of Disappearance of Diphtheria Bacilli from the Throat after an Attack of the Disease.[1]

By Percival Hartley, D.Sc., Captain R.A.M.C.(T.), and C. J. Martin, M.B., D.Sc., F.R.S., Lieutenant-Colonel A.A.M.C.

(*From the Lister Institute.*)

The persistence of *Bacillus diphtheriæ* after convalescence has been studied by many bacteriologists. The longest period which we have found recorded in the literature is six hundred days, observed by Prip (1901). The present communication, however, is concerned more particularly with the rapidity with which patients appear to become free from the bacilli. Data on this subject have been collected by Park and Beebe (1894), Woodhead (1895-96), the Massachusetts State Board of Health (1896-1905), Prip (1901), Westbrook (1905), Scheller (1906) and Tjaden (1907), but, although they deal in some cases with considerable numbers, they are none of them entirely satisfactory for our purpose. We propose, therefore, to deal first with our own observations, which, though not so numerous, were made under exceptionally favourable circumstances and the data possess a degree of homogeneity which is difficult to attain in civil practice.

The population was composed of young adults, admitted into a military hospital for infectious diseases in Rouen during 1917 and 1918. They were given 8,000 to 20,000 units of antitoxin, according to the day of the disease on which they came under observation. The samples from the throat were taken by the same medical officer. The methods of examination were also uniform and carried out by the same

[1] At a meeting of the Section, held May 28, 1920.

individuals. They consisted in the cultivation within an hour, of the material from the throat-swabs upon inspissated serum and microscopical examination of the growth after twenty-four, and if necessary, forty-eight hours. The diagnosis rested upon the morphology and staining properties of the bacilli, Gram's stain, methylene blue and, when desirable, acid toluidene blue being employed. Examinations were carried out on admission and subsequently every seven days until three successive negatives were obtained. The time the patient reported sick with a sore throat was taken as the day of onset and the time he apparently ceased to harbour the bacilli was computed to be the period half way between the last positive finding and the first of three successive negatives.

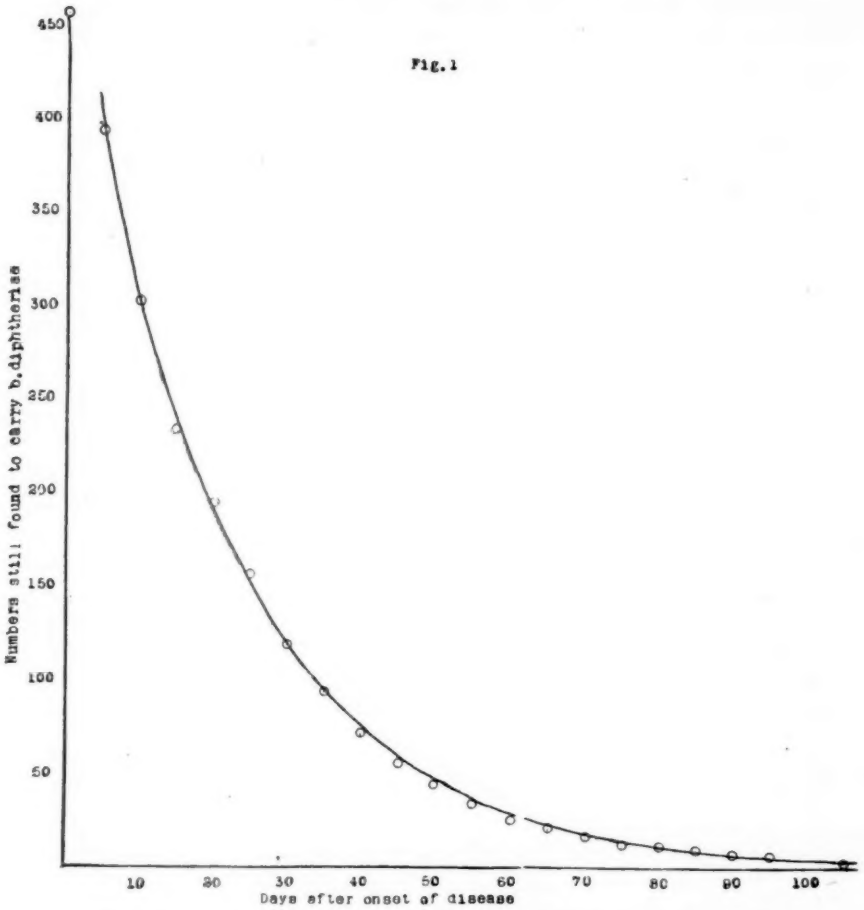
Three thousand and seventy observations upon 457 cases are suitable for analysis. The data are complete for a period of fifty days. After that we gradually lost sight of twenty-nine cases, which were evacuated to England in order to relieve pressure upon the hospital.

Analysis of the Observations.

We present our results, in the first instance, in the form of a graph (fig. 1) showing numbers still found to carry *Bacillus diphtheriæ* plotted, as ordinates, against periods of five days along the abscissa. The numbers fall rapidly at first, then more and more slowly. It will be seen that after the first five days have elapsed, the observations fall upon a curve of some regularity. That the bacilli should disappear less rapidly during the first few days, might be expected, for the patients are still suffering from the disease during this period.

The form of the curve (fig. 1) suggested a logarithmic relationship and fig. 2 shows another graph in which, as ordinates, logarithms are substituted for the numbers themselves. It is clear from this graph that the population still carrying bacilli after various intervals of time, is now well expressed by a straight line, provided the first period of five days is excluded. The best fitting line was kindly calculated for us by Dr. Major Greenwood, using the method of least squares. This line is drawn on the graph (fig. 2), and is represented by the formula $\log. y = 2{\cdot}6002 - 0{\cdot}0218\,x$. In the table below, the numbers still harbouring bacilli at each period of five days, calculated by this equation, are set out and against them the figures actually observed.

Fig. 1

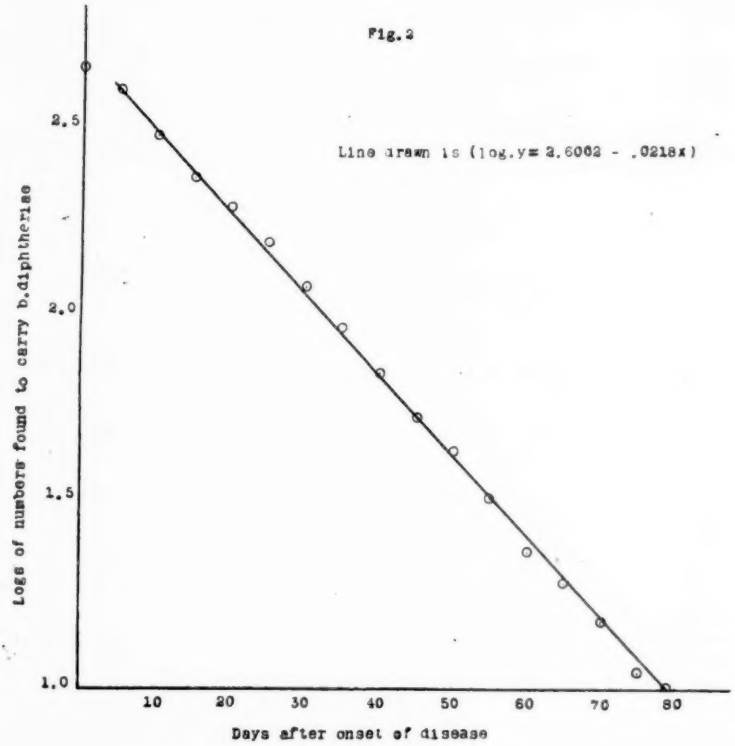


APPARENT RATE OF DISAPPEARANCE OF *Bacillus diphtheriæ* FROM THE THROATS OF A YOUNG ADULT POPULATION.

Days after onset of disease	Number still found to carry: Observed	Computed ($\log y = 2{\cdot}6002 - 0{\cdot}0218\,x$)
5	392	398
10	302	310
15	232	242
20	194	189
25	156	147
30	118	115
35	92	89
40	70	70
45	52	54
50	41	42

$P > 0{\cdot}99$ for ten groups.

The two columns show excellent agreement and the conclusion seems justified that the rate at which a population frees itself from diphtheria bacilli is represented by the simple expression $\frac{\log n_1 - \log n_2}{t_2 - t_1} = k$ where n_1 and n_2 are the numbers still carrying at times t_1 and t_2. The constant expresses the velocity at which the carrier state apparently disappears. A constant of 0·0218 means that approximately 5 per cent. of those remaining each day become free from the bacilli within twenty-four hours.

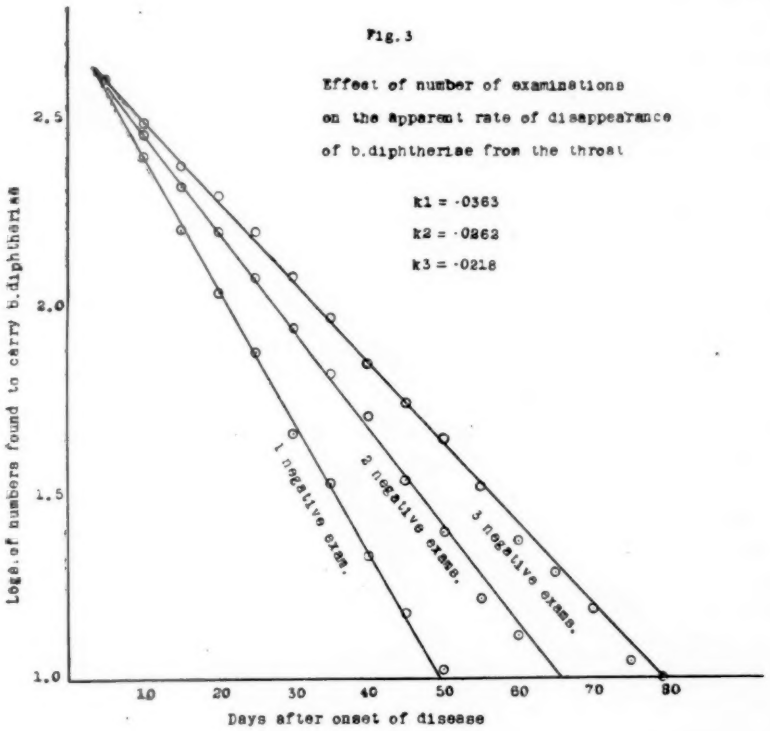


Such a simple relationship suggests that the disappearance of diphtheria bacilli from the throat is due to the operation of a number of small causes, in other words, to chance. Generally speaking, an individual who has carried the bacilli for a couple of months is just as

likely to become free in the next few days as one who has carried for a week. This statement may, however, require slight modification in the case of persons with abnormal throats. These will be referred to later.

Effect of Changing the Criterion upon Apparent Rate.

So far, three successive negatives at weekly intervals has been our criterion of freedom. Other observers have deemed two or even one sufficient. The standard applied is a compromise, for nobody supposes



that obtaining three negatives in succession excludes the possibility of discharging some few convalescents still harbouring diphtheria baccilli.

Fig. 3 represents the rate of apparent disappearance of bacilli when

one, two or three successive negatives is the criterion of freedom. In each case the logs. of the numbers still carrying are plotted against time. The observations lie well along three straight lines and the constants are 0·0363, 0·0262, and 0·0218 respectively.

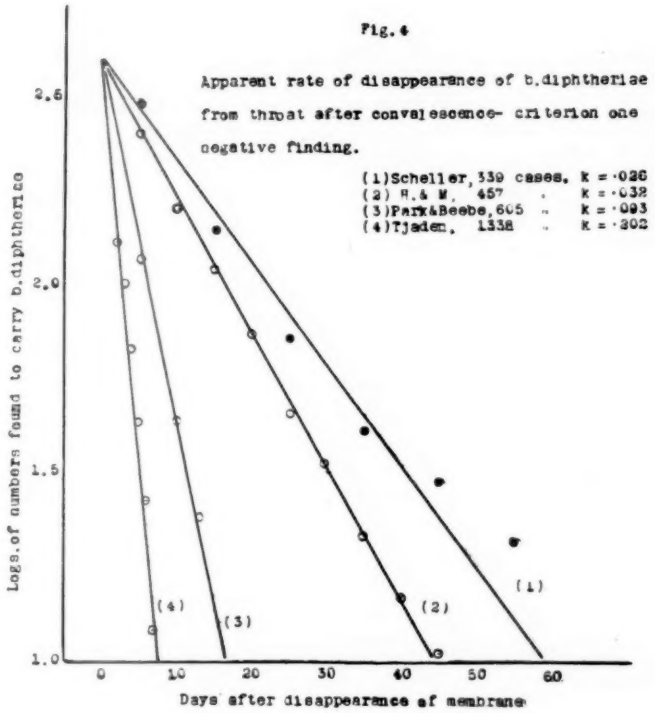
We are now in a position to consider the effect of enlarging the mesh of our net, by substituting two successive negatives or one negative, for the three. A constant of 0·0218 means that this amount, subtracted from the log. of the number carrying on Monday, will give the log. of the number which will probably be found to be still carrying on Tuesday. An arithmetical expression for each of the three constants, in terms of percentages which apparently became free each day, may be arrived at by subtracting each from the log. of 100 and finding the numbers corresponding to the residues. In our case, these are 95·1, 94·6, and 92·9, and these figures represent the individuals out of an original 100 whom we should have returned as still carrying, if we had based our conclusion upon three, two, or one negative finding respectively.

The difference in the number of carriers discovered by repeating the examinations two or three times is not great and the error per examination of the order of 2·5 per cent. An error of this magnitude becomes, however, considerable, when it all falls upon the small percentage declared free each day. With three negatives required, our population appeared to become free at the rate of 4·9 per cent. per day. This was increased to 5·4 per cent. if two negatives are demanded and to 7·1 per cent. as the result of only one negative finding. Put in another way, the number discharged would have been increased by 9·3 per cent. and 29 per cent. respectively and the persons constituting this increase would have been carriers.

The proportion of carriers missed, notwithstanding three successive negative findings, is unknown, but, from the smallness of the error for one examination, it cannot be considerable. On the other hand, there is no indication that a minimum value for k is being rapidly approached. This is likely due to reinfection amongst the convalescents whilst they are waiting for three successive negatives. Dr. Greenwood has pointed out to us that if the reversion to the carrier state is occurring *pari passu* with its disappearance, the proportion no longer carrying at time t will not be $1-e^{-gt}$, but, by an application of the reasoning of Ross (1916),

$$\frac{g}{g+r}\left(1-e^{-(g+r)t}\right)$$

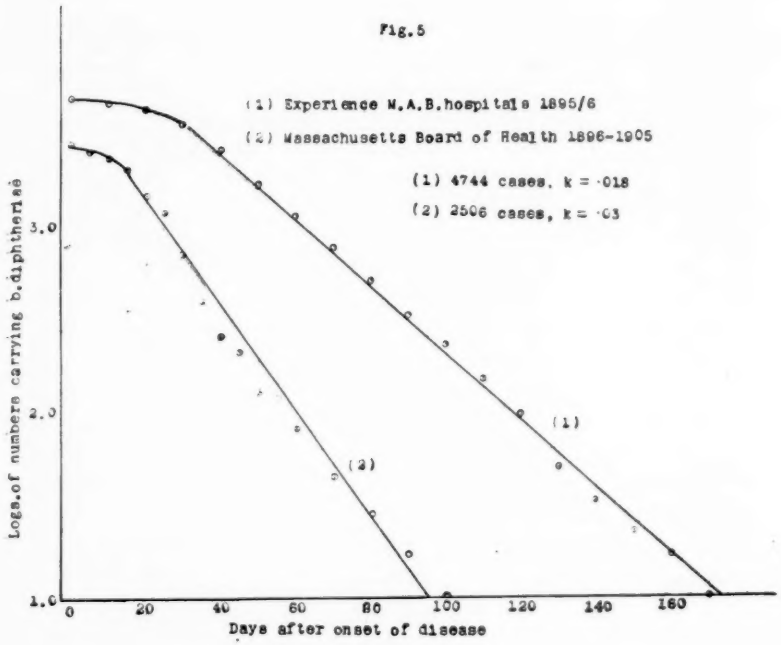
where r is a second constant measuring the rate of reinfection. Greenwood proposes to attempt to readjust the data to test the possibility.



COMPARISON OF OUR RESULTS WITH THOSE OF OTHER OBSERVERS.

Fig. 4 represents observations made by Park and Beebe, Scheller, and by Tjaden, together with one of our series, for comparison. The criterion in all these observations is one negative finding and time is reckoned from the disappearance of the membrane. The observations of Park and of Tjaden show a logarithmic relation. Those of Scheller are irregular. They were made upon out-patients and he lost sight of a good many of his cases.

In the next graph (fig. 5), the results obtained by Woodhead from the examination of cases in the hospitals of the Metropolitan Asylums Board during the years 1895 and 1896 and those published by the Massachusetts State Board of Health for the years 1896 to 1905 are similarly presented. In the case of the Metropolitan Asylums Board experience, the numbers are considerable, but the time of onset of the disease is only approximately known, and the treatment of the patients



varied in the different hospitals and from time to time. Nevertheless, the English observations show a logarithmic rate of disappearance of bacilli after the first month from the onset of the disease. As in the previous graph, the criterion was one negative finding. The striking fact which emerges on comparison of the results of these different observers is the variation in the velocity constants, the rate in Tjaden's observations being eleven times as rapid as in Woodhead's. It is difficult to explain this variation. There is no reason to suppose that

the technique of one observer was superior to that of another. Variation in the properties of the diphtheria bacilli in different epidemic periods seems to us the most probable explanation, some strains being with more difficulty cast out. Some part of the difference may, however, be due to the different circumstances under which the convalescents lived. They will undoubtedly cease to harbour diphtheria bacilli more quickly in their own homes than in a diphtheria ward, where opportunities for reinfection are considerable, especially amongst children. In this connexion, the probable beneficial effect of antiseptic toilet of the throats of the convalescents in hastening the rate of disappearance of the bacilli by reducing the reinfection rate, is worthy of consideration.

Another difference between our observations and those of the Metropolitan Asylums Board and Massachusetts figures, which are the only figures in which the graph starts from the onset of the disease, is that the period intervening before the steady fall begins is much greater in the latter. We imagine that this is to be attributed to the slower clearing up of the throat lesion, for in those early days the type of diphtheria was graver and the dose of antitoxin administered much less.

The Average Stay in Hospital Required.

This depends principally upon the rate at which the patients become rid of the bacilli. As this process proceeds logarithmically, the rate is measured by the velocity constant k. We have been able to measure the influence of the criterion of freedom demanded upon this constant, but in the preceding paragraphs we have drawn attention to the fact that, even when the criterion was apparently the same, the velocity constants derived from the experience of different observers in different countries show great divergence. We are unable to explain this and it is probably a coincidence, that the constant we obtain from the analysis of the early figures of the Metropolitan Asylums Board hospitals is approximately the same as that arrived at from our own observations. The average stay in hospital necessitated to attain freedom from infection at Rouen was forty-five days; twenty of these were taken up in procuring the information. During this latter time the patient might be, and often was, fit to leave.

This figure has been arrived at by multiplying the number of observations for each five-day period by the time the individuals remained infective, adding these products, dividing the result by the total number of observations and adding twenty. Here again, Dr. Greenwood has come to our assistance and calculated it from the logarithmic curve (fig. 2) by the approved actuarial method for calculating the mean expectation of life. He arrives at the same figure, a further testimonial to the accuracy of the logarithmic relationship.

Supposing but two successive negatives, or one negative, had been required, the mean stay in hospital to become supposedly free from infection can be arrived at from the relation of the constants of the three graphs in fig. 3. In the first case it will be $\frac{25\,k_3}{k_2} + 13 = 34$; in the second, $\frac{25\,k_3}{k_1} + 6 = 21$. The diminution in accuracy accompanying this economy is shown in tabular form below.

Criterion	Percentage discharged as free who are really carrying	Days in hospital
Three successive negatives	x ...	45
Two successive negatives	$x + 9{\cdot}3$ ...	34
One negative	$x + 29{\cdot}0$ ...	21

We have already referred to the case of convalescents with abnormally large tonsils with deep crypts, who seem prone to become stubborn carriers. We encountered four such and from their throats abundant growth of *Bacillus diphtheriæ* was obtained on every occasion. We can support the opinion of Pegler (1905), which has been recently emphasized by Sears (1919), that the best treatment of them is radical enucleation of the tonsils. This was done in the cases which came under our observation and the subsequent findings were uniformly negative. In sections of the tonsils the bacilli could be seen deep down in the crypts, which in one case were over a centimetre in depth. They did not invade the tissue of the tonsil.

In conclusion, it is our pleasing duty to express our indebtedness to our colleague, Major McHattie, R.A.M.C. (T.), who had charge of the cases, for his kind co-operation. Without his cordial assistance, it would not have been possible to have attained the same satisfactory uniformity in the conditions under which the observations were carried out. To our colleagues in the laboratory, Sister F. E. Williams, R.R.C. of the Australian Nursing Service, and Mr. Reginald Brain, late

Sergeant, R.A.M.C., who together carried out the greater part of the examinations with great skill and faithfulness, we tender our hearty thanks. Lastly, we acknowledge the help we have received from Dr. Major Greenwood in the statistical treatment of our observations. Without his stimulating advice and criticism, the presentation of our results would have been less effective and much shorter.

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DISCUSSION.

Dr. F. A. Turner: There has been for years and still is a wide discrepancy between the results obtained by clinical, or more correctly epidemiological observations, and by bacteriologists, as to infection by recovered diphtheria patients. Some day, perhaps, the discrepancy may be explained, but at present, in spite of the careful work of Dr. Martin and many others, the conflict of evidence still exists. Fourteen years ago I deliberately gave up cultivating my patients before discharge. The evidence I published at the time, and four or five years afterwards I published a supplementary note in the Metropolitan Asylums Board's Reports (1909, p. 263) showing that a further four years' experience had justified my action. Briefly put, the evidence was that in a series including many thousand cases and extending over all the Metropolitan Asylums Board hospitals for three years, those hospitals which used the culture test as a routine before discharge did not have fewer return cases than the others; in fact they often got more. Not only that, but two hospitals which had introduced routine culture before discharge got worse results after the change than before. And in statistics compiled by other observers I find invariably the same result. It has never yet been found, so far as I am aware, in any long series of cases, that culture before discharge has done any good whatever, that is it has never reduced the number of return

cases. The routine varies at different hospitals: at some one negative culture is deemed sufficient, at others two or three are required. At one hospital at which I have worked, five negative cultures were required, and in spite of all this labour (at the last mentioned hospital I found the man responsible for the diphtheria wards spent more time in the laboratory than in the wards), no good result has been shown; that is, no reduction in the number of return cases. On this evidence I hold it right to discard the bacteriologist when discharging recovered diphtheria patients. Of course I wish to put no ban upon research, but as a practical guide to us clinicians I hold that bacteriology has completely failed. Of course it seems difficult to reconcile this view with the view which I freely concede, that the Klebs-Löffler bacillus is the true cause of diphtheria. But various hypotheses are conceivable. Perhaps most of these late found bacilli are non-virulent. It is not practicable to test this in hundreds or thousands of cases. But where tested on a small number, usually a dozen or so, they have usually been found virulent. Another possibility is that the clinician who calls in the bacteriologist to his aid before discharging a case is a little less stringent than his colleague who does not in his clinical tests of fitness for discharge. I think that this is probably the true explanation, but it would be very difficult to devise a statistical test. I hope the future may bring some complete explanation. But for the present I hold that the facts are in favour of the practical steps that I have taken.

Dr. W. A. Bond: I think one of the causes of the different apparent rates of disappearance of diphtheria bacilli as shown in the three charts of the different countries is due to different climatic conditions in those countries. The effect of different climatic conditions on the rates of disappearance of meningococci from carriers has been shown by Colonel Mervyn Gordon and his colleagues in their report to the Medical Research Committee.[1] Dry warm weather with sunshine apparently has a beneficial influence and cold damp sunless weather the opposite. Climatic conditions would therefore probably also have a similar effect on diphtheria bacilli.

Dr. J. C. G. Ledingham: The analysis presented by Drs. Hartley and Martin interests me greatly for two reasons. It is the first attempt, so far as I know, to give mathematical expression to the rate of disappearance of micro-organisms from the body of the infected host. It is too often forgotten, I think, that the carrier quota represents only what might be called the residuum of the natural process of disappearance and that in most of the acute infective fevers possibly 95 per cent. of persons who recover from the infection get rid of the infecting organisms in a perfectly regular fashion and within fairly well defined periods. For the purposes of this analysis the carrier quota are not considered on the ground that other factors such as enlarged tonsils and adenoids come into play and interfere with the physiological processes of

[1] *See* Medical Research Committee Report No. 3, pp. 63, 68, 72, 76 and 79.

repair. Diphtheria offers perhaps the best conditions for an analysis of this kind as the site of vegetation is a single and readily accessible one and can be investigated freely and frequently. If a similar analysis were possible in typhoid fever, which is doubtful, the carrier with gall-bladder lesion would also not fit in. Secondly the analysis is of interest to me from the fact that, as yet, we have no precise experimental data with regard to the rate of disappearance of organisms from a culture. The mathematics of bacterial growth from the period of seeding to the phase of constant cell population when cell growth and cell death balance, have been fairly thoroughly investigated and there would appear to be little doubt that the terminal phases ending with complète death would proceed similarly to the active growth phases but in the inverse fashion, thus yielding some form of frequency curve for the expression of the whole phenomenon from time of seeding to time of complete cell death. If this mode of disappearance held good in the infected host, the analysis before us would represent simply the middle portion of a sigma curve when cell death proceeds logarithmically in accordance with a monomolecular reaction. Doubtless this phase, like the corresponding logarithmic phase in cell growth, is the most important phase and is also the one most amenable to observation and experiment. At the same time I think it must be regarded only as a partial expression of the phenomenon of disappearance, looking at the matter of tissue infection in the most general way and less from the clinical phenomena associated therewith. It is known for example that when certain organisms such as staphylococci die under the influence of a disinfectant the whole process cannot invariably be expressed as a monomolecular reaction. The monomolecular phase is then preceded by a phase of initial lag. Possibly also the carrier phase might really represent a phase of terminal lag and asymptotic finish of the sigma curve. We are very grateful to the authors for an analysis of this kind as, whatever the interpretation may be, it helps to clarify our notions and to bring clinical and experimental data into consonance.

Dr. MAJOR GREENWOOD: A possible explanation of the discrepancy noticed by the authors between the rate of decline in the proportion of carriers in their experience and in other series is the chance of re-infection which is perhaps greater when carriers are segregated. Thus, a person who at the end of, say, a week is not found to be a carrier but at the end of a second week provides a positive result, may not be a mere illustration of the experimental or sampling error of the method but may be a consequence of re-infection. It would be a matter of great interest if we could assess the importance of these two factors, but I doubt whether the material at present available is adequate for the purpose.

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THE EDITORIAL COMMITTEE

VOLUME THE THIRTEENTH

SESSION 1919-20

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1920

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SECTIONS OF ANÆSTHETICS AND LARYNGOLOGY.

(COMBINED MEETING.)

February 6, 1920.

DISCUSSION ON ANÆSTHESIA IN THROAT AND NOSE OPERATIONS.

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Shown by A. L. Flemming.

Regulating Dropper for the Supply of Ether 1

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Section of Laryngology.

President—Mr. E. B. Waggett, M.B.

Prolonged and Obstinate Functional Aphonia treated by Hypnotism.[1]

By Dan McKenzie, M.D.

The patient is a young woman, aged 25. She first lost her voice in January, 1916, but then only for twenty-four hours. A year later, in January, 1917, she lost it again, and this time it did not return.

In March, 1917, she came to the Central London Throat and Ear Hospital, and was perseveringly submitted to the usual therapeutic measures, but without any benefit.

Suggestion and consistent hopefulness were combined with iron and nux vomica, and with the faradic current, the last of which produced a faint but immediately inhibited scream—the sole vocal sound for about two years. The effect of general anæsthesia by chloroform and ether, assisted by suggestion, was also tried, together with exposure to strong ammonia fumes, as a consequence of which the exhibitor was all but "gassed." The patient, however, remained placid and voiceless through it all.

By practising Dr. Smurthwaite's moral suasion and encouragement we were able to prolong phonation a little longer than the cough effort; but any further extension of the sound was not permitted.

Finally, Dr. Burnett Rae kindly undertook the treatment of the case. She first went to him in October, 1918, and was hypnotized weekly for five months, until Dr. Rae began to fear that some organic lesion underlay the symptom. At the end of May, 1919, however, Dr. Rae had her once more anæsthetized, and succeeded in restoring her voice.

[1] At a meeting of the Section, held November 7, 1919.

DISCUSSION.

Dr. BURNETT RAE: I came here in order to hear Dr. McKenzie's remarks about this case, which interested me very much, mainly because of its difficulty. I have had many of these cases to treat, and I never found them particularly difficult; they usually respond quickly to suggestion, either conscious or hypnotic. This case, as Dr. McKenzie says, was submitted to the usual measures, without benefit. The aphonia had persisted for almost two years when the patient was sent to me for treatment. As suggestion under an anæsthetic had been tried previously and failed, I adopted suggestion under light hypnosis, and persisted with this for five months. She was never deeply hypnotized, but was rather in the hypnoidal state: there was not that absolute suggestibility which accompanies deep hypnosis. That fact may have had something to do with the long and tedious nature of the case. I persisted, however, because Dr. McKenzie was emphatically of the opinion that the trouble was purely functional. It is important, before sending a case to the psychotherapist, to be quite certain about the diagnosis, otherwise it is difficult for him to make the suggestion so positively as is necessary. Suggestion is the art of making a person believe. It is not only necessary for the patient to believe in the doctor, the doctor must believe in the patient — that is, that he has the power of response and recovery—before suggestion can be given as positively as is required. When the patient sees that the doctor is convinced that he can cure him, he begins to believe in himself: hence the value in this case of persistence. But, further, the case shows that the psychotherapist must make a thorough diagnosis from his own point of view, the mental diagnosis being as important as the physical, in order that the proper suggestion which is to ease the mental situation may be given. It is suggestion that cures, of course, not hypnosis. By the process of analysis, by ordinary conversational methods, I was able to elicit the interesting fact that from her early childhood this patient had been troubled by a bad dream. She dreamt that she was in a wood, being chased by a bull, and unable to cry for help; this was a recurring nightmare. She had lost her voice after an encounter with her employer, who was a hectoring "bully," and who, she felt, had treated her unjustly over a business contract. She was, so she said, almost speechless with anger; and when she wakened next morning she could not speak. I told her I thought we had found the clue to her condition, and that if she had the anæsthetic now, she would recover her voice. While she was in the light, suggestible stage of anæsthesia, I reconstructed the scene of her terror. The bull was described as chasing her, and she was told that she could cry for help. Her employer was also brought before her mentally, and it was suggested that she could say exactly what she thought of him. She was then allowed to come out of the anæsthetic, and positive suggestion as to her ability to speak was given to her. Her voice returned completely within a few minutes, and there has been no relapse. Functional aphonia is a psychosis and requires psychical treatment either by the laryngologist or psychologist.

Mr. STUART-LOW: I wish to protest against it going forth from this Society that the only or finally effective treatment in apparently intractable cases of functional aphonia is to resort to hypnotism. I have treated such cases for thirty years, and have had during this period all degrees of stubbornness to deal with, but have never failed to restore the voice by means of ordinary methods properly employed. Great stress should be laid in all cases on improving the general health and surroundings of patients. This girl told me that during the whole course of her treatment—viz., six months—she continued at her ordinary work of fur-making in a very dusty atmosphere and in ill-ventilated rooms. Such work leads to dryness of the nose and throat, especially where enlarged tonsils and adenoids are present as in this instance. It has always been my routine, if the voice is not restored at once with electrical treatment, to put the patient in favourable surroundings, remove the tonsils, adenoids, and all nasal obstructions, and the general anæsthetic necessary may be an assistance towards the restoration. I observe in the notes that it is stated that she was hypnotized twice a week for five months, and that then a general anæsthetic was administered, after which the voice returned. In my opinion the anæsthetic had more to do with the recovery of the voice than all the five months' hypnotism.

Mr. MARK HOVELL: I do not know whether my remark may have a bearing on this particular case, but in neurasthenic aphonia in females it is important to remember that the trouble may be related to uterine derangement.

Intratracheal Tumour removed by Per-oral Tracheoscopy.[1]

By HERBERT TILLEY, F.R.C.S.

M. R., aged 34, was active and robust until "gassed" (mustard gas) on March 21, 1918. He was in bed for fourteen days and then sent home for ten to twelve weeks. He recovered from all unfavourable symptoms except that any hurry in walking or an unusual exertion produced a feeling of difficulty in breathing.

Between June 22, 1918, and June 16, 1919, he was carefully examined by his family doctor on some six occasions and the chief feature elicited by these examinations was the increasing dyspnœa and distress on the least exertion. During the last three months of the period named, paroxysms of coughing accompanied by profuse expectoration, occasionally blood-stained, had been complained of. The only

[1] A more complete history of the case, with pathological details of the tumour, will be published in the special January number of the *Journal of Laryngology*.

abnormal physical signs in the chest were prolonged expiration and feeble breath sounds at root of right lung.

Radiographic examination of chest (Dr. Ironside Bruce): "There is evidence of some infiltration of the mediastinal contents at the level of the manubrium sterni which results in pressure on the trachea and œsophagus, but there is no evidence as to the nature of this infiltration. No evidence of any other lesion of the lung or aorta is available in this case."

Examination: I examined this patient on June 16. The mere exertion of walking into my room caused embarrassed respiration. The larynx was normal in colour and the action of the cords on phonation and respiration was perfect. By getting the patient to assume the Killian position, I was enabled to see the lower portion of the trachea and to ascertain that this region was nearly filled by a pale-reddish growth which moved upwards and downwards with expiration and inspiration.

Operation, June 17: By means of peroral tracheoscopy and using long Paterson's and Moore's forceps, I managed to remove the growth which is exhibited to-day. It appeared to be growing from the junction of the trachea and the right bronchus, and although it was pedunculated it was very difficult to seize the pedicle in forceps. The bleeding was very free, and had it not been for the skill with which Dr. Rood maintained anæsthesia and removed the blood from the lungs by direct air pressure, and the ability with which Dr. Irwin Moore kept my small mirror clear of exhaled blood spray, I do not think the operation would have been satisfactorily concluded. It lasted some fifty minutes. The relief in breathing was immediate, and the patient made a rapid and uneventful recovery.

Note, October 9.—Patient has put on weight, sleeps well, eats well, and seems to be in perfect health. I can see no sign of any recurrence in the lower part of trachea.

Professor Shattock has examined and reported on the specimen. His report will be printed in full in the *Proceedings*; he regards the tumour as in the nature of a papillary granuloma.

I can only suggest that the "mustard gas" set up a local ulceration and perichondritis with formation of excessive granulation tissue which eventually assumed the form of a tumour. Septic absorption in the neighbourhood of such a lesion might explain the physical signs and the radiographic findings.

Microscopical section of the growth will be shown.

DISCUSSION.

Professor SHATTOCK: This case offered an interesting pathological problem for solution. The patient had been gassed with "mustard gas," and the granuloma developed probably at the site of a resulting ulcer in the tracheal mucosa. There was thought to be some mediastinal shadow in the X-ray picture, and the possibility of a mediastinal lymphosarcoma fungating into the trachea had to be mentally dealt with. From the microscopic structure of the "tumour," I concluded that it was a simple granuloma with a papillary surface. For it presented throughout the characteristic structure of a granuloma, the small cell infiltration being throughout conspicuous, and consisting largely of polymorphonuclear leucocytes, and the deeper part (as might be anticipated) being of denser structure than the rest. The patient is at the present time, I understand, perfectly well. The pathological interest, however, is comparatively small compared with such a triumph of laryngological skill.

Dr. DUNDAS GRANT: I suppose such cases are extremely rare. I had one of the kind at Brompton Hospital a year ago. A man was referred to me with a kind of double stridor, not very loud but characteristically tracheal. By means of bronchoscopy I was able to see, to the left side of the carina, something flapping, the size of a pea. I removed it by upper bronchoscopy, and found it transparent, and irregular on the surface. I regarded it as an œdematous fibroma. The man had considerable relief and disappeared, and I have not heard of him since. The only tumour I had removed from the trachea before was a papilloma, but this was apparently almost a continuation of papillomata in the larynx. My specimen was, unfortunately, put into absolute alcohol, which so shrivelled it that it is now hardly perceptible in the bottle. Local anæsthesia was used, with the patient in the sitting posture. The growth was not by any means vascular, so there was no trouble from hæmorrhage.

Dr. JOBSON HORNE: The tumour, to the naked eye, is a papilloma. It is therefore difficult to accept the suggestion by Mr. Tilley "that the 'mustard gas' set up a local ulceration and perichondritis with formation of excessive granulation tissue, which eventually assumed the form of a tumour." A papilloma, in my opinion, was present before the "gassing" took place; it was irritated by the "mustard gas" and underwent inflammatory and degenerative changes. It was a case of aggravation and not of attributability. In my collection I have a specimen illustrating my point, and I should be pleased to exhibit it at the next meeting.

Dr. IRWIN MOORE: This case is in my opinion a very brilliant result to be credited to per-oral tracheoscopy. Formerly these growths were removed by means of a low tracheotomy, and in some cases the patient succumbed owing to hæmorrhage or septic pneumonia. I was much struck with the procedure adopted in this case. The point I wish to emphasize is, that an

operation of this kind should not be attempted, in the case of a very vascular growth in the neighbourhood of the bifurcation, unless means are employed for preventing the blood passing into the lungs. This can now be done by means of a catheter-tube attached to an electric rotatory air pump which gives a continuous blast of air (Kelly's apparatus). The catheter, which is of a size which will not obstruct the lumen of the bronchoscope, is passed down at intervals through the endoscopic tube and beyond the growth. The continuous blast of air drives out any blood which is shed, instead of allowing it to pass down into the lungs. As a result, in this case, the patient made an immediate and uninterrupted recovery from the operation, without, as it is said, "turning a hair," for there was no rise of temperature, nor any other complication, and the patient next day was perfectly well and none the worse for the ordeal. Primary intrinsic growths of the trachea are exceedingly rare, and particularly in the neighbourhood of the bifurcation. I can find only nine cases of benign growths recorded in this situation, and one case only in a bronchus. Of these ten two were papillomata and eight fibromata. Theisen,[1] of New York, published a very interesting paper in 1902; this gives a résumé of all the recorded cases of tracheal growths from 1875 up to the date of his paper, the first case being observed with the laryngoscope in 1861. He found that there were only 135 cases, of which eighty-nine were benign and forty-six malignant. Krieg,[2] in the latest statistics published in 1908, brings the records of published cases of primary neoplasms of the trachea (benign and malignant) up to 201 cases.

Postscript.—The following opinion has been received from Professor Shattock: "The possibility suggested by Dr. Jobson Horne is, one must think, negatived by the fact that there was nothing whatever in the clinical history indicating any tracheal obstruction before the gassing, the patient having been till then a perfectly healthy man, who was able, as Mr. Tilley's history states, to play football."

Instruments used for suturing the Pillars of the Fauces after Enucleation of the Tonsils in Case of Hæmorrhage.[3]

By Sydney Scott, M.S.

(1) The *needle* (fig. 1) has two definite angles in the *same* plane, so that the operator may know the position of the point though hidden momentarily in the pharynx or when embedded in the tonsil bed or faucial pillars. The acute angle (*see* fig. 2) converts the instrument into

[1] *Amer. Journ. Med. Sci.*, 1902, n.s. cxxiii, p. 1051.
[2] *Beitrage zur Klin. Chirurgie*, 1908, xviii, p. 162.
[3] Referred from the previous meeting of this Section on June 6.

a retractor of the posterior pillar after transfixing the pillar from the pharyngeal side. The extremity of the needle is "diamond" shaped, and the edges are sharp only between the extreme point and the side angles, which are slightly blunted to prevent the puncture being converted into a gaping wound. The eye of the needle is nearer the



FIG. 1.

Needle. (Full size.)

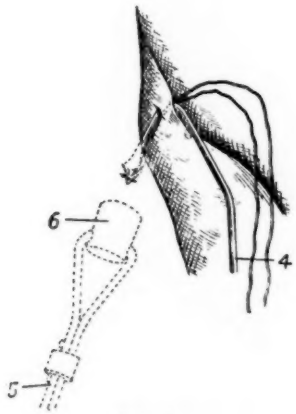


FIG. 2.

Needle armed with catgut penetrating both pillars of the tonsil. 4, stem of needle; 5, cork holder; 6, cork.



FIG. 3.

Cork and cork holder. (Half size.)

point than the transverse diameter, and is just large enough to admit No. 1 catgut when swollen by water.

(2) The *clamp* (fig. 3) to hold a small round cork may be replaced by the tonsil vulsellum. The use of the cork is illustrated in fig. 2 and fig. 4. After removal of the tonsil from its bed, and when it is decided

to suture the pillars together, the needle (fig. 2) armed with catgut is passed from the pharynx through the *upper* part of the posterior pillar, across the tonsil bed, and the point is embedded in the anterior pillar. Owing to the toughness of the anterior pillar, and to the want of

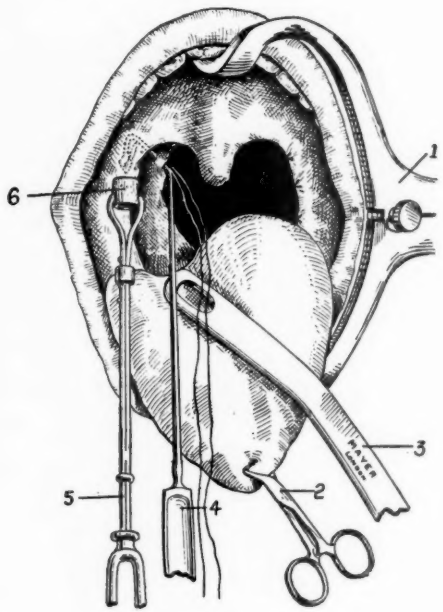


FIG. 4.

To illustrate suture of the tonsil pillars in difficult cases. The patient is in the lateral position in this operation.



FIG. 5.

Compression forceps for vessels in tonsil bed. (Half size.)

rigidity, it is found easier to pierce the anterior pillar, if the cork is gently pressed against the anterior surface of the anterior pillar, over the elevation produced by the point of the needle. The cork is most

useful in pressing the anterior pilllar on to the point of the needle, which is then embedded into the cork. The latter is quickly removed, and the operator immediately seizes the catgut close to the eye of the needle with forceps and draws out a loop from the needle. While retaining hold of this loop of catgut the needle is withdrawn, leaving the catgut ready in position for suture in the usual way. Two, three, or occasionally four sutures are inserted, always beginning above. Three half hitches are safer than only two. Sometimes the sutures are removed within twenty-four hours.

The *compression forceps* (fig. 5) are lighter and longer than ordinary artery forceps, so that the handles are out of the way of the operator's view. They are curved in the flat plane because they are used as lateral retractors to the anterior pillar to bring the vessels into view. The vessels are compressed, antero-posteriorly, as it is easier to pick up the adjacent part of the lining of the tonsil bed antero-posteriorly than vertically. The vessels are then ligatured in the usual way. When there is a tendency for the ligature to slip it is safer to transfix, but with care to avoid too deep penetration.

The Control of Hæmorrhage following Removal of the Tonsils.

By Irwin Moore, M.B.

In 1918, when I published a paper in the *Practitioner* on the subject of "Hæmorrhage after Removal of the Tonsils," Mr. Sydney Scott was away on active service. In this paper I reviewed the treatment of hæmorrhage since the days of Celsus, and I advocated the *early* suturing of the faucial pillars in all cases of bleeding after removal of the tonsils.

The simple needle which I now show was introduced to the profession three years ago, and is fully described in the above article. It is an ordinary curved needle attached to a long handle with the eye at the distal extremity and is at the present time used by many of my colleagues. With this needle is supplied a suture hook for securing the ligature at the needle point, but it has been found that the hook is not really required since any forceps are applicable for this purpose.

The great advantage of this ligature needle over others is that

ligatures may be easily and quickly inserted—only one needle being necessary for either side of the fauces, and a ligature can be inserted either facing the patient when he is in the recumbent position, or when he is in the more recent and safer posture of full extension of the head, the operator standing at the head of the patient.

There is nothing original in ligaturing the faucial pillars, for Professor Luc has advocated this procedure for some years in all



FIG. 1.

Needle for suturing the faucial pillars. The same needle can be used for either side.



FIG. 2.

Ligature hook.

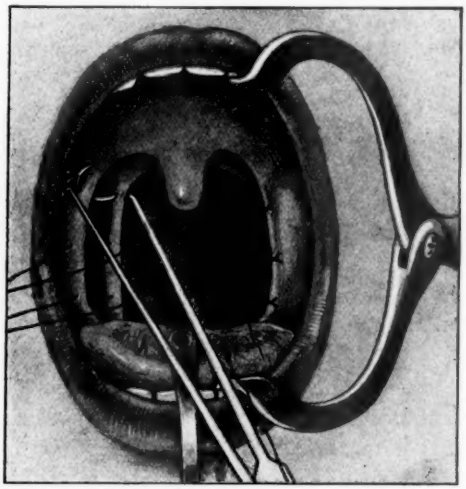
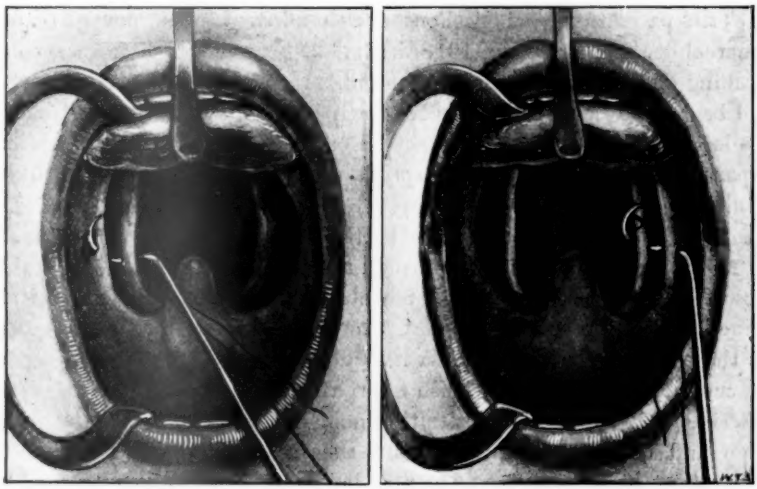


FIG. 3.

The arrest of hæmorrhage after removal of the tonsils by temporary suturing of the faucial pillars. Shows the needle (threaded with a catgut suture) passed through the faucial pillars, also the method of securing and drawing the suture through the needle by means of a long hook, when the patient is in the dorsal recumbent posture and the operator on the right hand side of the patient.

cases of tonsillectomy in adults, and during recent years I have followed his advice, and have been in the habit of temporarily ligaturing the pillars in all adult cases as a prevention of post-operative hæmorrhage.

Drawings were exhibited on the epidiascope showing how easily ligatures could be inserted through the faucial pillars.



L. *R.* *L.* *R.*

FIG. 4.

Shows how the same needle may be used when the patient is in the dorsal recumbent posture with over-extension of the head, and the operator standing at the head of the patient.

Dr. FRIEL: To introduce sutures I use a threaded small perineal needle with a handle. When the eye has perforated the tissues, the silk or catgut is grasped at the eye and drawn as a loop out of the mouth, while the needle retraces the path it travelled and is removed. Outside the mouth are the loop and the two loose ends. One of the latter is passed through the loop and then by traction on the loose ends the loop is drawn into the mouth. As the loop cannot be removed because of the loose end which was passed through it, traction on the loose ends draws together the pillars and the floor of the space between the pillars and compresses them. A half reef knot is made by the two loose ends. *The grip of the strand by the loop* in addition to the half reef knot prevents any slackening as an additional half reef is made.

Case of Epithelioma of Tongue, with large Secondary Deposits in Cervical and Sub-Lingual Glands, Treated with Aminopropionate of Copper, in a Man aged 65.

By James Donelan, M.B.

This patient enjoyed excellent health all his life and never had any venereal disease. In May, 1918, he had some annoyance from a small swelling on the left side of his tongue. In a few weeks it grew larger and began to bleed, then it broke down into an ulcer and the glands in the left side of his neck enlarged. He attended the general out-patient department of the Italian Hospital, and was advised to submit to immediate operation. He, however, did nothing further until December 24, 1918, when he went to the Throat Hospital, and was seen by Mr. George Dawson, who regarded the case as cancer and inoperable. Towards the end of May, 1919, more than a year after his first visit, he came back to the Italian Hospital, where he was seen for the first time by the exhibitor. He was then a man obviously suffering from advanced cancer: pale, cachectic and with a large deeply excavated ulcer of the left side of his tongue opposite the molar teeth. It was about an inch deep and extended slightly beyond the middle line, while a zone of infiltration half an inch wide extended beyond and around the ulcer. The whole tongue was immensely swollen and the patient could scarcely articulate or swallow. There was a large ulcer on his chin and left side of lower lip, apparently due to the foul discharge from the ulcer. The left cervical, sublingual and submental glands were enormously enlarged and painful, forming apparently a homogeneous mass extending from the middle line below the chin to the ear. The skin was tense and red and appeared about to break down below the angle of the jaw. The diagnosis of lingual cancer with extensive secondary deposits was made from the clinical appearances, and was subsequently confirmed by microscopic examination. It was obviously inoperable. It was considered a very suitable case for the trial of the aminopropionate or alanin of copper as suggested by Dr. Shaw-Mackenzie.

Before treatment was commenced the case was put on the agenda of this Section for the meeting of June 6. The patient, however, had severe hæmorrhage on that day and was unable to leave the hospital. On the following Wednesday, June 11 1919, an intravenous injection

of 1 c.c. of a 1 per cent. solution of copper alanin was given. On the Wednesday following there was a remarkable improvement in the general condition. The pallor and cachexia disappeared and patient became rosy and healthy in appearance. The injections were continued once a week, and after the third injection the ulcers on the lip and tongue began to disappear. A remarkable diminution of the glands also took place.

The improvement was progressive up to the beginning of September when the supply of the solution ran short and the doses had to be much diminished. There was no apparent change in the area already infected but the sublingual gland immediately beneath the tongue on the left side became enlarged, and to such an extent as to press his tongue up against the roof of his mouth and again render articulation and deglutition almost impossible. The exhibitor succeeded in obtaining a further supply of the salt and the injections were given twice weekly. The same effect in diminishing the swelling took place and continues (October 10).

A section prepared on this date shows that histologically the epitheliomatous structure is still present unchanged. There can be little doubt, however, from this experience that copper alanin has a marked effect in reducing the inflammatory zone that surrounds the cancerous tissue. Whether it has any effect on the cancerous tissue itself remains to be seen. From the improvement in the ulcer and diminution in the size of the glands, as well as from the great improvement in the patient's comfort and general condition, it would appear to be well worth further trial.

A microscopical section from this case is shown.

DISCUSSION.

Mr. C. W. HOPE: During the years 1912-14 I used seleniol, a colloidal preparation of selenium, and several of my cases did extremely well. The growth was not cured, but its extension was definitely limited.

Dr. WILLIAM HILL: Ought we not, in the light of this case and in that of other cancer cases reported here, always to use these copper compounds as adjuncts? I have come across cases in which practitioners have assured me that in some instances there has been most remarkable improvement, and I wonder whether I am justified in going on with any form of treatment, palliative or otherwise, without employing these colloidals and other copper compounds in addition. I use them in many of my malignant cases, but always in conjunction with radium or other methods, and therefore I cannot bring convincing evidence of their specific value.

Dr. IRWIN MOORE: I have used injections of cuprase (colloidal copper) in a number of cases, and I can confirm Dr. Donelan's experience, for I have observed not only a remarkable improvement in the general condition of patients but also a definite reduction in the size of malignant glands. In the majority of my cases the primary growth has been internal, so that the effect on the main growth could not be ascertained. In one case of carcinoma of the nasopharynx these injections were used after removal of the growth, and the patient has had no recurrence for three years. This case was shown at meetings of this Section on November 3, 1916,[1] and June 1, 1917.[2]

Mr. G. DAWSON: When I saw this patient he had a large hard ulcerated tumour on the left side of the tongue, and a big firm mass under the angle of the jaw. He was very ill, and appeared to be beyond operation. I recommended him to go to a general hospital to see if the tongue could be excised. He found his way to Dr. Donelan, who gave him this copper compound, and there was the greatest improvement: the condition of the glands and of the tongue are enormously improved, though, of course, the cancer is still there. Dr. Donelan gave me some of the remedy, and I have tried it in another case, and there, too, improvement has resulted.

Dr. DONELAN (in reply): I have brought this case forward because the opinion has been expressed that the benefit from the use of copper salts has been, for the most part, where the cancer could not be seen, as in abdominal or uterine cases. I was fortunate in meeting this case in which the clinical and microscopic evidence is indisputable. Although the cancerous structure still remains the injections appear to have had the power of checking the advance and of giving much comfort to the patient. For this reason I think it is only right that others should know of it. With regard to cuprase, I have tried that in other cases as well as the ammonic-sulphate of copper, but the administration by subcutaneous and intramuscular injection was attended with so much pain that it had to be given up. Dr. Shaw-Mackenzie, whose long researches on the subject of cancer are well known, suggested combining the copper with an amino-acid, in this case aminopropionic acid or alanin. Alanin was not previously made in England save in teaching institutions. After trying nearly every firm of repute without success it was at length made commercially, through the influence of a member of the Munitions Board, with a well known firm. [Dr. Donelan gave particulars whence supplies could be procured advising that no statement of its purpose should be made by applicants.] It is given by intravenous injection of a solution made either with sterile saline or distilled water, which is estimated at 0·44 per cent. at ordinary temperatures; 1 c.c. is the usual dose from one to three times a week. Mr. Aslett Baldwin has been giving 2 c.c. in some cases with good results.

[1] *Proc. Roy. Soc. Med.*, 1917, x (Sect. Laryng.), p. 32.

[2] Op. cit., 1918, xi, p. 9.

Large Antral Polypus with Two Distinct Pedicles.[1]

By JAMES DONELAN, M.B.

THIS specimen was previously shown at a meeting of the Section on April 4, 1919,[2] and gave rise to considerable interest on account of its having two pedicles. It was a question whether this was a case of fusion of two polypi from inflammation or other causes, and it was suggested that the specimen should be referred for a report.

The following report has been kindly supplied by Professor S. G. Shattock, F.R.S.: "A polypus, bisected, which was removed from the nasopharynx of a male, aged 22. The growth is of flattened triangular form, about 3 cm. (1¼ in.) along its sides. It is provided with two distinct pedicles, the longer of which has been partially torn longitudinally so as to give an appearance of having been double. Microscopically it presents a uniform structure and consists of a meshwork of wavy fibrils, amongst which are distributed a few cells of the lamellar, connective-tissue kind. The vessels are small and scanty."

Statistics bearing upon the Sex and Age-incidence of Carcinoma of the Post-cricoid Area, with Notes on the Duration of Symptoms in these Cases.

By A. LOGAN TURNER.

Demonstrated by means of a Series of Diagrams.

[*Postscript.*—This communication was considered by members present to be of such great importance that in view of the number of cases there would be insufficient time for discussion. Dr. Logan Turner agreed to bring these statistics forward again at the Summer Congress in May, 1920.]

[1] The specimen is now in the Museum of the Royal College of Surgeons.

[2] For report of case and discussion see *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.) p. 167.

A Case of Latent Double Frontal Sinus Suppuration with Expansion of the Sinuses, following a through-and-through Bullet Wound received Three Years previously.

By W. M. Mollison, M.Ch.

Captain F., aged 22, was wounded on July 23, 1916; a bullet fired at short range entered just behind the right external angular process and passed out on the left side over the upper and posterior part of the malar bones; the right eye was destroyed and the left had to be excised subsequently. The wounds healed in three weeks, and he made a good recovery. He worked in a responsible position at St. Dunstan's for two years. He never had any nasal symptoms.

He was sent to see me in August, 1919, by Mr. Ormond, on account of swelling in the roof of either orbit. The first evidence of anything wrong had been inability to lift the upper eyelids as well as formerly, and in July, 1919, Mr. Lawson saw him and noted some "thickening in the upper lids"; also, for some months (? six) he had thought his forehead was becoming more prominent; this had also been noticed by a relation who saw him a year before. A photograph taken in 1916 confirms that impression.

Examination: The supra-orbital region was well marked, the skin over it normal, no trace of tenderness on pressure. In the roof of either orbit could be felt a fluctuating swelling larger on the left than the right; neither was tender. Nasal examination showed nothing suggestive of inflammation or pus.

An X-ray examination by Dr. Ironside Bruce showed the frontal sinuses to be large but of equal opacity: a lateral view showed very marked prominence of the forehead; the bone bulged forward.

As the cause of the swelling was doubtful, aspiration of the swelling through the left eye-socket was undertaken. With some difficulty thick tenacious pus was drawn into a syringe; bacteriological examination by Dr. Eyre revealed a pure culture of pneumococci or streptococci. The diagnosis of frontal sinus suppuration distending the sinuses was made and external operation avoided.

September 4, 1919: Operation. Dr. Trewby kindly gave an anæsthetic. The usual incisions were made below the eyebrows: when the bone was exposed it was found to be of such thickness that it could easily be

pressed in by the finger, the sinuses were full of tenacious pus, there was a small communication between the sinuses, and the floor on both sides was deficient. After removing the floor and most of the anterior wall a passage had to be made into the nose on either side as no trace of a fronto-nasal duct was to be found. Rubber tubes were left in the artificially-made fronto-nasal ducts. Subsequently further operation to enlarge the fronto-nasal ducts was needed.

DISCUSSION.

Mr. HOWARTH: I have operated on about 200 cases at St. Dunstan's in the last five years who have lost one or both eyes and I think that Mr. Mollison's case is a good example of a type of case that one meets not infrequently. All cases in which an eye is lost are classed as "eye cases" by an omniscient War Office, when that unfortunately is the last thing that they are. Surgically speaking, they are compound fractures of the bony orbit with involvement of the nasal accessory sinuses in a large number of cases. Some of the cases that I have seen have come under my observation for months or even years after the original wound. The cases may be divided into three groups. First, those which have an open track leading to the affected sinus, or discharging on to the face or into the eye socket; secondly, those which discharge into the nose or nasopharynx; thirdly, the latent cases. I will pass over the first two groups and pass on to the latent group. I have seen more than a dozen of these. Apparently they give rise to no symptoms and may remain unsuspected even after thorough rhinological examination until some acute infection lights them up. What is the explanation of these cases? I think there certainly may be two explanations. One is that the bullet passing across the orbit and root of the nose produces a comminuted fracture of the bony wall of the frontal sinus. One fragment wholly or partially deprived of its blood supply may undergo slow necrosis and cause the suppuration, whilst at the same time the fronto-nasal duct may be partially blocked by scar tissue. Three times when opening one of these sinuses I found a large sequestrum in the middle of the abscess. Another explanation, in my opinion, is, that the passage of the bullet across the root of the nose may completely block up the ostium, so all the conditions for the production of a mucocele or pyocele are present. I think that has happened in Mr. Mollison's case and that the mucocele became infected recently and flared up. Points in favour of this are the fact that he found the bony wall of the sinus much expanded, the contents very thick, and no evidence of any opening into the nose. My experience is that these gunshot cases are often not cured by one operation.

Mr. MARK HOVELL: With regard to treatment, in September last a man was sent to me by a physician who was treating him for cardiac dilatation. He had left frontal sinus suppuration of old standing and there was a

considerable amount of pus in the left nostril. I advised spraying the nostril with collosol argentum and gave 2 c.c. subcutaneous injections of the same drug once a week for three weeks and then twice a week. The purulent discharge disappeared after the fourth injection. When seen by the physician after the seventh injection the cardiac dullness was reduced from 6 in. to 5¼ in. I have used subcutaneous injections of collosol argentum with good result in cases of chronic suppuration of the middle ear and intend to use it with daily doses in the early stages of acute cases.

Mr. E. D. D. DAVIS: The whole difficulty is one of drainage into the nose, and good drainage into the nose is very difficult to obtain and unsatisfactory when the fronto-nasal duct and ethmoid are obliterated by scar tissue.

Mr. MOLLISON (in reply): I also have seen a certain number of St. Dunstan's men, but none with completely latent suppuration like this one. Such cases may throw light on the so-called mucoceles of the frontal sinus; they may not be mucoceles at all, but suppuration due to organisms with a low form of infectivity. In this case there was absolute blockage of the fronto-nasal ducts.

Cases exhibited by Dr. Irwin Moore, Dr. William Hill, Mr. Colledge, Sir StClair Thomson, Mr. Tawse, Mr. Mollison and Mr. Dawson had to be postponed to the next meeting for want of time.

Section of Laryngology.

President—Mr. E. B. Waggett, D.S.O., M.B.

Case of Fibroma of the Aryepiglottic Fold removed with the Aid of Suspension Laryngoscopy.[1]

By W. M. Mollison, M.Ch.

Fred P., aged 40, presented himself in the ear and throat out-patient department at Guy's Hospital on August 14, 1919, complaining of intermittent pain in the left ear, of two months' duration. The pain was found to be due to meatitis. Routine examination of the larynx revealed a large globular swelling in the left aryepiglottic fold; the swelling obstructed the view of the vocal cords: it was smooth and the mucous membrane over it somewhat paler than normal. The patient's voice was a little "thick," but he himself had noticed nothing wrong with it: he had, however, had a few attacks of "a sensation of suffocation."

Operation was performed on September 26; morphia, $\frac{1}{4}$ gr., and atropine, $\frac{1}{100}$ gr., were given hypodermically, and under general anæsthesia the suspension laryngoscopic apparatus was adjusted. An incision was made over the swelling and after some difficulty a smooth tumour was dissected out. At the end of the operation the patient's respirations were rapid and shallow and he was somewhat cyanosed: this may have been due to the considerable traction exerted on the epiglottis and arytænoid in the efforts at removal of the growth. The day following operation the temperature rose to 101° F., and for five or six days remained up: at the same time there was pain in the right side of the chest, and he was found to have a patch of pleurisy (?). He was well in a week after operation.

[1] At a meeting of the Section, held December 5, 1919.

The tumour is shown, and is about the size of a pigeon's egg, it has a smooth white capsule. On section it presented a fibrous appearance with areas of degeneration. Dr. Nicholson reports that microscopically it is a hyaline necrotic fibroma.

Fibromata of the aryepiglottic folds appear to be rare. The cases quoted in "Allbutt's Medicine" (1909 edition) are those in which dissection was performed from without, and the fibroma appears to have occurred as several small masses attached to one another by strands.



Fibroma of the aryepiglottic fold laid open.

Enlarged and diseased Tonsils of Cryptic Type, destroyed by Applications of Escharotic Paste (London Paste).

By Irwin Moore, M.Ch.

Patient, a female aged 22, a pupil at the Guildhall School of Music, consulted me, on the advice of her singing master, on July 8, 1919. She had suffered for years from enlarged tonsils, which at times became more swollen and inflamed and prevented her making progress with her singing.

On examination, both tonsils were found to be considerably enlarged, protruded well beyond the faucial pillars, and exhibited larger open crypts, from which masses of caseated material could be squeezed. On these grounds the exhibitor advised enucleation of both tonsils, but patient refused.

Treatment by application of the London paste was therefore carried out, viz., on July 8, 15, 22, September 30 and October 21. During August, between the third and fourth applications, patient was away at Eastbourne, during which time a remarkable shrinkage of both tonsils took place without any treatment.

This case is shown to demonstrate the efficacy of this escharotic paste treatment in the reduction and destruction of the enlarged tonsil—

previously brought to the notice of members of this Section in a paper read by the exhibitor at the Summer Congress of the Section of Laryngology, May 3, 1919.[1] It shows its advantages over the galvano-cautery puncture in not causing occlusion of the crypts. It also shows that it is not necessary in every case to destroy the tonsil piecemeal, since a retrograde process is set up—following the application of the paste—which causes a general shrinkage of the tonsillar mass without the formation of cicatricial tissue.

DISCUSSION.

Mr. H. J. BANKS-DAVIS: I think this is an important case. The tonsils have not disappeared, but they are much reduced. Is the paste very painful when applied, and does cocaine have to be used? The exhibitor says in the notes, "It shows its advantages over the galvano-cautery puncture in not causing occlusion of the crypts"; but I think that is precisely what one wishes to do—occlude the crypts. I think we should not let it be thought, in view of this particular case, that applications should be made to tonsils as a routine. I have seen many children whose tonsils have been painted for years and years, and they are really "pickled" and hard, and difficult to remove when the time comes. I do not consider it a surgical procedure, though it may be useful in cases in which operation is refused. Acid nitrate of mercury will do it much quicker, and only two applications are required: its corrosive action is not always limited to the seat of the disease, and great care must be taken in its employment.

Mr. W. STUART-LOW: I noticed on carefully examining the throat that there is still a mass of tonsil remaining, and that the crypts are quite patent and filled with accumulated septic material, which must therefore still be swallowed much to the detriment of the patient and of her digestion and of the quality of her blood—she is very anæmic. I do not approve of this line of treatment at all, and do not think that it ought to be entertained or encouraged. Operation is so effective and safe in skilled hands that substitutes for it such as the resort to painting can only temporize, as surgical treatment will be necessary in the end.

Dr. W. HILL: The class of case this treatment was meant for was discussed at our Summer Congress. It is obvious that caustic reduction of tonsils should not include occlusion of the crypts. For singers who will not consent to surgery this method has a legitimate field of application.

Dr. DOUGLAS GUTHRIE: I have used the paste in one case only, and the pain was considerable despite the fact that 20 per cent. cocaine solution had been thoroughly applied.

[1] "The Treatment of Enlarged and Diseased Tonsils in Cases where Surgical Procedures are contra-indicated," *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.), p. 243.

Dr. WALKER DOWNIE (Glasgow): I have no doubt members will recollect that London paste was introduced by Sir Morell Mackenzie. Its application causes considerable pain, and the paste is apt to spread to the faucial pillars and produce ulceration. For those reasons I speak against its use. It is composed of caustic soda and unslaked lime. It is a powder, should be kept in a stoppered bottle, and is mixed with water when about to be used.

Dr. IRWIN MOORE (in reply): The treatment of this case is not yet completed. I have only shown the patient in order that members might note progress. This lady refused operation, and, rather than allow her to go untreated, I have given her four applications of the paste. The right tonsil is finished, but not the left. The composition of the paste is detailed in the *Journal of Laryngology*.[1] Cocaine in 5 per cent. solution is sufficient to deaden any pain which might be complained of during the application of the escharotic, or this can be quelled at once by a gargle of vinegar and water. To use the galvano-cautery successfully one must be sure of penetrating the tissue between the crypts. If the cautery point is inserted into the superficial portion of the crypts and not deeply in and down to the capsule, inflammatory swelling and obliteration of the opening of the crypt may occur and secretions subsequently become pent up, and the patient's condition may be worse than before. I have used this paste in about fifty cases, and I have not known one instance of sealing up of the crypts. I do not put this treatment forward to replace surgical removal of the tonsils, but only for certain cases where operation is inadvisable. Many practitioners maintain we are over-operating on tonsils, and that we have no effective substitute. We now have a most satisfactory substitute, and I only ask members to try it in suitable cases, and I am sure they will not be disappointed. Recently I had sent to me a child, aged 8, with well-marked congenital heart disease and enlarged tonsils. Anæsthetists have refused to give a general anæsthetic, and throat specialists have refused to deal with the tonsils. The least nervous shock to the child would be serious. I have undertaken to treat the child's tonsils by this method.

Multiple Polypi (Œdematous Fibromata) of the Middle Third of the Œsophagus.

By WILLIAM HILL, M.D.

MAN, aged 26. Admitted to hospital in May for dysphagia. X-ray showed fusiform narrowing in middle third of gullet. Œsophagoscopic examination under general anæsthetic showed stenosis due to large swelling of right wall of gullet—it had the appearance of either an

[1] *Journ. Laryngol., Rhinol. and Otol.*, 1919, xxxiv, p. 392.

intramural or peri-œsophageal neoplasm. A portion removed for microscopy showed only normal coats of gullet and inflammatory tissue, but as pain was present later it was assumed to be a case of mediastinal growth, probably sarcoma.

Applications of emanation equivalent of 100 mgr. of radium bromide for twenty hours made at beginning of June and middle of July. There was no improvement in swallowing and pain distinctly increased after last application of radium: progressive loss of weight and strength. Further endoscopic examination in middle of September showed a very altered local condition. The original neoplastic swelling was larger, and below it were three large polypi, red, fleshy-looking, and some smaller gelatinous-looking mucous polypi. Masses have been removed on two occasions under a general anæsthetic with a special snare and with scissors and forceps, but the growth progresses, and the pain in the back is now continuous, and the relief to dysphagia was only temporary. The growths in microscopic structure resemble the ordinary nasal mucous polypi apart from the covering epithelium.

Advice as to more radical treatment is invited — is diathermic destruction feasible? Skiagrams and sketches in colour of endoscopic appearances are also exhibited.

DISCUSSION.

Mr. E. D. D. DAVIS: Why not have a gastrostomy done? It is not a dangerous operation if carried out early and the man is in good condition. The treatment could be continued after gastrostomy. I have seen cases of malignant disease, in which the œdema of the mucous membrane above the growth is so considerable that the value of œsophagoscopy is much diminished.

Dr. DAN MCKENZIE: I have had a case of what looked like mucous polypi growing out from the posterior surface of the soft palate, and anywhere where there is œdema of the mucosa there may be a polypoid condition, and so one would not attach much importance to the naked-eye appearance of growth. My case was called endothelioma: it was very malignant, and the patient died.

Dr. IRWIN MOORE: Benign growths of the œsophagus are very rare. The majority of the cases recorded may be found in Morell Mackenzie's "Diseases of the Throat and Nose," in which there is a short history of twenty-four cases. One interesting point in the present case is the position of the growth in the gullet—i.e., in the middle third, for in most of the reported cases the growths have been in the upper third, which is the most common site. In our Summer Congress Museum two interesting

specimens of fibroma of the œsophagus were exhibited: (1) Specimen No. 1857C, from the Museum of St. Bartholomew's Hospital. Portion of an œsophagus, containing a small fibroma embedded in the muscular coat. It was found post mortem in the body of a man, aged 42, who died of chronic interstitial nephritis. (2) Specimen No. 1016F, from the Museum of Charing Cross Hospital. Discovered unexpectedly at post-mortem on a woman, aged 68, who died of a strangulated umbilical hernia. The microscope showed that it consisted of dense fibrous tissue.

Sir STCLAIR THOMSON: I would like to elicit opinions on the point raised by Mr. E. D. D. Davis as to the value of gastrostomy in such cases. Like him, I used to recommend that operation, always trying to send such cases early, but I have not always found the result satisfactory. Many died of shock and collapse within a short time of the operation; those who got through it successfully complained of leaking and the consequent discomfort so much that now I rejoice when I can recommend Dr. Hill's feeding tube, which, except for social inconvenience, is very helpful.

Mr. MUSGRAVE WOODMAN: I have performed gastrostomy in a large number of these cases, and my opinion is that, if it is left too late it is quite useless. Amongst general surgeons the view is that it should not be performed until the patient is unable to swallow anything by the throat. I hold the reverse view, namely, that the moment one has seen the carcinoma and obtained a portion for verification the operation should be recommended. After the operation patients are restricted to liquid food, but if the right technique be followed and the valvular operation through the rectus muscle be done, there is neither leakage nor regurgitation, and so the skin is not excoriated. One man who came to me to have the operation done has now gone back to his work as a gamekeeper, and he is fit and well six months after the operation. There should not be any waiting until the patient is cachectic.

Mr. COLLEDGE: I collected the records of all the cases in which gastrostomy was done in St. George's Hospital during a period of ten years, and only 47 per cent. were sufficiehtly improved to leave the hospital alive. Therefore, I take it, the majority were done when the patient was already too emaciated to derive any benefit.

Mr. E. D. D. DAVIS (in further comment): I endorse what Mr. Woodman has said, that gastrostomy should be done early and that Witzel's gastrostomy with the valvular procedure is quite satisfactory. Gastrostomy is the most efficient and comfortable way of feeding the patient and it should be done, rather than allow him to die of starvation. Symonds's and Hill's tubes become choked, they are vomited or coughed out, difficult to replace, reflex salivation causes great discomfort and patients beg to have them removed. Sooner or later gastrostomy has to be performed.

Mr. HOPE: I had a man who wore a Hill's tube for six months: it was taken out to give the patient a rest every few weeks and then put back. Then he became emaciated and died in two days. At King's we had six cases, in three weeks, of carcinoma of the œsophagus, ranging in age from 19 years to elderly patients. We sent them direct to a surgeon of repute for gastrostomy, and all, except one, were dead within a week of the operation. I do not feel that even to-day the operation is without danger.

Dr. IRWIN MOORE: I do not think Mr. E. D. D. Davis can have had sufficient experience of Dr. Hill's oro-œsophageal feeding tubes. Some of you may remember a commercial traveller from Plymouth suffering from malignant disease of the œsophagus, whose case I brought to the notice of this Section in 1915,[1] to illustrate the benefits of these feeding tubes. When he arrived in London he was in great distress and could only swallow a little liquid food. He had lost 37 lb. in weight, and was practically ready for gastrostomy. By the use of a Hill's feeding tube I was able to keep him alive and comfortable for eight months, during which time he was able to carry on his business, travelling over Cornwall and Devon with the tube *in situ*. He could take 9 pints of food in twenty-four hours, consisting of beef-tea, milk, custard, Benger's food and lightly boiled eggs. He was so comfortable that he would not come to London to have a renewal of the tube, until finally the rubber became macerated and destroyed and had to be removed. After a further delay he came to town, but the stricture had contracted, and a fresh tube could not be inserted. He returned to Plymouth and had a gastrostomy done, but died in a fortnight. The feature of this case was that the patient put on considerable weight, 6 lb. in the first seven weeks, though he had previously lost 37 lb. Later he almost recovered his former weight.

Dr. W. HILL (in reply): I gather Dr. McKenzie thinks the papillomata in his case and the polypi in mine may be of the nature of an epi-phenomenon on the essential lesion, which may be a sarcoma in the mediastinum. The idea had occurred to me, because when I applied radium there was no suggestion of polypus: there was only this ovoid tumour, half of which was embedded. Only after radium had been twice applied, at an interval of six weeks, did we find polypus, six months later. And I think it unlikely I should have missed even a small polypus at my earlier examinations. Possibly the radium determined the appearance of polypi. I do not understand the meaning of this case, and I have shown it because it is a very rare one. I believe only four previous cases have been recorded as verified by post-mortem examination. Gastrostomy is the last thing I would recommend in these cases: I do not want to give a patient a 33 per cent. chance of dying in a week, nor put him to the inconvenience of laparatomic feeding when there are far safer means of relieving the dysphagia. We are getting better results by using Symonds's funnels and my tubes. Mr. Woodman says he flies first to gastrostomy, but

[1] *Proc. Roy. Soc. Med.*, 1915, viii (Sect. Laryngol.), p. 8; also op. cit., 1916, p. 17.

with us who are more experienced it is the last resort. It is very rare for dysphagia not to be relieved by methods other than gastrostomy, though the latter may be necessary for very tight and painful growths, and when the patient cannot bear an apparatus: also for œsophago-gastric growths. I believe Mr. Davis must have had a run of luck, and I do not think his will turn out to be the average experience. My patient is losing flesh, but not because he cannot take plenty of nourishment. He is losing flesh because (1) he is in pain, (2) possibly because the disease is spreading in the mediastinum, and (3) possibly because the thoracic duct is involved. Glandular disease also may press on the thoracic duct. I have had many cancer cases in which the patients swallow well after radium and yet rapidly lose flesh.

Fish-bone which perforated the Inferior Constrictor and caused Fatal Posterior Mediastinal Abscess.

By William Hill, M.D.

Woman, aged 38, was admitted to hospital with history of fish-bone impaction in throat three weeks previously, suffering from aphagia and some respiratory obstruction, and presenting œdema of right side of neck, with great inflammatory swelling of fauces and pharynx. Temperature 105° F. and pulse 120, and history of rigors. Laryngotomy had to be performed during administration of anæsthetic. On passing the endoscope through the fauces a peripharyngeal abscess burst on the right lateral wall of the pharynx behind the posterior faucial pillar, and many ounces of fœtid pus evacuated. On passing the endoscope through the deep pharynx into the cervical œsophagus it was seen that the posterior wall of the pharynx and of the gullet were tumefied and bulging forward. The posterior mediastinal collection of pus was emptied by pressure with the endoscope and by passing large bougies.

The patient was able to swallow on recovering from the anæsthetic, but septicæmic symptoms, including rigors, continued, and she died from heart failure thirty-six hours after operation.

Post mortem, in addition to the rent in the lateral wall behind the posterior pillar where the abscess burst, there was found a small perforation in the posterior wall of the deep pharynx through the inferior constrictor muscle near its lower border; and on the mucosa covering the cricoid plate near its upper border was a small ulcer. The abscess cavity extended from the base of the skull to the level of bifurcation of the trachea, forming a large post-pharyngeal and post-œsophageal—i.e., posterior mediastinal abscess.

The fish-bone, ¾ in. long, which was found at the bottom of the mediastinal part of the abscess, had evidently been impacted in a slanting direction from before backwards, one point against the cricoid high up and the other point perforating the inferior constrictor as the result of swallowing movements, and perhaps hastened by blind bougieing by a panel doctor.

DISCUSSION.

Mr. HOPE: I had a very similar case a fortnight ago. A woman had swallowed a beef-bone which was retained in the post-cricoidal pharynx six days; she came into hospital with marked dysphagia and dyspnœa. I examined with the œsophagoscope, and found a large swelling bulging forward the posterior wall of the trachea. There was tumefaction in the post-cricoidal region, with food lying about; and with forceps I opened up an abscess which contained 1½ oz. of very fœtid pus: it was in the anterior wall of the œsophagus. I never got the bone out: she may have swallowed it. The condition cleared up in four or five days.

Mr. LAWSON WHALE: I think mediastinitis is the most interesting condition we have to consider in our specialty. I had a case from which there was an additional moral to be drawn; one of a series of four cases of foreign body occurring within one month. This case was the easiest of the four to deal with, and the only one that died. The skiagram showed a halfpenny behind the cricoid. Before trying removal, I had another skiagram taken, and it showed the coin below the manubrium. With tube and forceps I removed it in a few seconds. The boy was found sitting up in bed and playing with his toys, but that of course was stopped. He went out on the seventh day, but forty-eight hours later the mother brought him back as he was in obvious distress. She would not leave the child at hospital until my arrival, but insisted on at once taking it home, where it died a few hours afterwards. So I did not see the child. I could not attend the post-mortem, and through a mistake the œsophagus was not kept. There was a perforation where the coin had lodged near the cricoid, and a large mediastinal abscess led down to and involved the thyroid gland. Even if the foreign body slipped beyond the cricoid, it does not follow that it did not cause perforation when there. If I had not known the history I should have blamed myself for having ruptured the pharynx.

Dr. DOUGLAS GUTHRIE: Has Dr. Hill ever removed, during life, and before the formation of abscess, a fish-bone which had entirely passed through the mucous membrane of the pharynx? Such a case might present great difficulty in diagnosis and treatment. I have here a fish bone, 2 in. in length, which had penetrated the posterior pharyngeal wall and had disappeared from view almost completely (specimen shown). It was easy to

remove, because its enlarged end still projected; except for that it might have passed into the surrounding tissues.

Mr. SOMERVILLE HASTINGS: In April, 1918, I had a similar case. A lady was eating mutton, when a triangular piece of bone was swallowed. The passage was cleared by passing a probang and full-size bougies down the œsophagus. On the fourth day the patient was sent to me with swelling and stiffness of the neck, a temperature of 102° F., a pulse of 120, clearly very ill. While waiting for the skiagram, I passed the œsophagoscope, and 1½ in. below the larynx I saw a transverse slit in the posterior wall of the œsophagus, from which oozed foul-smelling pus. I mopped this away, and at length felt and then saw a piece of hard white bone in the mediastinum. I extracted it with forceps, and the patient did well.

Mr. ROSE: Could not Dr. Hill have opened and drained this abscess by an external operation?

Dr. W. HILL (in reply): I had another case, in which the patient came with a swelling in the neck and a post-pharyngeal abscess, but this abscess was only evident on her second visit. She said she had swallowed a fish-bone and got pain in the back. When I found the abscess there was œdema of the neck, and when there is pharyngeal abscess with œdema one should suspect mediastinal abscess as well. In the other case I did not get hold of the bone, as it went down to the level of the bifurcation. The patient went away still complaining of the pain, but we never got the bone. I had not the courage to explore the cavity with the endoscope tube, because when I tried it I found the patient had a stiff back. In answer to Mr. Rose, I suggested to a surgeon that the right thing would be to open through an incision in the neck and search, but I could not get anyone in the hospital to agree with me. The books give cases in which abscesses have been successfully evacuated through an incision in the supraclavicular region. In the case I have recorded the patient was moribund when seen, and died of septicæmia in a few hours.

Supernumerary Nostril and Cavity.

By H. BELL TAWSE, F.R.C.S.

FEMALE child, aged 16 months—one of twins; the other was anencephalous and died at birth. Supernumerary nostril and nasal cavity on the right side. The cavity leads up to the root of the nose, and on the skin surface corresponding to the termination of the cavity is a dimple which is said to have discharged watery looking fluid at times, but is now dry. There is profuse discharge of mucus from the cavity and on probing it blood appears from the sound and normal

nostril lying beneath it, indicating some communication between the two. On touching the eyelids the dimple and the supernumerary nostril are seen to contract. The bacteriological examination of the discharge made by Dr. Dobrashian is as follows: Direct films, *Micrococcus catarrhalis* and diphtheroids. Cultures, *Micrococcus catarrhalis* and diphtheroids. Photograph, also stereoscopic X-ray of the skull, are shown. Suggestions are asked as to surgical treatment.



I am sorry that I had neither the time nor the opportunity to look up the literature on the subject before I came here. I have never seen such a case and have not met anyone who has.

I am anxious to know what I should do with the child, who is a girl and is very disfigured. Is it wise to deal with it surgically now, or should it be left alone for two or three years? If it is thought by

members that I should operate now, my opinion is that there are two alternatives: either to incise the skin from the root of the nose to the tip, excise the supernumerary nostril and the partition between it and the normal one, removing at the same time any dermoid remains, or to throw the two cavities into one by removing the bridge of skin and the partition between them. I am inclined to think that the former plan is the better, as it allows of dealing with the dermoid remains.

Mr. W. M. MOLLISON: I think it would be easier to cut off the accessory nostril than to open it into the cavity; then later do a grafting operation and insert cartilage to make up for the deficit.

Case of Double Abductor Paralysis following Cut Throat.

By LIONEL COLLEDGE, F.R.C.S.

THE patient is a sailor aged 35. Cut throat in March, 1918. Tracheotomy performed in December, 1918. Patient was first seen at hospital in June, 1919. He was then wearing a tracheotomy tube with an extension upwards on a spring. This was causing irritation of the vocal cords and was replaced by an ordinary tube. There is no power of abduction in the cords.

The patient is shown with the object of eliciting opinions as to whether anything can be done to remedy the condition.

DISCUSSION.

Mr. COLLEDGE: Would laryngostomy or the removal of one or both cords be justifiable in this case. The man would be no worse off in the matter of voice, but owing to the loss of the front of the cricoid cartilage it might be difficult to close the wound afterwards, even if some cartilage were grafted in the flaps.

Dr. KELSON: The question here is whether the paralysis is due to the recurrent laryngeal nerves having been caught in the scar tissue or whether it is due to contraction of the muscle and fibrous tissue. If the former, it is the paralysis described by Semon, the abductors being paralysed before the other muscles, and complete paralysis will occur later. In this case the obstruction would be temporary, in the other more or less permanent.

Spontaneous Cicatrization of well-marked Tuberculosis of the Larynx, with General Treatment only.

By Sir StClair Thomson, M.D.

This lady, aged 42, developed tuberculosis, with tubercle bacilli in the sputum seven years ago. In April, 1918, the larynx was attacked with ulcerating deposit in the interarytænoid region.

Without other treatment than silence the larynx has now completely healed. It shows well-marked cicatrized tissue in the interarytænoid region, and this scarred mass is so evident that it prevents complete approximation of the vocal cords posteriorly; but she talks in a fair rough voice. Two lobes in each lung show tubercle, and the patient still brings up sputum containing tubercle bacilli. The case is therefore pathologically interesting as showing that the larynx can heal, although expectoration charged with bacilli is still passing through it. The case also would tend to discredit the view that the larynx is infected from the surface.

DISCUSSION.

Dr. Jobson Horne: There are one or two points about this case which need explanation. The notes say "It shows well-marked cicatrized tissue in the interarytænoid region, and this scarred mass is so evident that it prevents complete approximation of the vocal cord posteriorly." I very much question whether that is the case. The exhibitor also says the case shows "the larynx can heal although expectoration charged with bacilli is still passing through it." We all knew that, and I do not know why it was mentioned. His last statement is that it would tend to discredit the view that the larynx is infected from the surface. I do not think that is scientific. But the case shows how well the larynx in a tuberculous subject will get on if only it is left alone. Some time ago, Sir StClair Thomson brought forward some cases which had been at Midhurst Sanatorium, in which he had cured the laryngeal condition by applying the cautery. At that time I expressed the opinion that silence is sufficient to heal the larynx in many cases, and it is in private patients that you can get these results, for in the class of hospital patients it is almost impossible to ensure the maintenance of the silence which is necessary.

Dr. Dan McKenzie: This is an interesting case, because it looks like being a milestone. We remember the time when the laryngologist faced with tuberculosis of the larynx "went for it." I have now come to the conclusion

that, in the absence of urgent symptoms, our duty is to leave the larynx alone. Only a short time ago there was a discussion here on the latest method of treating tuberculosis of this part—namely, by galvano-cautery puncture, and it was pointed out that there was risk in this puncture when active disease was present in the larynx and lungs, but that when the lungs were quiescent then the puncture might be applied. Shortly afterwards I carried out that advice in a case, and with disastrous effect. The disease in the lungs and larynx in my case was quiescent, but the puncture never healed; it developed into a deep ulcer; the ulcer, extended, and the patient died from laryngeal phthisis. I have therefore come to the conclusion, and the case now shown supports me, that unless there is some urgent symptom to be treated, the duty of the laryngologist faced with tuberculosis of the larynx is to leave the larynx alone.

Mr. PERMEWAN: Dr. Horne's remarks surprise me, for the assumption at the root of them is, that if you let tuberculosis of the larynx alone, it will recover, especially if the subjects of it are private patients. I regret that has not been my experience. In this specialty there always seems to be some fashion reigning at the moment. Last time I was here it was galvano-puncture, but I understand that that is now discredited by some. My main object in speaking is to say I do not think it is the fact that, in the majority of cases of tubercular disease of the larynx, even in private patients in sanatoria, the patients will recover if only you leave them alone. The outlook is not as bright as that, and it would be wrong to leave the profession under the idea that it is so.

Mr. CYRIL HORSFORD: I support the remarks of the last speaker, as I have had an interesting case the result of which is a tribute to the value of the galvano-cautery. The man was living in conditions unfavourable for the cure of laryngeal tuberculosis by rest and proper hygiene. He had active disease of the larynx, and I sent him to a sanatorium for three months to improve the lung condition. When it appeared to be quiescent, I cauterized the larynx, which was plainly infiltrated in both vocal cords. In two or three months the larynx appeared to have healed. Against my wishes, he resumed his former work in a close atmosphere, and took up smoking again, and after a year or so the disease recurred. I found great infiltration of both arytænoids, ventricular bands and vocal cords, but no definite ulceration. I cauterized the affected regions by slow stages, and he is now practically healed, and the larynx is quiet.

Dr. DUNDAS GRANT: I practised at a period so far back as when tuberculosis of the larynx was practically untreated, and in those days the prognosis was infinitely worse than it is now. Then the time came when we improved matters with such treatment as injection of solutions of menthol, and more recently the use of the galvano-cautery. The sister-in-charge in my Throat Department at Brompton Hospital said those cases did best in which the

cautery had been used. Dr. McKenzie's case was a disconcerting one, but I hope it will not deter him from using the cautery in appropriate cases. It is important, however, to be sure the patient has some powers of reaction: you can hardly expect a patient who is much run down to respond well. In a "bed" case I would not use the cautery, but would employ palliatives, mainly local anæsthetics. With regard to propping apart the vocal cords by cicatrized tissue in the arytænoid region, I think the mechanism is as described by Sir StClair Thomson. I showed lately a case in which I had punched away that bar of cicatrized tissue, and the patient, who was previously unable to drink on account of regurgitation of liquids into the larynx, did so afterwards, and made good progress.

Sir StClair Thomson (in reply): Tuberculosis of the larynx is still a dire and dread disease: a large majority of the cases which show it are bound to die. Of the cases of tubercle of the lungs which would be put in Group I, six years afterwards 85 per cent. are still alive; but of those which had a lesion of the larynx, with the same amount of lung mischief, only 57 per cent. are alive six years later. Therefore I would sooner have a hole in each lung than a small hole in one lung and a lesion in the larynx. Only a small number are suitable for local treatment, and the indications are that they shall be free from a temperature, that they shall be increasing in weight, and be in what the physician would call a quiescent state. I see about sixty cases of tubercle of the larynx in one sanatorium in a year, yet there are only a few on whom one would even try the cautery. I have had no bad effects.

Case of ? Lupus of the Larynx.

By W. M. Mollison, M.Ch.

M. T., aged 18, has been hoarse for two years. On the inner side of the lower lip there is a small ulcer, irregular in outline; this has been present about a year. It is quite painless. The larynx shows much heaping up in the interarytænoid space, the right ventricular band is ulcerated, the ulcer extending down to the true cord. Examination of the chest shows extensive consolidation at the right apex, also a fair-sized cavity, some active tubercle at the left apex and apices of both lower lobes. The patient has not lost weight. The Wassermann reaction is weakly positive. The sputum is being examined.

DISCUSSION.

Dr. Irwin Moore read the following further remarks by Mr. W. M. Mollison: "The medical officer looking after the patient is giving tuberculin in increasing doses: and has expressed his surprise at the Wassermann reaction

being positive. He cannot find any history of syphilis, and the exhibitor thinks that the case should be looked upon as a chronic lupoid condition. The sputum contains tubercle bacilli."

The CHAIRMAN (Mr. MARK HAVELL): I think the Wassermann reaction is unreliable in these cases. The effect of anti-syphilitic treatment is a better criterion.

Nasopharyngeal Angeiofibroma.

By G. W. DAWSON, F.R.C.S.I.

BOY, aged 13½, came under my observation in June, 1918, suffering from a tumour which bulged the left cheek, appeared in the pharynx displacing the soft palate downwards, and presented itself at the left nostril. After a preliminary laryngotomy the tumour was removed by working through the mouth and nostril.

He returned to hospital again in March, 1919. The tumour had recurred, presenting much the same appearance as when first seen. On April 5 I inserted 90 mg. of radium under the mucous membrane over the left antrum, and left it for twenty-four hours, and again on May 1 I inserted 80 mg. of radium into the post-nasal space, and left it for twenty hours. As the action of radium is very delayed, he was sent home, and was seen again in October last. Beyond causing a puckered scar in the left cheek, the applications of radium had no beneficial effect.

The tumour was then removed by the external route. Laryngotomy was performed and an incision made from the angle of the eye along the nose and dividing the upper lip. On reflecting the cheek the white mass of the tumour came into view, the whole external wall of the antrum being absorbed. In fact most of the superior maxilla had disappeared. The floor of orbit was very thin, and two molars so loose as to be easily plucked out. The tumour was then removed with raspatory and scissors. Recovery was uneventful.

Case of Congenital Occlusion of one Choana.

By J. DUNDAS GRANT, M.D.

A LITTE girl was brought to me on account of the persistence of nasal obstruction in spite of removal of adenoids. I found the left nasal cavity occupied by a good deal of firm, slimy mucus, and complete

obstruction of the posterior choana, as shown by means of the probe and nasopharyngeal examination. The obstruction was mainly osseous, and only the central portion appeared to be fibrous. The posterior part of the septum was removed, and the obstructing bone was chiselled away till the choana was restored to nearly its normal size. She has had no return of obstruction.

Newly-designed Instruments for Œsophagoscopy and Bronchoscopy.

By Irwin Moore, M.Ch.

(1) An improved adjustable handle for exhibitor's foreign-body forceps.

(2) New pattern cutting shears for cutting up foreign bodies—e.g., tooth plates. Up to recently it was considered impossible to manufacture this instrument to reach a tooth plate impacted in the lower portion of the œsophagus. This difficulty has now been overcome.

(3) Tracheal forceps for the removal of benign growths from the trachea or main bronchi. This is the pattern of one of the forceps referred to and used by Mr. Tilley in his recent case of intratracheal tumour removed by per-oral tracheoscopy.

Papilloma at the Bifurcation of the Trachea.

By W. Jobson Horne, M.D.

The specimen is the one referred to at the previous meeting of the Section when I stated that the bifurcation of the trachea was a site for a papilloma to occur, and further expressed the opinion that the intratracheal tumour in the case brought forward by Mr. Herbert Tilley was originally a papilloma and that it had undergone degenerative changes in consequence of the "gassing."

DISCUSSION.

Dr. IRWIN MOORE read the following note from Mr. Tilley, who was unable to be present: "With reference to Dr. Jobson Horne's remarks on the pathogenesis of the intratracheal tumour shown by me at the last meeting, I can only state that previous to being 'gassed' the patient was in perfect health, was reputed to be a 'very active officer,' and furthermore, was a first-class Rugby footballer. In view of these facts, and that the symptoms of tracheal obstruction followed immediately upon those of an acute nature caused by the 'gassing,' it seems to be a somewhat gratuitous assumption that the tumour existed before the patient fell a victim to one of the many forms of German barbarity."

Dr. IRWIN MOORE: I would like to remark that Mr. Shattock's opinion of the growth removed by Mr. Tilley confirms the idea that it was caused by inflammatory reaction and ulceration, followed by granuloma. I have come across a very similar case recorded by Sir Robert Woods,[1] of Dublin, in 1913. The patient was a boy, aged 11, who had been perfectly well until two months before he was seen, when he had an attack of bronchitis followed by difficulty in breathing. The first attack of bronchitis occurred after eating periwinkles, and it was supposed that a piece of shell may have entered the trachea and caused a septic wound of the tracheal wall. Examination with the bronchoscope showed a granuloma growing from the same position as in Mr. Tilley's case, i.e., from the right wall of the trachea immediately above the bifurcation, and the air passage was reduced at this point to a small crescentic opening. The X-rays showed a condition similar to that in Mr. Tilley's case, in the position of the bifurcation. The tumour was removed, and the child recovered. Microscopical examination showed that the growth consisted of very vascular granulation tissue.

Dr. JOBSON HORNE (in reply): The tumour from Mr. Tilley's case was macroscopically a papilloma, and if "gassing" can cause that, then the discussion is settled. I exhibit this specimen merely to show that a papilloma can occur at that site, without "gassing," because the man from whom that specimen was taken has been dead twenty years. With regard to Mr. Tilley's question as to whether it is primary, or secondary to a mass of glands, there is a mass of glands at the bifurcation, but I do not think there is any relationship between the two conditions. The fact that a man is an athlete does not exempt him from the possibility of a papilloma developing at the bifurcation of the trachea.

[1] *Journ. Laryngol., Rhinol. and Otol.*, 1913, xxviii, p. 509.

Foreign Body (Mutton Bone) removed by Crico-tracheotomy from the Larynx of a Boy, aged 10.

By Douglas Guthrie, M.D.

On September 2, 1915, whilst eating bread and dripping, the patient was suddenly seized with a fit of coughing, and said that something sharp had stuck in his throat. Next day he was brought to hospital, suffering from pain referred to the larynx, dyspnœa, hoarseness, and stridulous breathing. An antero-posterior skiagram showed no foreign body, but a lateral view (shown) revealed a shadow in the subglottic region, opposite the fifth cervical vertebra.

Under suspension laryngoscopy the piece of bone was clearly visible, but the only available forceps proved too slender, and a good grip could not be secured. At this stage breathing became so embarrassed that tracheotomy was performed, and it was decided to defer any further attempt to dislodge the bone, which remained tightly impacted in the larynx. Two days later, the wound was explored and enlarged. So firmly fixed was the foreign body that it was necessary to divide the cricoid cartilage before removal could be effected. The tracheotomy tube was removed next day and subsequent progress was uneventful. In spite of the injury to the cricoid cartilage, the patient, on being examined two years later, was found to be free from any sign of laryngeal stenosis.

DISCUSSION.

Dr. Irwin Moore: The failure to remove this foreign body by the direct method was no doubt due to the operator being handicapped by the want of proper and efficient forceps. Under ordinary circumstances an external operation should not be necessary. When a bone is impacted across the larynx and cannot be removed by forceps it is possible to cut it in half with the smaller-sized cutting shears, such as I have introduced for cutting up dental plates.

Dr. Douglas Guthrie (in reply): I tried, before the meeting, to cut the specimen with Dr. Irwin Moore's shears, but was unable to make much impression on the hard, thick bone. I exhibited the case to show that, despite the great vogue of endoscopic methods, there is still a field for external surgery, this foreign body being easily removed from the subglottic region through a

tracheotomy wound. Had the bone been dragged through the glottis, the cords would surely have been damaged. It is remarkable that no stenosis followed the division of the cricoid cartilage. Laryngeal stenosis seems prone to occur after so high a tracheotomy, but on examining this boy two years later I could find no trace of stenosis.

Female, aged 33, four months after a Laryngofissure for Intrinsic Cancer of Larynx.

By G. A. Carter, F.R.C.S. (Stoke-on-Trent), C. W. M. Hope, F.R.C.S., and Sir StClair Thomson, M.D.

The points of interest are: (1) The sex and comparative youth of the patient; (2) hoarseness of four years' duration; (3) growth involving the entire right vocal cord, and projecting sufficiently to allow of a piece being removed for preliminary microscopic examination; and (4) free mobility of the cord.

At the operation the growth was removed in one mass, after excision of the greater part of the right thyroid ala. The subglottic area was not invaded. A tracheotomy tube was left in for forty-eight hours.

Sections are shown under the microscope.

DISCUSSION.

Dr. Jobson Horne: All four clinical points given here are those usually given against it being malignant disease. Therefore we are thrown back upon the microscopical sections, which I looked at. I was unfortunate in not finding carcinoma, though that may have been the fault of the sections, which are not cut in a conclusive manner. The part removed at the operation has not been cut at right angles to the cord; it looked as if it had been cut partly obliquely, and partly parallel to the growth. You can demonstrate what seems to be carcinoma in innocent larynges if the section is not cut at right angles to the cord.

Dr. Irwin Moore: I do not agree with Dr. Horne that the clinical history of this case suggests non-malignancy. The projection of the growth, though involving the entire cord, and the free mobility of the cord showing that the growth has not yet infiltrated the tissues deeply, are indications of early malignant disease.

Case of Melanotic Sarcoma of the Nose (Sections shown).

By W. M. Mollison, M.Ch.

Mrs. S., aged 43, consulted me in October, 1919, on account of left-sided nasal obstruction of about eighteen months' duration. A polyp had been removed without relief. Some epiphora of the left eye. Some epistaxis.

Anterior rhinoscopy revealed a dark purple mass of the shape of a nasal polyp: indeed I considered it might be a polyp which had undergone some degeneration or which had become gangrenous from strangulation. Under cocaine and hemisine the mass was removed with a snare; the wire cut through with great ease. The piece thus removed resembled dark blood clot very closely. There was slight bleeding, but it was possible to see that beyond the piece removed there were more dark-coloured "polypi."

Microscopical examination was made by Professor Laidlaw and the section (here shown) revealed typical melanotic sarcoma.

Further operation was advised: before this was done Sir StClair Thomson saw the case. Lateral rhinotomy was decided upon. Intra-tracheal insufflation of ether was given by Dr. Shipway. The external carotid was ligatured in case it were found necessary to carry out an extensive removal of ethmoid and maxilla. Moure's incision gave good access to the nasal chamber and ethmoid region. The growth appeared to have arisen in the lower part of the outer wall—? from the inferior turbinal—certainly an edge was found quite far forward, even anterior to the inferior turbinal: there was no invasion either of the ethmoid or of the antrum.

Dr. Nicholson kindly was present at the operation and took pieces of the growth for section, also a gland which was found lying on the carotid. His sections are shown and his report confirms the previous one. The gland shows no secondary deposit.

The patient made a somewhat slow recovery on account of an attack of erysipelas.

Mr. W. M. Mollison (in further remarks): The sections show a large amount of pigment and a sarcomatous arrangement of the cells. At operation a definite line of demarcation was observed between the normal mucosa and the growth; but microscopically, the growth is seen to be originating under normal mucous membrane, and it is impossible to see where the growth begins.

Case of Extensive Papilloma of the Hard Palate.

By Chichele Nourse, F.R.C.S.Ed., and
Archer Ryland, F.R.C.S.

Patient, a male aged 55, a tram conductor, was first seen in July, 1919. The buccal surface of the hard palate on the right side is involved by a new growth which has been present for three months. There is no glandular enlargement. The patient wears an upper denture. Transillumination: Right antrum dark. Puncture and lavage: A small quantity of pus evacuated. The Wassermann reaction is negative. Report of the microscopical section shows that the growth is a papilloma.

Present condition: The growth has extended slightly across the middle line and is beginning to involve the buccal surface of the left hard palate. It has also extended externally, involving the right upper alveolus, and also postero-externally. There is now a small sinus in connexion with the right alveolus which extends upwards and inwards towards the floor of the nose. There is no pain, no ulceration, and no bleeding.

Infiltration of Right Ventricular Band, Vocal Cord and Pyriform Sinus. ? Malignant.

By W. H. Jewell, M.D.

G. S., a male, aged 71, Has been hoarse for about ten weeks. There is no pain nor difficulty in swallowing. The apices of the lungs and the sputum are negative for tubercle bacilli. The Wassermann reaction is negative.

Present condition (December 1, 1919): Infiltration of right vocal cord, ventricular band and pyriform sinus. On the surface of the latter there is a small abrasion and three or four pin-head spots of blood. The movement of the cord is impaired.

I regard the condition as malignant, and the fact that the infiltration involves the pyriform sinus proscribes the operation of laryngo-fissure and his general infirmity that of total excision of the larynx.

Postscript.—Shortly after he was shown he developed great difficulty in swallowing and much pain.

Case of Hoarseness; Ventricular-band Phonation.

By J. Dundas Grant, M.D.

The vocal cords are only seen in their whole length during inspiratory phonation. Opalescent white plaques are seen on the tonsils, more opaque than ordinary secondary syphilitic patches, and apparently free from spirochætes. Primary specific infection three and a half years ago. The voice "went" about ten weeks ago after hoarseness for about a fortnight; returning in its present hoarse form five weeks ago. A functional condition, probably the residuum of a temporary laryngitis.

The following series of specimens representing Diseases of the Larynx, and exhibited at the Summer Congress of Laryngology, have been presented to the Section of Laryngology Museum, Royal Society of Medicine, by Dr. Jobson Horne.—

(1) Œdema of the larynx, following upon the breaking down of a gumma.

(2) Pachydermia laryngis.

(3) Cystoma of the larynx.

(4) Malignant disease of the larynx.

(5) Malignant disease of the larynx.

The following specimens and drawings shown at the Section Meeting on December 5, have also been presented to the Museum:—

(1) Fibroma of the aryepiglottic fold—Mr. W. M. Mollison.

(2) Watercolour drawing of œsophageal lumen, showing multiple polypi (œdematous fibromata)—Dr. William Hill.

(3) Fish bone which perforated the inferior constrictor and caused fatal mediastinal abscess—Dr. William Hill.

(4) Photograph of supernumerary nostril and cavity—Mr. Bell Tawse.

(5) Papilloma at the bifurcation of the trachea—Dr. Jobson Horne.

CORRIGENDUM.

In the report of Dr. James Donelan's "Case of Epithelioma of the Tongue" (No. 2, December, 1919, p. 14, line 4 from bottom) for "estimated" read "saturated."

Section of Laryngology.

President—Mr. E. B. Waggett, D.S.O., M.D.

A Description of the more interesting Specimens from the Pathological Museum of the 1st Annual Summer Congress of the Section of Laryngology, held at the Royal Society of Medicine, on May 2 and 3, 1919.

(Dr. James Donelan, President of the Section, 1918-1919, and subsequently Brigade-General H. S. Birkett, C.B., C.A.M.C., in the Chair.)

Compiled by Irwin Moore, M.Ch.

CLASSIFICATION.

Specimens representing diseases of the following regions were exhibited :—

(I) Head and Face.
(II) Nasal Passages and Accessory Sinuses.
(III) Pituitary Fossa.
(IV) Post-nasal Pharynx.
(V) Mouth and Pharynx.
(VI) Larynx.
(VII) Trachea.
(VIII) Bronchi.
(IX) Œsophagus.
(X) Stomach.

With the approval of the Council of the Section of Laryngology I have compiled the following notes and references relating to the more interesting specimens exhibited in the Museum at the 1st Summer Congress of the Section of Laryngology.

With two exceptions the drawings have been executed by Mr. Thornton Shiells under my immediate supervision. As in the case of the specimens of Œsophagectasia, previously compiled and published,[1] all the preparations were submerged in a water bath with flat sides in order to obviate the magnification caused by the cylindrical jars in which the specimens were preserved, and great care has been taken to accurately define their true proportions and special features.

I am indebted to many members of the Section for their kind assistance, and especially to Professor Shattock, for much help and advice.

The original drawings of these specimens with their references will be placed in the Museum at the Royal Society of Medicine.

(I) HEAD AND FACE.

Encapsuled Osteoma of the Frontal Bone.

Specimen No. 4328 *from the Museum of Guy's Hospital.*[2]

Description.—E. P., aged 15, a girl, was admitted under Mr. Birkett. Two years previously, after an attack of scarlet fever, a swelling had been noticed on her forehead, and shortly afterwards the left eyeball became displaced. Four months before her admission she had a "fit," after which she remained insensible for twenty-four hours, and the tumour was incised, giving vent to a large quantity of gelatinous fluid. For a week before admission she had suffered with severe pain in the left side of the face and head. At this time there was a considerable swelling above and below the left eyebrow, and the left eyeball was depressed and displaced outwards. Her general health was good. A month later an attempt was made to excise the tumour, and during the operation a large quantity of ropy, tenacious fluid was evacuated. Thirty-eight days later she died from abscesses in the frontal lobes of the brain. At the autopsy, apart from the local condition, there were no signs of disease.

Pictures of the patient, the skull and the fresh specimen were also shown.

[1] Irwin Moore: "The Pathology of Œsophagectasia (Dilatation of the Œsophagus without Anatomic Stenosis at the Cardiac Orifice)," *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.), p. 57.

[2] See *Guy's Hosp. Reports*, xxxi, p. 503, and Drawings 2, 85, 86, 87.

Encapsuled Osteoma of the Frontal Bone.

Specimen No. 4329 *from the Museum of Guy's Hospital.*[1]

Description.—A. K., aged 24, was admitted under Mr. Bryant in 1873. He stated that his attention had first been drawn to his condition five years previously by protrusion of his right eye. This had gradually increased for three and a half years, when a small swelling appeared on his forehead. After this the protrusion of his eye remained stationary, while the swelling on his forehead grew rapidly. On his admission there was a hard bony tumour in the centre of his forehead just above the root of the nose, and his right eye was displaced outwards, downwards and forwards. His eyesight was unimpaired, except that when looking upwards he had double vision. There were two small sinuses discharging pus close to the upper border of the right orbit. His general health was excellent, and he had been able to perform his work as an ostler up to the time of his admission. The tumour was removed by operation in two parts. It was found to be quite loose and apparently necrosed, and that portion which lay in the frontal sinus was surrounded by a layer of granulation tissue, which had replaced the normal mucous membrane of the sinus. The patient recovered with very little deformity, but with loss of sight in his right eye from ulceration of the cornea during convalescence, and when seen a year later he was in good health.

Composite Odontoma of the Upper Jaw.

Specimen No. 4370 *from the Museum of Guy's Hospital.*[2]

Description.—T. M., aged 36, was admitted under Mr. Hilton in 1836. The tumour had been slowly growing for twenty-three years in the position of the left maxillary antrum. In its growth it had destroyed by its pressure the floor of the left orbit, pushing the eyeball outwards and finally causing it to burst. Afterwards the anterior wall of the antrum gave way and, the integument sloughing, the surface of the tumour became exposed. This condition lasted for six years, suppuration being copious meanwhile. At last, to the man's great astonishment, the bony mass dropped out of its bed, leaving a deep chasm in his face, which was bounded below by the hard palate, above by the cribriform plate of the ethmoid bone and the left frontal sinus, and internally by the septum nasi, while posteriorly it opened into the nasopharynx. The cavity gradually closed and the man eventually recovered.

Pictures of the patient at the time that the tumour fell out and thirty years later were shown.

[1] See *Guy's Hosp. Reports*, xxxiv, p. 102, and Drawing 2.

[2] This specimen is figured in Bland-Sutton's "Tumours," 4th ed., 1906, p. 244. See *Guy's Hosp. Reports*, 1836, i, p. 493; Wax model, 38; and Drawing 3, 50.

Necrosis of the Upper Jaw.

Specimen No. 4167 *from the Museum of Guy's Hospital.*[1]

Description.—A portion of a left superior maxilla, which has become detached as the result of acute necrosis. The sequestrum comprises the anterior part of the body of the bone with its alveolar process and carries the temporary first and second molar and canine teeth, in front of which is seen the socket of the lateral incisor. Its surface is smooth, except where it has been separated from the living bone, and shows no alteration in shape. The teeth are free from disease.

A. B., aged 4, was admitted under Mr. Davies-Colley for necrosis of the left upper jaw. He had been confined to his bed for some months previously with "congestion of the lungs" and had been very ill. About six weeks before his admission, when he was just beginning to get about again, his mother noticed that his left cheek was swollen, and that his breath was very foul. The swelling persisted and a fortnight before his admission the alveolus was seen to be exposed at the base of the second molar tooth. The sequestrum was removed without difficulty and the child made a rapid recovery.

Necrosis of the Upper Jaw.

Specimen No. 4172 *from the Museum of Guy's Hospital.*[2]

Description.—Two irregular sequestra, which were removed from a left superior maxilla. The smaller one is from the alveolar process, and presents the sockets of the two incisor teeth, while the larger constitutes a considerable portion of the body of the bone.

E. P., aged 24, was admitted under Mr. Bryant in 1869, with necrosis of the upper jaw. The illness had started without any assignable cause about six months before, with pain and swelling, speedily followed by suppuration. Both the upper incisor teeth, together with the lateral incisor and canine of the left side, had come away about a month previous to her admission. The sequestra were removed by operation, the cavity of Highmore being thereby exposed, and recovery followed.

Necrosis of the Nasal and adjoining Bones of the Face.

Specimen No. 4165 *from the Museum of Guy's Hospital.*[3]

Description.—T. S., aged 34, was admitted under Mr. Steward for nasal polypi, which were attached around the opening of the left infundibulum, and completely blocked the left nostril. These were removed, together with part of

[1] See *Guy's Hosp. Surg. Reports*, 1890, No. 194.

[2] See *Guy's Hosp. Reports*, xv, p. 244.

[3] See *Guy's Hosp. Surg. Reports*, 1911, No. 133.

the left inferior turbinate bone. Four months later he was readmitted with swelling of his left cheek, which had been present for several days. During the next six weeks a number of abscesses formed in this position, and were opened. At the end of this time he suddenly began to have fits and became comatose. Extradural abscess was diagnosed and the anterior fossa of the cranium explored, but with a negative result. At the operation the necrosed bone seen in the specimen was removed. The patient died four hours later, and at the autopsy he was found to have basal meningitis, with infective thrombosis of most of the venous sinuses of the base of the brain.

Dermoid Cyst of the Face projecting into the Frontal Sinus.

Specimen No. 4836 *from the Museum of Guy's Hospital.*[1]

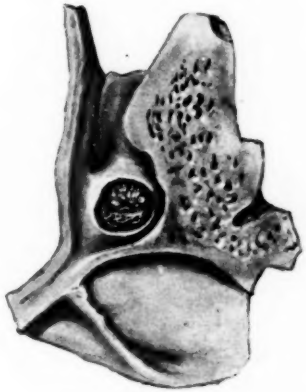


FIG. 1.

Dermoid cyst of the face projecting into the frontal sinus. (Specimen No. 4836 from the Museum of Guy's Hospital.) (Natural size.)

Description.—A vertical section through the frontal region of a face, exhibiting a small dermoid cyst, which lies beneath the skin over the root of the nose and is lodged in a depression in the frontal and nasal bones. The cyst measures $\frac{1}{2}$-in. in diameter, and contains hair and sebaceous matter.

[1] "Descriptive Catalogue of the Pathological Specimens in the Museum of Guy's Hospital," 1916, v, p. 236.

(II) NASAL PASSAGES AND ACCESSORY SINUSES.

Calvarium showing Result of Infective Osteomyelitis of the Skull in a Case of Chronic Frontal Sinus Suppuration.

From the Museum of University College Hospital.[1]

Description.—From a patient, aged 22, on whom a radical operation was performed for chronic frontal suppuration. Subperiosteal abscesses followed at different parts over the frontal and parietal bones which were opened and exposed necrosed bone, until finally the whole scalp was undermined. The

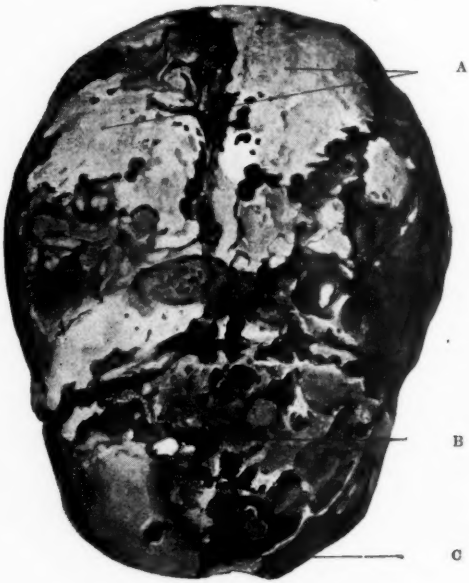


FIG. 2.

Calvarium showing result of infective osteomyelitis of the skull in a case of chronic frontal sinus suppuration. A, necrosed portions of outer table of skull; B, longitudinal sinus; C, left frontal sinus. (From the Museum of University College Hospital.)

outer table became necrotic in large patches, some of which loosened and came away. Operative attempts were made to get beyond the spreading disease, but the septic phlebitis of the diplöic veins continued and nine months later patient died from septic infection.

[1] "Descriptive Catalogue of the Specimens illustrating Surgical Pathology in the Museum of University College, London," new ed., 1906, part iii.

The specimen shows very extensive necrosis of the vault of the skull extending a little lower than the level of the torcular. In several areas an attempt at the formation of new bone will be noted (vide illustration). The dura mater was adherent all over the vault. The right petrous bone is necrosed to the apex. The left mastoid antrum and sigmoid groove are thrown into one cavity through a large hiatus. It was from this patient that Mr. Tilley described the first case in 1897,[1] in which these relationships were noted.[2]

Rhinolith.

Private Specimen exhibited by Mr. J. T. Layton.

Description.—The larger mass was removed from the nose in June, 1913: by September it had re-formed, several bits were removed under anæsthesia, and amongst it the pebble was found. This fits into one of the pieces, showing that the lighter coloured laminæ were those nearest the stone.

G. T., aged 42, gave a history that as a child he easily caught cold and had a tendency to nasal discharge. Twenty-one years ago he fell while skating on the ice and hurt his nose and broke a tooth. Since that time he has continually had a nasal discharge. He has no memory of putting anything up his nose, but distinctly remembers as a child a mass "of flesh looking like a grape" coming down from it. As a boy he lived by the seaside. When shown the pebble he said it must have been there since childhood. He had a very narrow, ill-developed nose.

Glanders of the Nasal Septum and Maxillary Antrum.

Specimen No. 1763c *from the Museum of St. Bartholomew's Hospital.*[3]

Description.—Upper jaw, nose, septum and adjacent parts from a case of human glanders. Certain of the lesions shown probably depend upon tertiary syphilis, from which the patient also suffered. Externally the nose and upper lip show extensive ulceration and scabbing, giving the surface a blackened and eroded appearance. The ulceration extends into the nostrils, and is seen on the septum nasi. The latter is perforated behind, probably as the result of syphilis, and part of the necrosed vomer lies in the perforation. The mucous membrane on the septum shows several yellowish-white glanders nodules, and the left antrum, which has been laid open, presents similar appearances. It would appear that in this case recent glanders lesions have been superposed on old lesions of tertiary syphilis.

[1] British Medical Association Meeting (Portsmouth), August 3, 1899. Vide *Lancet*, 1899, ii, p. 534. Dan McKenzie, "Diffuse Osteomyelitis from Nasal Sinus Suppuration," *Journ. Laryng., Rhinol. and Otol.*, 1913, xxviii, p. 7.

[2] An illustration of this specimen is shown in Tilley's "Diseases of the Nose and Throat," 1919, p. 279.

[3] "A Descriptive Catalogue of the Anatomical and Pathological Museum of St. Bartholomew's Hospital," 1882.



FIG. 3.

Nasal polypi. (Specimen No. 2611 from the Museum of University College Hospital.) ($\frac{3}{4}$ natural size.)

BENIGN GROWTHS.

Nasal Polypi.

Specimen No. 2611 from the Museum of University College Hospital.[1]

Description.—A long, continuous chain, 13 in. long, of mucous polypi removed from the nose with forceps. The individual polypi are lobulated and smooth and are connected together by torn shreds of mucus. At the middle of the specimen is a fragment of bone, probably from the middle turbinate. The polypi were removed by Mr. Christopher Heath with polypus forceps.

MALIGNANT GROWTHS.

Perithelioma of the Ethmoid and Maxillary Antrum.

Private Specimen exhibited by Mr. Lawson Whale.[2]

Description.—A solid tumour which easily shelled out of the right maxillary antrum of a woman, aged 52. It was removed through an opening in the canine fossa, leaving, as the operator (Mr. H. L. Whale) thought, a small portion of growth at the upper and inner angle of the antrum. The growth recurred, and six to nine months later the patient died of secondary recurrence in the brain.

Patient was shown at a meeting of the Section of Laryngology, on March 5, 1915.

Recurring Epithelioma of the Maxillary Antrum from a Case in which Moure's Lateral Rhinotomy was performed.

Private Specimen exhibited by Dr. Irwin Moore[3]*. Now in the Museum of the Royal Society of Medicine.*

Description.—The upper half of the right side of the face removed post-mortem from a female, aged 62, upon whom lateral rhinotomy had been performed one year previously. The growth involved the antral and orbital walls, causing a large sloughing cavity in the cheek, and extended backwards into the sphenoidal sinus and cranium.

History.—Following the operation the face wound healed in five days. Three months later patient appeared to be doing well. Six months after

[1] "Descriptive Catalogue of the Specimens illustrating Surgical Pathology in the Museum of University College, London," New Ed., 1906, part iii.

[2] For clinical history and histological report see Lawson Whale, *Proc. Roy. Soc. Med.*, 1915, viii (Sect. Laryngol.), p. 92; also op. cit., 1919, xii, p. 179.

[3] This specimen was exhibited at a meeting of the Section of Laryngology, Royal Society of Medicine, on November 1, 1918. *Vide* Irwin Moore, *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.), p. 24. For full notes of the case op. cit., 1917, x (Sect. Laryngol.), p. 60.

operation recurrence was observed rapidly extending from the zygomatic fossa to the orbit, necessitating removal of the right eye. The patient died six months later, the growth having extended into the cranial cavity.

(III) PITUITARY FOSSA.

Pituitary Tumour.

Specimen, as yet unnumbered, from the Museum of St. Thomas's Hospital.

Description.—Tumour of pituitary, in connexion with which there is a circumscribed cyst filled with blood clot, the parts being removed after death. Microscopically it consists of a freely interconnected network of epithelial cells supported by capillaries. A few laminated calcareous spherules are scattered through the tissue: some of these lie amidst the epithelial cells, some in the delicate interstitial tissue (S. G. Shattock).

From a patient, aged 45, who four years previously had noticed a visual defect in the right eye. The right visual field showed a large central scotoma

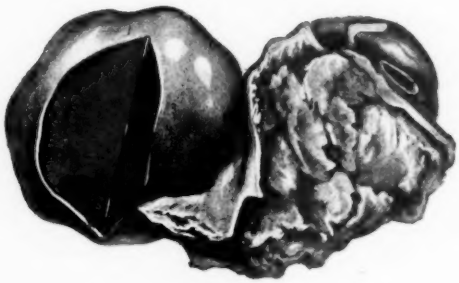


FIG. 4.

Pituitary tumour. (Specimen as yet unnumbered, from the Museum St. Thomas's Hospital.) (Natural size.)

for white and colours; the left eye became involved. Signs of hypopituitarism were well marked; headache severe. The presence of the tumour was diagnosed by skiagraphy.

Sellar decompression was undertaken by Mr. Howarth to relieve the headache, and with good result. The patient, however, died suddenly the next day. At the autopsy the right optic nerve was found attenuated over the tumour, which was embedded in the sella turcica. The cyst possibly became filled with blood as a result of the decompression, and so caused death; it was furnished with a well defined fibrous capsule, and was not ruptured.

(Mr. Howarth's case.)

Pituitary Cyst.

Private Specimen exhibited by Mr. E. D. D. Davis.[1] *Now in the Museum of the Royal Society of Medicine.*

Description.—Specimen of a pituitary cyst opened by the Killian-Hirsch operation and removed post mortem from a man aged 37. The cyst was of a cherry-red colour, and projected between the optic nerves into the anterior fossa of the skull, whilst the tail end of the cyst lay immediately over the opening made by the operation on the floor of the sella turcica. There was considerable absorption of the dorsum sellæ and basal meningitis.



FIG. 5.

Pituitary cyst. (Private specimen exhibited by Mr. E. D. D. Davis, now in the Museum of the Royal Society of Medicine.) (Natural size.)

History.—Patient suffered from progressive blindness, with optic atrophy and signs of hypopituitarism. At the operation hæmorrhage was profuse on incision of the dura. The day following the operation hemiplegia developed, with loss of speech, paralysis of the right face, right arm and leg. Four days later the sight improved and the patient could read a newspaper. Patient died seven weeks after the operation with symptoms of meningitis, but the paralysis of the leg had practically disappeared.

[1] This specimen was exhibited at a meeting of the Section of Laryngology, Royal Society of Medicine, on February 5, 1915. For full history of the case see E. D. D. Davis, *Proc. Roy. Soc. Med.*, 1915, viii (Sect. Laryngol.), p. 57.

(IV) POST-NASAL PHARYNX.

BENIGN GROWTHS.

Antro-choanal Polypus.

Private Specimen exhibited by Mr. T. B. Layton.

Description.—The growth consists of two masses attached by a common pedicle. A portion of the growth occupied the posterior part of the left naris, whilst the remainder extended into the naso-pharynx. It is marked by a constriction which corresponds to the choanal margin. Removed from a male, aged 78. Polypi were also removed from the posterior portion of the right naris.

Antro-choanal Polypus.

Specimen No. 2615A *from the Museum of University College Hospital.*[1]



FIG. 6.

Antro-choanal polypus. (Specimen No. 2615*a* from the Museum of University College Hospital.) (Natural size.)

Description.—The tumour forms a flattened ovoid, measuring 4½ cm. by 3 cm. by 2 cm., and near the insertion of its pedicle gives off a slender secondary outgrowth, 4 cm. in length by 5 mm. in diameter. Removed from a patient aged 13.

Antro-choanal Polypus.

Unnumbered Specimen from the Museum of the Central London Throat, Nose and Ear Hospital.

Description.—Showing that it has undergone necrosis.

[1] "Surgical Catalogue," 1906, part iii.

Antro-choanal Polypus.

Private Specimen exhibited by Mr. W. S. Syme.

Description.—It was attached by a long pedicle to a large polypus in the left maxillary antrum.

Antro-choanal Polypus.

From the Museum of the Royal College of Surgeons.[1]

Description.—A polypus, bisected, which was removed from the naso-pharynx. The growth is of flattened triangular form, about 3 cm. (1¼ in.), along its sides. It is provided with two distinct pedicles, the longer of which has been partially torn longitudinally, so as to give an appearance of having been double.

Microscopically it presents a uniform structure, and consists of a meshwork of wavy fibrils, amongst which are distributed a few cells of the lamellar, connective tissue kind. The vessels are small and scanty (S. G. Shattock).



FIG. 7.

Antro-choanal polypus. (From the Museum of the Royal College of Surgeons.) Dr. Donelan's case. (Natural size.)

The tumour was removed by Dr. Donelan from a man, aged 22. One pedicle passed into the maxillary antrum ; the other into an accessory antrum. The antro-nasal operation was carried out shortly afterwards, and the antrum cleared of polypi with which it was filled.

[1] This specimen was exhibited at a meeting of the Section of Laryngology, Royal Society of Medicine, on April 4, 1919. For report of case and discussion see Donelan, *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Larygol.), p. 167.

Postscript by Professor S. G. Shattock.—The specimen of antro-choanal polypus No. 2615A from the Museum of University College Hospital suggests that one of the two pedicles in the above specimen may represent a secondary outgrowth of the polypus which has acquired a secondary attachment to the mucosa.

(Dr. Donelan's case.)

Recurring Spheno-choanal Polypus (Soft Fibroma).

Specimen, not yet numbered, from the Museum of the Royal College of Surgeons.[1]

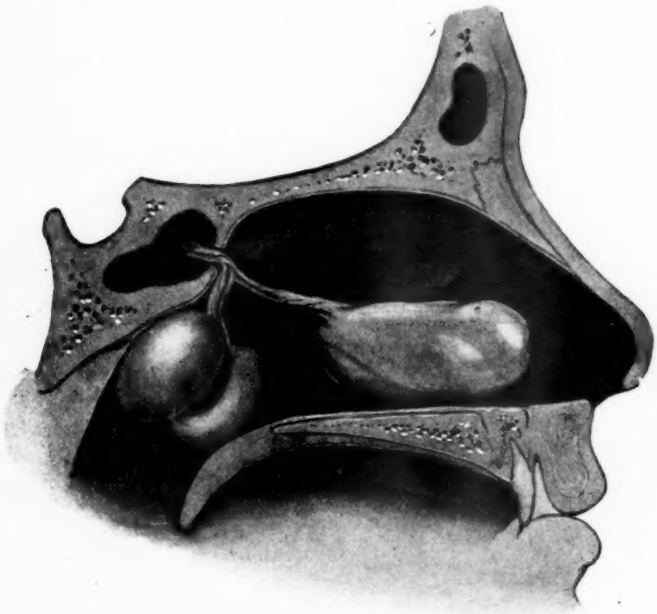


FIG. 8.

Recurring spheno-choanal polypus (soft fibroma.) A sagittal section through the left naris and sphenoidal sinus showing the original growth placed in position. (Natural size.)

Description.—A recurring growth removed from the nasopharynx of a little girl, aged 8, consisting of two polypi on separate pedicles (fig. 9). The larger specimen consists of a short folded pedicle which has become flattened. At the

[1] Patient and the growth were exhibited at a meeting of the Section of Laryngology, Royal Society of Medicine, on February 2, 1917, and February 7, 1919. For full notes see Irwin Moore, *Proc. Roy. Soc. Med.*, 1917, x (Sect. Laryngol.), p. 65, also *ibid.*, 1919, xii, p. 104.

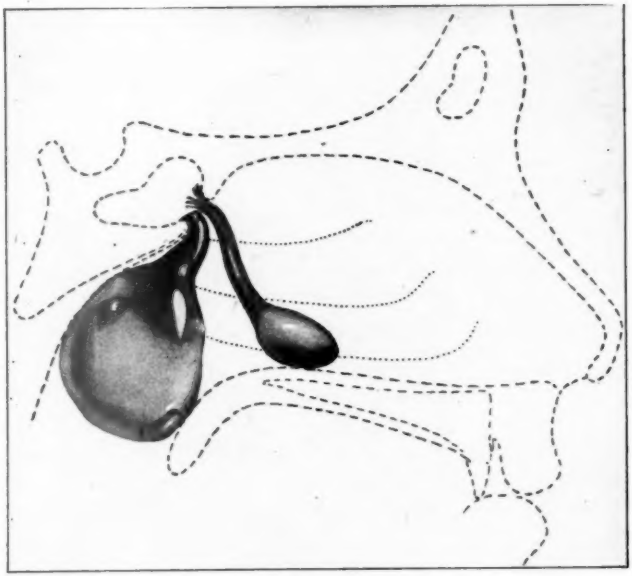


FIG. 9.

Recurring spheno-choanal polypus. A similar section showing the recurring growths removed two years later. (Natural size.)

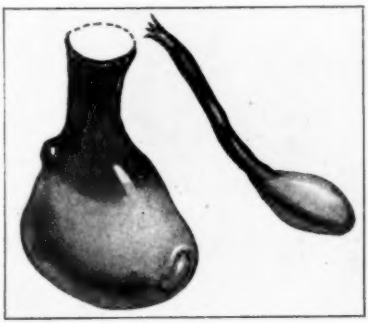


FIG. 10.

Recurring spheno-choanal polypus. The two polypi on separate pedicles after removal. The dotted circle represents the sphenoidal ostium, to the lower margin of which was attached the flattened pedicle of the larger growth. (Specimen, not yet numbered, from the Museum of the Royal College of Surgeons.) Dr. Irwin Moore's case. (Natural size.)

extremity of the pedicle, i.e., at its origin, a portion of the antero-inferior wall of the sphenoidal sinus is attached. The pedicle measures 12 mm. (nearly $\frac{1}{2}$ in.), whilst the body of the growth measures 26 mm. (1 in.) by 22 mm. ($\frac{7}{8}$ in.). The smaller specimen consists of a long funicular pedicle measuring 30 mm. ($1\frac{3}{16}$ in.) in length, to which is attached a small polypus (12 mm. by 8 mm.). The origin of the pedicle attached to the larger growth was traced to the lower margin of the left sphenoidal ostium (S. G. Shattock) (fig. 10). The original growth (fig. 8) in this case was removed two years previously, i.e., at 6 years of age, and consisted of two polypi (a nasal and post-nasal portion) attached by separate pedicles to a common stalk originating in the sphenoidal sinus.

This specimen is of exceptional interest and rarity, for no case of spheno-choanal polypus has previously been recorded under 20 years of age, and only nine have been recorded in medical literature, and these occurred between 20 and 30 years of age.

(Dr. Irwin Moore's case.)

(1) Nasopharyngeal Fibroma.

Private Specimen exhibited by Mr. E. D. D. Davis.[1]

Description.—A globular, hard, smooth growth measuring $2\frac{1}{2}$ in. by $1\frac{1}{2}$ in. Removed from the nasopharynx of a boy, aged 14, in July, 1914, by means of a strong curved dissector and the finger, without splitting the palate. Patient suffered from nasal obstruction, slight deafness, but no epistaxis.

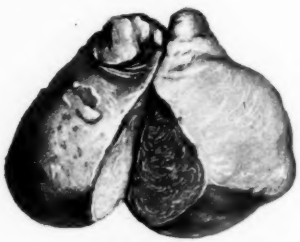


FIG. 11.

Naso-pharyngeal fibroma. (Private specimen exhibited by Mr. E. D. D. Davis, now in the Museum of the Royal Society of Medicine.) (Natural size.)

The origin of the growth was from the body of the sphenoid, and it filled the nasopharynx and had caused absorption of the posterior edge of the vomer and hard palate.

[1] This case was shown, with microscopical sections of the growth, at the meeting of the Section of Laryngology, February 5, 1915. See E. D. D. Davis, *Proc. Roy. Soc. Med.*, 1915, viii (Sect. Laryngol.), p. 55.

(2) Nasopharyngeal Fibroma.

Private Specimen exhibited by Mr. Herbert Tilley.[1]

Description.—A large tumour removed piecemeal at three successive operations. Many of the fragments are lobulated and have a smooth surface covered with mucous membrane. The tumour is a very vascular fibroma.

From a boy, aged 14, who had suffered from complete nasal obstruction and blood-stained discharge for five months. Recently the discharge became offensive and the patient became weak and anæmic. The nose was broadened and the left nostril distended by a grey, sloughy mass, which bled freely; the right nostril was blocked by a deviation of the septum. By posterior rhinoscopy the left choana was seen to be filled by a reddish mass, smooth and elastic, and springing apparently from the basi-sphenoidal and ethmoidal regions. An inverted U-shaped incision was made by Mr. Tilley over the root of the nose and the latter turned downwards, and part of the growth removed with forceps. The palate was completely divided in the middle line and the left half of the hard palate removed; the remainder of the growth was then removed with snares, scissors, and cutting forceps. Three months later a considerable recurrent growth was removed, and two months later a further removal was repeatedly treated with the galvano-cautery, but without success. The nasal cavities and left antrum were then freely opened from within the mouth and an extensive recurrent growth removed from the antrum and from the basi-sphenoidal and ethmoidal regions. Six months later the cleft in the soft palate was closed by operation. Two years later the patient was in perfect health, with no sign of recurrence.

Patient is now a tall, powerful man.

(V) MOUTH AND PHARYNX.

A Series of Specimens showing Implications of the Throat in Spinal Caries, from the Museum of Guy's Hospital.

(*a*) Necrosis of the Atlas.

Specimen No. 3604.[2]

Description.—Two fragments (marked A and B) of the anterior arch of the atlas. The fragment A shows the odontoid facet, and both are roughened and are evidently sequestra. The portion C is the corresponding part broken off the normal atlas D.

From a patient of Mr. J. Babington in 1834. There was no history of syphilis. For four months she had pain at the back of the head and her neck

[1] Tilley, *Proc. Laryng. Soc. Lond.*, 1902-03, x, p. 19.

[2] "Descriptive Catalogue of Guy's Hospital Museum," 1913, iv, p. 49.

became stiff. These symptoms increased until one day on coughing she brought from her mouth several fragments of bone. After this her head became almost fixed, and she had a discharging ulcer on the posterior pharyngeal wall. Four years later she was in good health.

This case is quoted in John Hilton's "Rest and Pain," 4th ed., 1887, p. 113.

(*b*) Tuberculous Caries of the Atlas and Axis.

Specimen No. 3608.[1]

Description.—J. H., aged 19, was admitted for pain and swelling at the back of his neck after a fall five months previously. He stated that two months before he went to bed well, but on waking in the morning was paraplegic, in which condition he was admitted; the respiratory movements were impaired, and there was incontinence of urine and increased reflexes. An attempt to perform laminectomy had to be abandoned, owing to the intense dyspnœa caused by placing him in the prone position. At the autopsy caries of the atlas and axis was found, the transverse ligament was destroyed, and there was an abscess cavity opening into the pharynx at the level of the axis. There was an extensive patch of myelitis extending for 4 in. below the origin of the fourth cervical nerve, while a second smaller patch was present opposite the diseased vertebræ.

(*c*) Tuberculous Caries of Cervical and Dorsal Vertebræ.

Specimen No. 3611.[2]

Description.—W. S., aged 50, was admitted in 1835 for pain and weakness of the right arm, dyspnœa and dysphagia. At the autopsy an abscess, the size of a hen's egg, was found opening into the œsophagus and situated in front of the seventh cervical vertebra, which was carious; the seventh cervical and first dorsal nerves were embedded in the inflammatory exudation. The lungs contained numerous tubercles.

(*d*) Tuberculous Caries of Cervical Vertebræ associated with Tuberculosis of the Adrenals.

Specimen No. 3612.[3]

Description.—L. T., aged 30, was admitted for spinal caries. Two weeks after admission an abscess was opened on the right side of the neck below the mastoid process, and ten days later a piece of bone the size of a pea, which

[1] *See* Mr. Symonds's "Surgical Reports," 1890, No. 75, and Insp. 1890, No. 4.

[2] *See* Insp., xxi, p. 73.

[3] *See* Mr. Davies-Colley's *Guy's Hosp. Surg. Reports*, 1892, No. 202, and Insp. 1892, No. 153.

had worked its way through the posterior wall of the pharynx, was removed by the mouth. The following day the patient became unconscious and died. No symptoms of Addison's disease had been observed.

Styloid Process Embedded in the Left Tonsil.

Private Specimen exhibited by Dr. W. S. Syme.

Description.—Found and removed during enucleation of the tonsil in a man, aged 50.

Large Submucous Lipoma of the Palate and Pharynx.

Private Specimen exhibited by Mr. Herbert Tilley. Now in the Museum of the Royal College of Surgeons.[1]

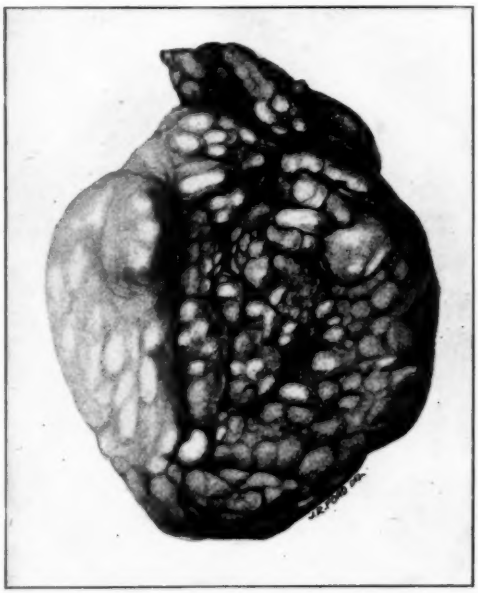


FIG. 12.

Large submucous lipoma of the palate and pharynx. (Private specimen exhibited by Mr. Herbert Tilley, now in the Museum of the Royal College of Surgeons.)

[1] This specimen was exhibited at a meeting of the Section of Laryngology, Royal Society of Medicine, on June 6, 1919. For full description of the specimen, along with history of the case, see Tilley, *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.), p. 189. A life-size drawing of the growth is also illustrated on. p. 190.

Description.—A tumour of rare occurrence in this situation, measuring $2\frac{1}{2}$ in. in diameter, $1\frac{1}{5}$ in. at its greatest thickness, i.e., the palatal portion of the growth, removed from a female, aged 15.

Histological report by Professor S. G. Shattock, F.R.S., showed that it consisted of simple adipose tissue, the groups of fat cells being supported by scanty trabeculæ of connective tissue furnished with lamellar corpuscles. In a few of the fat cells the nucleus presents a single vacuole, as it may in normal fat.

Artificial Pharyngeal Pouch.

Specimen No. 3391D *from the Museum of St. Bartholomew's Hospital.*[1]



FIG. 13.

Artificial pharyngeal pouch. (Specimen No. 3391D from the Museum of St. Bartholomew's Hospital.) (Natural size.)

Description.—Pharynx and larynx of a native of India, showing a pouch formed by the prolongation downwards of the left sinus pyriformis. The pouch has been produced by artificial means for the purpose of concealing money.

[1] "A Descriptive Catalogue of the Anatomical and Pathological Museum of St. Bartholomew's Hospital," 1882.

Extract from a letter accompanying the specimen: "A larynx taken from an habitual convict who had made a pouch in his throat for keeping money." The leaden disks preserved in the next specimen are employed by the convicts to form these pouches, which, "I have been told, can be made large enough to hold 10 rupees (a rupee is about the size of a two-shilling piece), but I think this must be rare—a five-rupee size is more usual."

Leaden Disks for producing Artificial Pharyngeal Pouches.

Specimen No. 3391M *from the Museum of St. Bartholomew's Hospital.*[1]



FIG. 14.

Leaden disks for producing artificial pharyngeal pouches. (Specimen No. 3391M from the Museum of St. Bartholomew's Hospital.) (Natural size.)

Description.—Six disks of lead weighing from 70 to 120 gr., used by convicts in India to make pouches in their throats, as illustrated by the preceding specimen. No. 1 was removed in presence of the donor by a warder, who

[1] Op. cit.

compelled the prisoner to disgorge his disk by gripping him firmly by the throat. The disk had been worn by the prisoner while eating his food, and the white material in the crevices is flour from chupatties. No. 2 is a large-sized disk, and of the others some are rough and some are smooth. They are anchored in position by the pieces of string shown, which are wedged in between the back teeth. Sulphuric acid obtained from the dyeing sheds is sometimes smeared on the disks before insertion, a practice which occasionally gives rise to acute laryngitis.

These six disks, which are numbered, are mounted in a glass case, and weigh respectively: (1) 70 gr.; (2) 120 gr.; (3) 35 gr.; (4) 40 gr.: (5) 45 gr.; (6) 48 gr.

Professor S. G. Shattock has kindly supplied the following particulars of two similar disks of lead in the Museum of the Royal College of Surgeons (Specimen No. 2235·1)[1]: "Two disks of lead each about 2·5 c.m. (1 in.) in diameter, of the kind which are used by habitual criminals in India for the purpose of increasing the depth of the sinus pyriformis. The string attached to the weight is fixed to one of the teeth in order to prevent its being accidentally swallowed. The pockets, known as ganahs, are utilized for secreting coins or precious stones. In one of the cases, in which one of the above weights was used, it was found after death that the foreign body had produced ulceration as far as the thyroid cartilage on one side, and half way through the muscles on the other."

Presented by I. W. Little, Esq., 1911.

An interesting point, mentioned by the donor to Professor Shattock, is that criminals with coins secreted in these artificial pouches, may be inverted and shaken without the coins coming away.

Pharyngeal Diverticulum.

Specimen No. 1523B *from the Museum of University College Hospital.*[2]

Description.—The pouch is attached to the lower extremity of the pharynx at its posterior aspect, and overlaps the upper part of the œsophagus. It is conoidal in shape and flattened from before back. Vertically it measures 2 in., from side to side $2\frac{3}{4}$ in. It is situated almost exactly in the middle line, and projects on either side beyond the œsophagus.

From a patient, aged 76, under Sir Rickman Godlee in University College Hospital. He had for six months complained of difficulty in swallowing and vomiting after food; he also noticed a fullness in the left side of the neck after taking fluids and a tendency to eructations on pressing in that situation. Under X-rays the bismuth meal was arrested at the level of the upper border

[1] "A Descriptive Catalogue of the Museum of the Royal College of Surgeons."

[2] "Descriptive Catalogue of the Specimens illustrating Surgical Pathology in the Museum of University College Hospital, London," new ed., 1906, part iii.

of the sternum, with its long axis transverse to that of the œsophagus. Death occurred from bronchitis and pneumonia.

Additional Note.—In this instance the pouch, which is particularly capacious, is unusual in that it rises as high as the upper level of the



FIG. 15.

Pharyngeal Diverticulum. (Specimen No. 1523B from the Museum of University College Hospital.) The arrow indicates the upper border of the cricoid cartilage. (¾ natural size.)

cricoid plate, and has a mouth of exceptional size. Like the summit of the pouch, the upper edge of its mouth lies at the same level; the sac involves much of the lateral as well as the posterior walls of the pharynx (S. G. Shattock).



FIG. 16.

Pharyngeal diverticulum ruptured by passage of a bougie. (Specimen not yet numbered, from the Museum of the Royal College of Surgeons.) ($\frac{1}{2}$ natural size.)



FIG. 17.

Pharyngeal diverticulum ruptured by passage of a bougie. Upper view showing orifice of the pouch. ($\frac{1}{2}$ natural size.)

Pharyngeal Diverticulum ruptured by Passage of a Bougie.

From the Museum of the Royal College of Surgeons.[1]

Description.—The lowest part of the sac presents a ragged perforation which resulted from the use of a bougie. The connective tissue on the back of the œsophagus below the sac is covered with the inflammatory exudate. A rod of white glass has been passed into the sac through an oval aperture (1 cm. in chief diameter) in the posterior wall of the pharynx at the level of the lower border of the cricoid cartilage. (S. G. Shattock.)

History.—Patient died about forty-eight hours after the operation from acute mediastinitis.

Pharyngeal Diverticulum.

Private specimen exhibited by Dr. Kelson.[2]



FIG. 18.

[1] Referred to by Irwin Moore in discussion on paper by Kelson, "Pharyngeal Diverticula," Summer Congress, May, 1919, Section of Laryngology. Vide *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.) p. 250.

[2] For reference to this case see *Proc. Roy. Soc. Med.*, 1914, vii (Sect. Laryngol.), p. 248; also op. cit., 1919, xii, p. 249; *Journ. Laryngol., Rhinol. and Otol.*, 1914, xxix, p. 198 (in this journal an illustration of the pouch is shown).

Description.—A pouch which extended down to the aortic arch causing great difficulty in swallowing by compression of the œsophagus. It measured 2½ in. in depth, and its mouth was 1 in. in diameter. Removed under local anæsthesia (cocaine) from a male aged 70, who was very emaciated.

Pharyngeal Diverticulum.

Private specimen exhibited by Mr. Patrick Dempsey.

Description.—Pharyngeal diverticulum removed from a patient aged 56. Usual symptoms and X-ray finding. Operation through neck under ether, no complication afterwards, and full restoration to health.

(VI) LARYNX.

FOREIGN BODIES.

Piece of Bone from Sheep's Head.

Specimen No. 1708 *from the Museum of the London Hospital.*[1]

Description.—A portion of bone removed on October 19, 1889, from the larynx of a woman aged 33. About two months previously whilst eating some sheep's head a piece of bone stuck in her throat. She pushed the bone down with her fingers to relieve the feeling of suffocation. Since the accident her breathing had been interfered with. When seen she had a slight dyspnœa accompanied with a little stridor on deep inspiration, and the cough which came on occasionally had the sound similar to that produced by laryngeal obstruction.

On examination the larynx was seen to be normal, but congested. About ¼ in. below the level of the vocal cords the upper border of the foreign body was visible. The larynx was sprayed with cocaine 10 per cent., and the bone was removed through the mouth with forceps. Until after removal the two pieces were held together by fibrous tissue.

Bolus of Meat.

Specimen No. 1711 *from the Museum of the London Hospital.*[1]

Description.—A larynx in which a large mass of meat is plugged. No history has been preserved, but no doubt the accident caused immediate death.

[1] "A Catalogue of the Pathological Specimens in the Museum of the London Hospital," Series "Injuries and Diseases of the Larynx and Trachea."

Fish Bones.

Specimen No. 1712 *from the Museum of the London Hospital.*[1]

Description.—A larynx of a young child who died of suffocation at the moment of admission. The post-mortem disclosed the cause in the form of some fish bones which had become fixed in the larynx by perforating the mucous membrane.

Necrosis of Thyroid and Cricoid Cartilages.

Specimen No. 1730 *from the Museum of the London Hospital.*[1]

Description.—A cricoid and thyroid cartilage, ossified and necrosed.

INFLAMMATORY AND INFECTIOUS AFFECTIONS.

Septic Laryngitis.

Specimen No. 1726 *from the Museum of the London Hospital.*[1]

Description.—The trachea and larynx of an adult opened posteriorly. There is considerable œdema of the epiglottis, aryepiglottic folds and ventricular bands. There is a superficial ulceration of the mucous membrane of the larynx. Below the level of the vocal cords of both sides, but much more extensive in area on the right side, the ulceration is covered by a membranous exudation. Tracheotomy has been performed.

Acute Œdema of the Larynx.

Specimen No. 805 *from the Museum of Charing Cross Hospital.*[2]

Description.—A larynx of a patient, aged 18, who died of septicæmia consequent on gangrenous stomatitis. The posterior aspect of the epiglottis shows several submucous hæmorrhages, and an extensive submucous extravasation has occurred into the left aryteno-epiglottidean fold of mucous membrane and the adjacent portions of the glottis and larynx. This caused swelling and œdema of the glottis, and necessitated a laryngotomy.

Mr. Boyd's case. Died May 16, 1899.

[1] "A Catalogue of the Pathological Specimens in the Museum of the London Hospital," Series "Injuries and Diseases of the Larynx and Trachea."

[2] "A Descriptive Catalogue of the Pathological Museum of Charing Cross Hospital," 1902, p. 107.

Multiple Hæmorrhages of the Larynx and Trachea from a Case of Septicæmia.

Specimen No. 1753 *from the Museum of the London Hospital.*[1]

Description.—A larynx and trachea, opened from behind, showing multiple hæmorrhages into the mucous membrane.

Diphtheria of the Larynx and Trachea.

Specimen No. 1734 *from the Museum of the London Hospital.*[1]

Description.—A larynx and trachea showing diphtheritic disease from a woman who was admitted for dyspnœa. Tracheotomy was performed but the patient was only temporarily relieved, and died soon after.

Diphtheritic Cast of Trachea and Bronchi.

Specimen No. 1734A *from the Museum of the London Hospital.*[1]

Description.—A membranous cast of the trachea and bronchial tubes, which was coughed up by the patient from whom the preceding specimen was obtained as soon as the trachea was opened.

Diphtheria of the Larynx and Trachea.

Specimen No. 812 *from the Museum of Charing Cross Hospital.*[2]

Description.—Fauces, larynx and trachea of a child, aged 2, who died of diphtheria. The palate, side walls of pharynx and whole larynx and trachea are covered by false membrane.

Dr. Bruce's case. Died June 27, 1897.

A Series of Specimens representing Typhoid Conditions of the Larynx.

Typhoid Ulceration of the Larynx.

Specimen No. 1782A *from the Museum of St. Thomas's Hospital.*[3]

Description.—Showing marked stenosis which followed—below the cords.

[1] "A Catalogue of the Pathological Specimens in the Museum of the London Hospital," Series "Injuries and Diseases of the Larynx and Trachea."

[2] "A Descriptive Catalogue of the Pathological Museum of Charing Cross Hospital," 1902, p. 107.

[3] "Descriptive Catalogue of the Pathological Preparations in St. Thomas's Hospital Museum."

Typhoid Ulceration and Necrosis of the Larynx.

Specimen No. 1641A *from the Museum of St. Bartholomew's Hospital.*[1]

Description.—Larynx from a case of typhoid fever. Its mucous membrane is injected and in many places superficially ulcerated. The upper part of the epiglottis is deeply ulcerated, exposing the necrosed cartilage. Ulcers are also present at the base of the epiglottis, and immediately below each true vocal cord. The patient was a man, aged 39, who died of exhaustion on the forty-third day of the disease.

Typhoid Ulceration of the Larynx.

Specimen No. 1641A1 *from the Museum of St. Bartholomew's Hospital.*[1]

Description.—The larynx removed from the body of a girl, aged 17, who died between the third and fourth week, of typhoid fever. The tip of the epiglottis is eroded and superficially ulcerated. The intestines showed well-marked typhoid ulcers, most of them still with adherent sloughs.

Typhoid Ulceration and Necrosis of the Larynx.

Specimen No. 1641A2 *from the Museum of St. Bartholomew's Hospital.*[1]

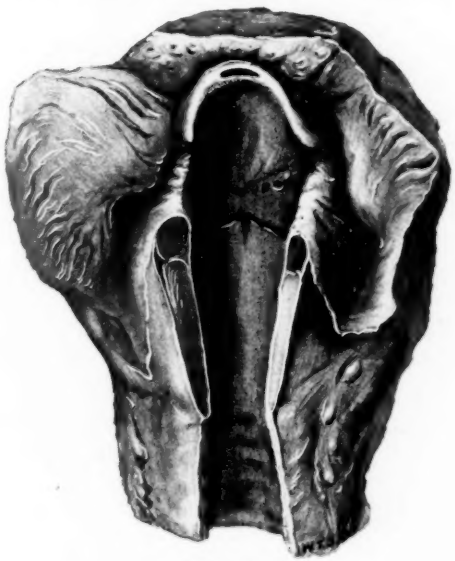


FIG. 19.
Typhoid ulceration and necrosis of the larynx. (Specimen No. 1641A from the Museum of St. Bartholomew's Hospital.) (Natural size.)

[1] "A Descriptive Catalogue of the Anatomical and Pathological Museum of St. Bartholomew's Hospital," 1882.

Description.—Larynx of a man, aged 20, who died in the fifth week of typhoid fever. The tip of the epiglottis is ulcerated in its whole extent, and the cartilage exposed and bare in a necrotic condition. The intestines show fresh typhoid ulcers.

Typhoid Ulceration and Necrosis of the Larynx.

Specimen No. 1641B *from the Museum of St. Bartholomew's Hospital.*[1]

Description.—Larynx from a patient who died from typhoid fever. A small ulcerating opening is seen immediately behind the posterior attachment of the left true vocal cord. A bristle has been passed through the opening into a cavity in which lie the necrosed arytænoid cartilages.

Typhoid Ulceration and Necrosis of the Larynx.

Specimen No. 1641B_1 *from the Museum of St. Bartholomew's Hospital.*[1]

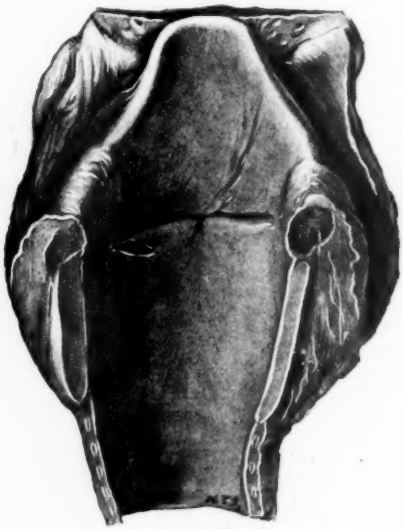


FIG. 20.

Typhoid ulceration and necrosis of the larynx. (Specimen No. 1641B1 from the Museum of St. Bartholomew's Hospital.) (Natural size.)

Description.—Larynx removed from the body of a man, aged 28, who died in the sixth week of typhoid fever from general peritonitis (not perforative in nature). The specimen shows a deep ulcer just under the left processus vocalis;

[1] "A Descriptive Catalogue of the Anatomical and Pathological Museum of St. Bartholomew's Hospital," 1882.

a probe passed into it downwards and forwards grated against the bare cartilage (or bone) of the cricoid ring. At the autopsy pus could be squeezed out from below through the slit-like opening of the ulcer. The intestines showed healing and cicatrizing ulcers, except on the ileo-cæcal valve, where fresh ulcers still existed.

Typhoid Ulceration and Necrosis of the Larynx.

Specimen No. 1641c *from the Museum of St. Bartholomew's Hospital.*[1]

Description.—The larynx of a boy, aged 15, in which extensive necrosis of the cartilages has taken place after typhoid fever. The ventricular bands were swollen and the aperture of the glottis was closed. There was no ulceration of the mucous membrane within the trachea, but after the glottis was opened it was found that the whole of the cricoid cartilage was necrosed and lying loose in the abscess, which extended for 2 in. upwards and downwards along the back and sides of the larynx. The right arytænoid cartilage could not be identified, but the left appeared to be normal. The patient died fourteen days after defervescence. He had suffered from noma on the inside of the right cheek, which was treated by scraping and nitric acid. He also had right parotitis. There was very slight tenderness over the larynx, and he was able to swallow solid food up to the day of his death. He died quite suddenly from laryngeal spasm.

Tuberculous Ulceration of the Larynx.

Specimen No. 1747 *from the Museum of the London Hospital.*[2]

Description.—A larynx laid open posteriorly. On the left processus vocalis there is a deep tubercular ulcer, and its edge is undermined anteriorly, the ulcer narrows until it is merely a splitting of the cord. On the right side there are several small ulcers, at the root of the epiglottis there are several irregular ulcers, and the largest is near the middle line and its edge is considerably undermined.

Tuberculous Ulceration of the Larynx and Trachea.

Specimen No. 818 *from the Museum of Charing Cross Hospital.*[3]

Description.—The larynx and trachea from a case of pulmonary tuberculosis, opened from behind. The natural vascularity is preserved, showing very extensive tuberculous lesions and ulceration. The mucosa of the trachea and

[1] "A Descriptive Catalogue of the Anatomical and Pathological Museum of St. Bartholomew's Hospital," 1882.

[2] "A Catalogue of the Pathological Specimens in the Museum of the London Hospital," Series "Injuries and Diseases of the Larynx and Trachea."

[3] "A Descriptive Catalogue of the Pathological Museum of Charing Cross Hospital," 1902, p. 108.

bronchi is acutely inflamed and shows at various parts irregular ulceration with exposure in some places of the subjacent cartilages. The ulceration is deepest just below the right half of the glottis, and from there down the trachea for a distance of over 3 in. The mucosa over the cords presents a characteristic greyish sodden appearance, with ulceration at their posterior extremity.

Dr. Green's patient, aged 33. Died February 11, 1900.

Syphilis of the Larynx.

Specimen No. 1737 *from the Museum of the London Hospital.*[1]

Description.—A deep ulcer with sharply defined border situated immediately below the true vocal cords and opening into the left ventricle of the larynx. There is much fibrous thickening from the left ventricular band to the epiglottis.

Syphilis of the Larynx and Trachea.

Specimen No. 1738 *from the Museum of the London Hospital.*[1]

Description.—A larynx and trachea, the mucous membrane of the latter being everywhere thickened and showing patches of flocculent lymph. An inch below the vocal cords there is a long patch of ulceration, the margins of which are very definite; it involves the whole thickness of the mucous membrane; it is more than an inch long and half an inch broad. Probably syphilitic.

Mixed Tuberculosis and Syphilis of the Larynx.

Unnumbered Specimen from the Museum of the Central London Throat, Nose and Ear Hospital.

BENIGN GROWTHS OF THE LARYNX.

Papillomata of the Larynx.

Specimen No. 1754 *from the Museum of the London Hospital.*[1]

Description.—A larynx and tongue of a child. The larynx has been opened up from behind and shows numerous warty growths. The main mass is on the left side and is attached by a broad base to the true vocal cord and the region of the arytænoid cartilage. There is also a smaller growth protruding from the laryngeal ventricle. A more definitely pedunculated papilloma is attached to the arytænoid region, and several minute ones are seen along the right vocal cord.

[1] "A Catalogue of the Pathological Specimens in the Museum of the London Hospital," Series "Injuries and Diseases of the Larynx and Trachea."

Papilloma of the Larynx.

Specimen No. 1755 *from the Museum of the London Hospital.*[1]

Description.—Larynx opened from behind, showing a single papilloma springing by a narrow base from the middle third of the left true vocal cord.

Papillomata upon the Vocal Cords.

Specimen No. 830 *from the Museum of Charing Cross Hospital.*[2]



FIG. 21.
Papillomata upon the vocal cords. (Specimen No. 830 from the Museum of Charing Cross Hospital.) (Natural size.)

No history is obtainable.

[1] "A Catalogue of the Pathological Specimens in the Museum of the London Hospital," Series "Injuries and Diseases of the Larynx and Trachea."

[2] "A Descriptive Catalogue of the Pathological Museum of Charing Cross Hospital," 1902, p. 109.

Lipoma of the Larynx.

Specimen No. 1786B *from the Museum of St. Thomas's Hospital.*[1]



FIG. 22.

Lipoma of the larynx. (Specimen No. 1786B from the Museum of St. Thomas s Hospital.) (Natural size.)

Description.—Deeply lobulated lipoma about $2\frac{1}{4}$ in. in diameter, removed from right aryepiglottic fold of a man aged 40. It hung down into the pharynx and could be protruded at will by the patient into the cavity of the mouth. (S. G. Shattock.)

Lipoma of the Larynx.

Private Specimen exhibited by Mr. Hunter Tod.[2] *Now in the Museum of the Royal Society of Medicine.*

Description.—The growth was the size of a pigeon's egg, of a yellowish appearance and composed of fatty tissue. Its surface was smooth and sparsely covered with fine vessels. It was attached to the posterior part of the larynx, and interarytænoid space on the left side. On deep inspiration it was sucked down towards the glottis.

[1] "Descriptive Catalogue of the Pathological Preparations in St. Thomas's Hospital Museum." *Vide* also S. Jones, *Trans. Path. Soc. Lond.*, 1881, xxxii, p. 243; S. G. Shattock, *Proc. Roy. Soc. Med.*, 1909, ii (Sect. Path.), p. 285.

[2] This case was shown at a meeting of the Section of Laryngology, Royal Society of Medicine, on December 7, 1917. For full description of the specimen and history of the case see *Proc. Roy. Soc. Med.*, 1918, xi (Sect. Laryngol.), p. 67.

Patient, a female, aged 58, had suffered for twelve months from increasing difficulty in breathing, at night and after exertion, along with an aggravating cough with occasional spasmodic attacks of dyspnœa.

The growth was removed by suspension-laryngoscopy and a snare.

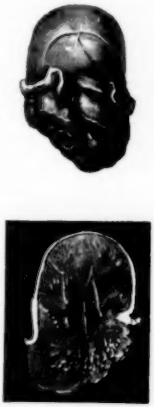


FIG. 23.

Lipoma of the larynx, attached to posterior wall of larynx and inter-arytænoid space on left side. (Exhibited by Mr. Hunter Tod; now in the Museum of the Royal Society of Medicine.) (Natural size.)

Cyst of the Epiglottis.

Private Specimen exhibited by Dr. Jobson Horne.[1]

Description.—A larynx showing a cyst on the lingual aspect of the epiglottis. The tumour, which was relatively of a large size, had occasioned no symptoms during life.

In this specimen the tenseness and colour of the cyst are remarkably well preserved.

The specimen is now in the Museum of the Royal Society of Medicine.

[1] This specimen was exhibited at a meeting of the Laryngological Society, London, on January 4, 1907; also at a meeting of the Section of Laryngology, Royal Society of Medicine, on June 6, 1919. See *Proc. Laryngol. Soc. Lond.*, 1907, xiv, p. 32, also *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.), p. 195. It is also referred to in an article by the exhibitor on "Cystoma of the Epiglottis," *Journ. Laryngol., Rhinol. and Otol.*, 1907, xxii, p. 245. An illustration of this specimen also appears in both the first and last publications.

MALIGNANT GROWTHS OF THE LARYNX.

Squamous-celled Carcinoma of the Epiglottis.

Specimen No. 1756 *from the Museum of the London Hospital.*[1]



FIG. 24.
Squamous-celled carcinoma of the epiglottis. (Specimen No. 1756 from the Museum of the London Hospital.) ($\frac{4}{5}$ natural size.)

Description.—The right half of a tongue, larynx and trachea showing a cauliflower-like growth of the epiglottis. The tumour, which is about the size of a pigeon's egg, hangs over the upper part of the larynx and fills up the lumen of the pharynx. The microscopical section of the growth shows it to be a squamous carcinoma.

Epithelioma of the Larynx.

Private Specimen exhibited by Mr. E. D. D. Davis.[2]

Description.—Larynx removed post mortem from a male aged 76, following thyrofissure for malignant disease of the right vocal cord. The patient,

[1] "A Catalogue of the Pathological Specimens in the Museum of the London Hospital," Series "Injuries and Diseases of the Larynx and Trachea."

[2] This specimen was shown at the meeting of the Section of Laryngology, Royal Society of Medicine, on May 27, 1914. Vide *Proc. Roy. Soc. Med.*, 1914, vii (Sect. Laryngol.), p. 196.

aged 76, complained of hoarseness and a sense of obstruction during breathing for nine months. The growth, which was circumscribed and ulcerating, involved the anterior third of the right vocal cord and ventricular band, extending into the anterior commissure, with limitation of movement of the cord. The larynx was opened and the growth widely removed with the anterior third of the left cord. With the exception of difficulty of swallowing the patient did well, but on the seventh day after operation he collapsed and died from heart failure.

Epithelioma of the Larynx (with Implantation).

Private Specimen exhibited by Dr. W. S. Syme.

Description.—Epithelioma of the larynx to show secondary growth due to implantation. The main growth occupied practically the whole of the left side of the larynx above the cord. On the right side was a growth attached to a narrow base just above the anterior end of the cord. It was quite isolated from the main growth. From a woman aged 37.

Sarcoma of Arytænoid Region (small round-celled).

Specimen No. 838 *from the Museum of Charing Cross Hospital.*[1]

Description.—Primary growth. Specimen laid open from behind to show mass of new growth involving interarytænoid membrane and arytænoid cartilages, of a dense, dirty-white, fleshy appearance. The mucosa over it on the left side is somewhat papillomatous, and on the right ulcerated. The arytæno-epiglottic folds are invaded, but the true vocal cords are uninvolved. The growth is extending down under the mucosa, covering the thyroid alæ. The tip of the cornicula laryngis presents on the cut surface. The thyroid gland is entirely replaced by sarcomatous growth confined within its capsule. The cut surface shows yellowish points of degeneration or old hæmorrhages.

Secondary growths existed in the cæcum, intestine and elsewhere.

Microscopy.—Small round-celled sarcoma.

[1] "A Descriptive Catalogue of the Pathological Museum of the Charing Cross Hospital," 1902, p. 110 (Mr. Boyd's case). For history see Intestine No. 1131 ; P.M. 2069, November 3, 1898.



FIG. 25.

Sarcoma of the arytænoid region. (Specimen No. 838 from the Museum of Charing Cross Hospital.) (⅔ natural size.)

(VII) TRACHEA.

FOREIGN BODIES.

Orange Seed in Trachea.

Specimen No. 1709 *from the Museum of the London Hospital.*[1]

Description.—A larynx and upper part of the trachea from a young child who had an orange pip impacted in the glottis. An operation was proposed, but the parents would not consent. The orange pip can be seen from below effectually blocking up the rima glottidis.

Perforation of the Trachea caused by Suppuration of a Lymphatic Gland.

Specimen No. 1719 *from the Museum of the London Hospital.*[1]

Description.—The larynx, trachea and œsophagus of an adult. The trachea has been laid open from the front, and shows on its posterior wall a small

[1] "A Catalogue of the Pathological Specimens in the Museum of the London Hospital," Series "Injuries and Diseases of the Larynx and Trachea."

perforation through which a white rod has been passed and this orifice is in communication with an abscess cavity in an enlarged lymphatic gland which is firmly attached to the outer surface of the trachea. The glandular mass has been cut across at its lower part and exhibits a large cavity surrounded by a shell of glandular tissue.

Perforation of the Innominate Artery in a Child from the use of a Tracheotomy Tube.

Specimen No. 1801D *from the Museum of St. Thomas's Hospital.*[1]

Description.—The lungs and trachea of a child with the arch of the aorta and its branches. A rod of green glass has been passed through a small circular communication between the middle of the anterior wall of the trachea, and the innominate artery from which fatal hæmorrhage occurred. The

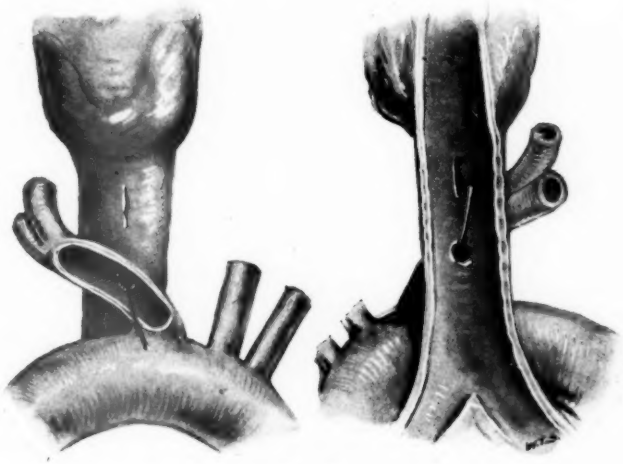


FIG. 26.

Perforation of the innominate artery in a child from the use of a tracheotomy tube. (Specimen No. 1801D from the Museum of St. Thomas's Hospital.) (Natural size.)

communication was due to ulceration set up by the end of a tracheotomy tube. In the trachea above, there may be recognized the vertical slit of the tracheotomy wound. (S. G. Shattock.)

From a child suffering from diphtheria, upon whom tracheotomy had been carried out fourteen days before the fatal hæmorrhage.

[1] "A Descriptive Catalogue of the Pathological Preparations in the Museum of St. Thomas's Hospital."

Syphilitic Stenosis of the lower part of the Trachea.

Specimen No. 1804 *from the Museum of St. Thomas's Hospital.*[1]

Description.—It extended into the bronchi. There was pronounced fibrosis of the lungs. In a boy, aged 15, with inherited syphilis.

MALIGNANT GROWTHS.

Sarcoma of the Trachea.

Specimen No. 1658A *from the Museum of St. Bartholomew's Hospital.*[2]

Description.—The specimen, which was obtained from the body of a woman, aged 35, shows the bifurcation of the trachea opened from behind. At the angle of the bifurcation there is a small growth which microscopically

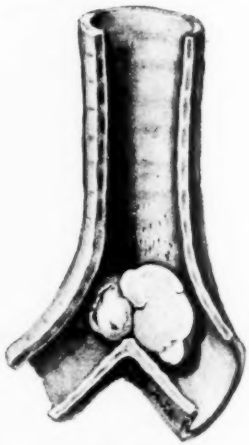


FIG. 27.

Sarcoma of the trachea. (Specimen No. 1658A from the Museum of St. Bartholomew's Hospital.) (Natural size.)

proved to be a very vascular sarcoma. It was secondary to a sarcoma of the left kidney, and there were secondary deposits in the lungs.

Postscript by Prof. S. G. Shattock.—The fact that there were metastatic tumours in the lung suggests that the growth in the lower end of the trachea may have been due to the implantation of a fragment in the orifice of a gland.

[1] "A Descriptive Catalogue of the Pathological Preparations in St. Thomas's Hospital Museum."

[2] "A Descriptive Catalogue of the Anatomical and Pathological Museum of St. Bartholomew's Hospital," 1882.

(VIII) BRONCHI.

FOREIGN BODIES.

Two Fragments of Bone in the Left Bronchus.

Specimen No. 1710 *from the Museum of the London Hospital.*[1]

Description.—A portion of the trachea and bronchial tubes showing two fragments of bone impacted in the left bronchus. There is no history to the case.

Bean impacted in the Right Bronchus.

Specimen No. 1681c *from the Museum of St. Bartholomew's Hospital.*[2]



FIG. 28.
Bean impacted in the right bronchus. (Specimen No. 1681c from the Museum of St. Bartholomew's Hospital.) (⅔ natural size.)

Description.—Parts of a trachea and lungs prepared to show a bean impacted in the right bronchus and the results of the impaction. The whole of the right bronchus from the bifurcation of the trachea to the origin of the

[1] "A Catalogue of the Pathological Specimens in the Museum of the London Hospital," Series "Injuries and Diseases of the Larynx and Trachea."

[2] "A Descriptive Catalogue of the Anatomical and Pathological Museum of St. Bartholomew's Hospital," 1882.

main branches is occupied by a haricot bean, lying longitudinally. The bronchus is enlarged at the site of impaction. Both lungs contain air. The left lung is apparently healthy; the right lung is slate-coloured and mottled with yellowish areas of early broncho-pneumonia; its air-tubes are generally dilated. On the surface are a few areas of interstitial emphysema.

Microscopic examination shows early broncho-pneumonia and some emphysema.

From an infant aged 21 months. He had been playing with beans, but was not known to have taken any into his mouth. Three days before his death he was seized with violent dyspnœa, and examination showed that no air was entering the right lung. During the last three days of life he suffered many recurrent attacks of dyspnœa, in one of which he died. Post-mortem examination showed no lesions beyond those seen in the specimen.

Pencil Protector lodged in the Left Bronchus of a Child.

Specimen, as yet unnumbered, from the Museum of St. Thomas's Hospital.

Description.—The lower part of the trachea with the left lung, of a child. Impacted in the left bronchus there is the metal cap of a dance pencil, the rounded apex of which lies downwards. From a child, aged 3, who was seized with sudden choking whilst playing with the pencil, and became black in the face. After an hour the patient was apparently well, but cough persisted. The foreign body was seen with the bronchoscope forty-eight hours later. Two days after that tracheotomy was performed; by lower bronchoscopy the body was again visible. For three hours, attempts to remove it were unsuccessful, as the forceps could not be got between it and the mucosa; it was thereupon left *in situ* for suppuration to occur around it, before further attempts were made. Death occurred suddenly three days later. The lower lobe of the lung was collapsed from the blockage of its supplying bronchus. No cause of death was discovered (S. G. Shattock).

From a case under Mr. Howarth.

(IX) ŒSOPHAGUS.

FOREIGN BODIES.

Toothplate impacted in the Œsophagus: Death from Perforation into the Aorta.

Specimen No. 1868A *from the Museum of St. Bartholomew's Hospital.*[1]

Description.—An œsophagus and aorta, showing a toothplate which, impacted in the œsophagus just below the level of the bifurcation of the trachea, caused ulceration and perforation of the wall of the œsophagus and of the

[1] "A Descriptive Catalogue of the Anatomical and Pathological Museum of St. Bartholomew's Hospital," 1882.

aorta. The toothplate is of vulcanite holding a single tooth, which replaced the left upper lateral incisor; it has two wings which fitted the molar teeth on either side. The plate lies across the œsophagus with the tooth and part of



FIG. 29.

Toothplate impacted in the œsophagus, which caused perforation of the aorta. (Specimen No. 1868A from the Museum of St. Bartholemew's Hospital.) (¾ natural size.)

the plate embedded in the wall on the right side, and the left wing embedded in the left lateral wall. The right wing with its curved wire hangs free in the lumen. The ulceration caused by the tooth on the right side is confined to the

œsophageal wall, and has left the bronchus unharmed; on the left side, however, the ulceration has perforated the œsophagus and penetrated the wall of the aorta by a small round opening at the junction of the transverse and descending aorta. The opening is marked by a red glass rod.

A young woman, aged 24, about eight months before her death, swallowed the toothplate in a fainting fit. A soft œsophageal tube was passed, and near the cardiac orifice of the stomach met with obstruction, which seemed to give way easily. She lived on soft foods for a time and then returned to ordinary diet. After her confinement which took place soon after the accident, and was without abnormal incident, she suffered from pain in the stomach, and sought relief at a London hospital, where the diagnosis of "foreign body in the stomach" was made, but a skiagram failed to show anything abnormal. An operation was proposed, but refused by the patient. Six weeks later she suddenly vomited two pints of blood; hæmatemesis continued and she died in a few hours.

Toothplate impacted in the Œsophagus: Death from Perforation into the Pleural Cavity.

Specimen, not yet numbered, from the Museum of the Royal College of Surgeons.[1]

Description.—Posterior view of a portion of the thoracic œsophagus with the aorta attached, removed from a male, aged 57. Showing portion of an upper denture (composed of platinum) with one tooth and two metal hooks embedded in the anterior wall of the œsophagus at the level of the aortic arch. The denture measured 1½ in. in its broadest diameter, and had been accidentally swallowed eight weeks previously. An ulcerated track (shown by glass rod) is seen passing through the œsophageal wall into the right pleural cavity. Two attempts by œsophagoscopy failed to locate the denture, but a third attempt with the fluorescent screen was successful. Attempts at extraction however failed on account of its being securely fixed by the hooks to the œsophageal wall. Death occurred from pyo-pneumothorax and collapse of the lung.

(Mr. Claud Woakes's case.)

[1] This specimen was shown at a meeting of the Section of Laryngology, Royal Society of Medicine, on November 1, 1918. For full notes of the case and illustration of the specimen *see* Woakes, *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.), p. 8.



FIG. 30.

Toothplate impacted in the œsophagus. Death from perforation into the pleural cavity. (Specimen, not yet numbered, from the Museum of the Royal College of Surgeons.)

Halfpenny embedded in the Œsophagus.

Private Specimen exhibited by Mr. W. H. Jewell.[1] *Now in the Museum of the Royal Society of Medicine.*

Description.—Child, aged 1 year and 10 months, was brought to hospital with a history of having swallowed a coin ten days previously. Death occurred on the thirteenth day after the accident with perforation of both sides of the gullet. The mucous membrane of the shallow pouch in which the halfpenny is retained is continuous with that of the œsophagus. Only the larger perforation

[1] This specimen was shown at a meeting of the Section of Laryngology, Royal Society of Medicine, on May 7, 1915; *vide* Jewell, *Proc. Roy. Soc. Med.*, 1915, viii (Sect. Larygol.), p. 108.

with the coin protruding on the posterior aspect of the specimen is seen and was situated just below the sternoclavicular articulation.

The specimen is now in the Museum of the Royal Society of Medicine.



FIG. 31.

Halfpenny embedded in the œsophagus. The dotted circle represents the embedded coin. (Private specimen exhibited by Mr. W. H. Jewell, now in the Museum of the Royal Society of Medicine.)

Bowl of Clay Pipe impacted in the Œsophagus.

Private Specimen exhibited by Mr. Patrick Dempsey.

Description.—Head of clay pipe bitten off stem and swallowed by lunatic. It measured 42 mm. ($1\frac{5}{8}$ in.) in length and 34 mm. ($1\frac{5}{16}$ in.) in diameter. Patient seen by me three days after occurrence suffering from complete obstruction of swallowing. Direct examination showed smooth end of bowl presenting about 8 in. from incisor teeth, and absolutely immovable by instrumentation. Irwin Moore's forceps easily grasped but teeth always cut through the soft clay. Attempts to break up or push downward failed. The following day patient triumphantly produced the pipehead, having passed it that morning at stool. It was subsequently discovered that this was his third similar success, and he is now reluctantly compelled to smoke nothing more satisfying than cigarettes.

This specimen is important as showing the extent to which the œsophagus is capable of dilating and adapting itself to the passage of a foreign body.

The specimen is now in the Museum of the Royal Society of Medicine.



FIG. 32.

Bowl of clay pipe impacted in the œsophagus. The marks on the lower figure represent the marks of the forceps in attempts at extraction. (Exhibited by Mr. Patrick Dempsey. Now in the Museum of the Royal Society of Medicine.) (Natural size.)

Œsophagectasia.

Specimen No. 4004 *from the Museum of University College Hospital.*[1]

Description.—The upper 6 cm. of the œsophagus is cylindrical, the remainder of the tube shows fusiform dilatation, the circumference at the widest part being 16 cm. The lowest 1·5 cm. shows no hypertrophy, but for 4 cm. above this the wall is 7 cm. thick, the hypertrophy becoming less above this.

From a man aged 55, under Dr. Batty Shaw in University College Hospital.

[1] "Supplementary Catalogue (Medical Series) of the Pathological Museum of University College Hospital." This specimen was exhibited at a meeting of the Section of Laryngology, Royal Society of Medicine, on December 6, 1918. For full description and illustration, together with history of the case, see Irwin Moore, "The Pathology of Œsophagectasia," *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.), pp. 75 and 88.

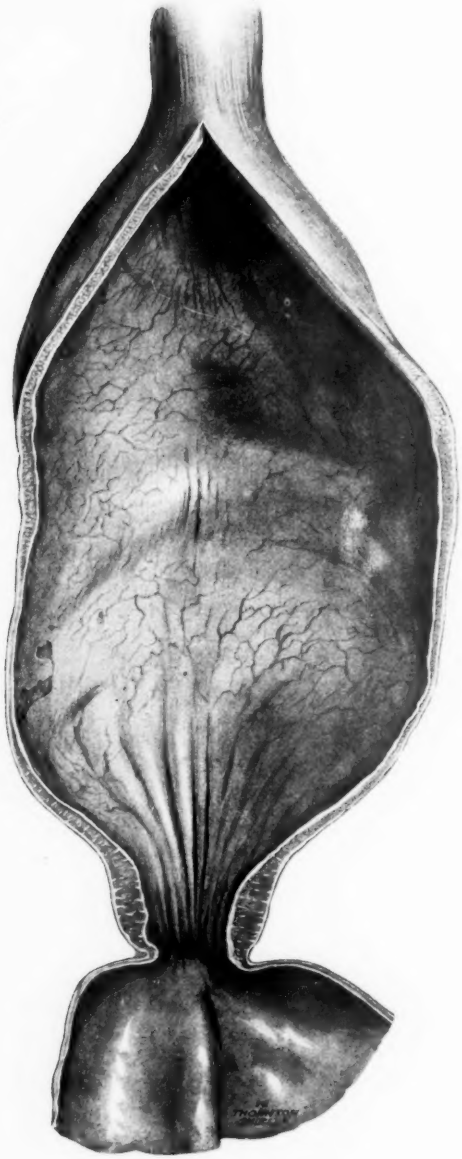


FIG. 33.
Œsophagectasia. (Specimen No. 4004 from the Museum of University College Hospital.) (⅔ natural size.)

Symptoms began eighteen years previously, when after having dinner the patient had a fit of coughing and fell down unconscious; he recovered and felt quite well, but three days later found that he could not swallow properly. Since then patient had had difficulty in keeping down food, off and on, with pain in the chest not related to meals. These symptoms greatly increased during the last few weeks, when there was also shortness of breath and cough. An œsophageal bougie passed readily apparently into the stomach, but a rather small bore œsophageal tube caused immediate discomfort and retching. Vomit varied in amount from 1 to 3 oz. and contained no free hydrochloric acid. Patient wasted rapidly and died in an attack of angina pectoris.

Post mortem the coronary arteries were extremely calcified.

Œsophagectasia.

Specimen No. 552 from the Museum of Guy's Hospital.[1]

(This is Sir Samuel Wilks's well-known case.)

Description.—An œsophagus and stomach removed from patient, aged 74. The œsophagus is greatly dilated. Its walls are of great thickness from the increased muscular development of its coats. It is 11 in. long and measures 6½ in. in external circumference at its widest part. It is of nearly uniform size throughout, although slightly bulging in two parts. Towards the stomach it suddenly contracts. It is here constricted to half its usual diameter. The cardiac orifice is laid open and measures only 1¼ in. transversely. There is no appearance of fibroid thickening and cicatrization of this orifice.

Hypertrophy of the Mucous Membrane of the Œsophagus.

Specimen No. 551 from the Museum of Guy's Hospital.[2]

Description.—Local hypertrophy of the mucous membrane of the œsophagus. An œsophagus laid open to show upon its mucous membrane numerous raised patches, round and oval in shape, and varying from the size of a pin's head to that of a split pea. The nodules have a smooth convex surface, and histologically are seen to be due to local thickenings of the epithelium. The muscular coat at the lower end of the œsophagus is seen to be hypertrophied.

[1] "Descriptive Catalogue of the Pathological Specimens contained in the Museum of Guy's Hospital," 1894, 3rd ed., i, p. 187. This specimen was exhibited at a meeting of the Section of Laryngology, Royal Society of Medicine, December 6, 1918. For full description and illustration, together with history of the case see Irwin Moore, "Pathology of Œsophagectasia," *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.), pp. 72 and 84.

[2] "Descriptive Catalogue of the Pathological Specimens in the Museum of Guy's Hospital," 1894, 3rd ed., i, p. 187. *Vide* also Irwin Moore, "Pathology of Œsophagectasia," *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.), pp. 73 and 85.



FIG. 34.

Œsophagectasia. (Specimen No. 552 from the Museum of Guy's Hospital.)
($\frac{3}{4}$ natural size.)



FIG. 35.

Hypertrophy of the mucous membrane of the œsophagus. (Specimen No. 551 from the Museum of Guy's Hospital.) (¾ natural size.)

Œsophagectasia accompanied with Ulceration.

Specimen No. 553 from the Museum of Guy's Hospital.[1]

Description.—A greatly dilated œsophagus, the circumference of which measures 2½ in. at its upper end and 4 in. just above the cardiac orifice. The mucous membrane of the upper 3 in. of the œsophagus is healthy, whilst below

[1] "Descriptive Catalogue of the Pathological Specimens in the Museum of Guy's Hospital," 1894, i, p. 188.



FIG. 36.

Œsophagectasia accompanied with ulceration. (Specimen No. 553 from the Museum of Guy's Hospital.) (¾ natural size.)

this it is extensively destroyed by numerous ulcers of various sizes, the larger ones exposing the muscular coat. At the upper limit of the ulceration there is a flat wart one third of an inch in diameter. The muscular coat is somewhat hypertrophied, and the lymphatic glands at the bifurcation of the trachea are much enlarged. From a young lady, aged 19, who, from the time she was 9 years old, was noticed to have some peculiarity in her manner of swallowing, the cause of which could not be determined. She, however, enjoyed fair health till within about a year of her death, when signs of œsophageal obstruction occurred and she became emaciated. At the autopsy the stomach was found enormously dilated but apparently healthy. Nothing was found to explain the condition of the œsophagus.

(Presented by Dr. Blasson in 1884.)

Traction Pouch of the Œsophagus.

Specimen No. 1523 *from the Museum of University College Hospital.*[1]

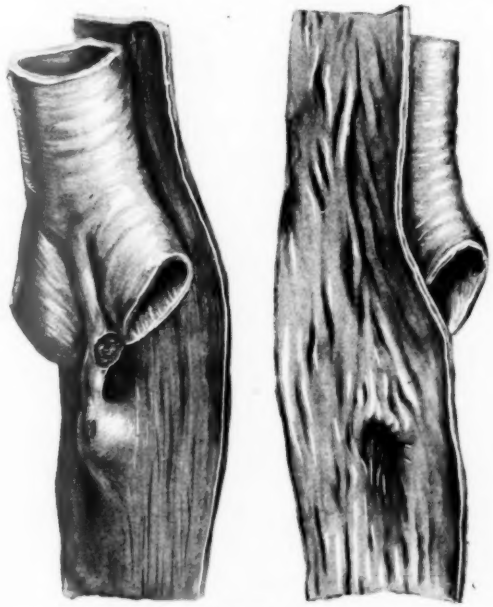


Fig. 37.

Traction pouch of the œsophagus. (Specimen No. 1523 from the Museum of University College Hospital.) (⅓ natural size.)

[1] "Descriptive Catalogue of the Specimens illustrating Surgical Pathology in the Museum of University College, London," new ed., part iii, 1906.

Description.—The pouch is in the anterior wall of the œsophagus, 3 cm. below the bifurcation of the trachea. It is of small size and its opening about 1 cm. in diameter. The interior is lined with normal mucous membrane. In the bifurcation of the trachea is a group of pigmented and calcareous glands, and to the lowest gland is adherent the apex of the conical protrusion of the muscular wall of the œsophagus which forms the pouch. From a man who died in University College Hospital of pulmonary tuberculosis.

BENIGN GROWTHS OF THE ŒSOPHAGUS.

Œsophageal Warts.

Specimen No. 1857A *from the Museum of St. Bartholomew's Hospital.*[1]

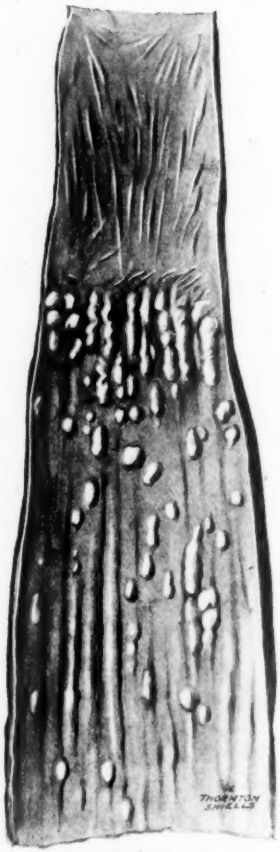


FIG. 38.

Œsophageal warts. (Specimen No. 1857A from the Museum of St. Bartholomew's Hospital.)

[1] "A Descriptive Catalogue of the Anatomical and Pathological Museum of St. Bartholomew's Hospital," 1882.

Description.—On the mucous membrane, which is somewhat thickened, numerous small nodules are seen, which on microscopical examination were found to consist of thickened epithelium and hypertrophied mucous membrane, the submucosa not being involved. From a case of colloid carcinoma of the stomach with secondary growths in the peritoneum.

For other specimens showing warts, *vide* Specimens Nos. 551 and 553 from the Museum of Guy's Hospital.

Fibroma of the Œsophagus.

Specimen No. 1857c *from the Museum of St. Bartholomew's Hospital.*



FIG. 39.

Fibroma of the œsophagus. (Specimen No. 1857c from the Museum of St. Bartholomew's Hospital.) (Natural size.)

Description.—Portion of an œsophagus containing a small fibroma embedded in the muscular coat. The specimen is viewed from its outer surface. Found post mortem in the body of a man, aged 42, who died of chronic interstitial nephritis.

[1] "A Descriptive Catalogue of the Anatomical and Pathological Museum of St. Bartholomew's Hospital," 1882.

Submucous Fibroma of the Œsophagus.

Specimen No. 1016F *from the Museum of Charing Cross Hospital.*[1]

Description.—Unexpectedly discovered at post-mortem on a woman aged 68, who died of strangulated umbilical hernia. The microscope showed that it consisted of dense fibrous tissue.

Myoma of the Œsophagus.

Specimen No. 1857D *from the Museum of St. Bartholomew's Hospital.*

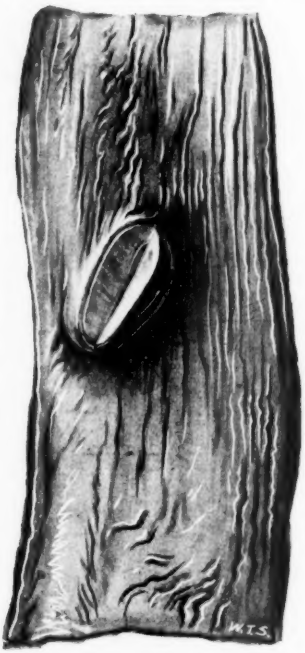


FIG. 40.
Myoma of the œsophagus. (Specimen No. 1857D from the Museum of St. Bartholomew's Hospital.) (Natural size.)

Description.—Part of an œsophagus laid open to show a myoma. The latter forms an oval protuberance on the mucous surface and measures rather less than 1 in. in length. It is encapsuled and, in the recent state, lay immediately beneath the mucosa; but this membrane is now retracted as the result of an incision made for the purposes of microscopic examination, and the tumour is left completely exposed. It is dense and white. Microscopic examination shows that the growth consists of interlacing bundles of unstriped muscle with some fibrous tissue. The muscle cells are very large. The specimen was found

[1] "A Descriptive Catalogue of the Pathological Museum of Charing Cross Hospital," 1902, p. 139.

at the post-mortem examination of a patient whose death had no connexion with the tumour. The record of the case is not known.

Stenosis of the Œsophagus due to a Tumour (probably a Lymphadenoma).

Specimen No. 1858 *from the Museum of St. Bartholomew's Hospital.*[1]



FIG. 41.

Stenosis of the œsophagus. A tumour, presumably a lymphadenoma arising in the mediastinal lymphatic glands. (Specimen No. 1838 from the Museum of St. Bartholomew's Hospital.) ($\frac{4}{5}$ natural size.)

Description.—A tumour (probably lymphadenoma), which completely surrounds the œsophagus, so compressing the tube as it lies behind the division of the trachea that its canal barely admits the quill which is thrust along it.

[1] "A Descriptive Catalogue of the Anatomical and Pathological Museum of St. Bartholomew's Hospital," 1882.



FIG. 42.
Malignant strictures of the œsophagus with Symonds' intubation tube *in situ*.
(From the Museum of Guy's Hospital.) (⅔ natural size.)

MALIGNANT GROWTHS OF THE ŒSOPHAGUS.

Carcinomatous Stricture of the Œsophagus treated by Intubation.

Specimen No. 564 from the Museum of Guy's Hospital.[1]

Description.—An œsophagus with the cardiac end of the stomach showing a stricture which commences 4 in. below the level of the cricoid cartilage, and is about 4 in. in length. A soft rubber tube is seen passing through the stricture and projecting into the stomach. Above the stricture the mucous membrane of the gullet is healthy. Histologically the constriction of the œsophagus is seen to be due to infiltration of its walls with squamous epithelioma. From J. M., aged 40, who was kept alive for eight months in comparative comfort by means of intubation, and eventually died from gangrene of the lung.

This is the original case in which Sir Charters J. Symonds first demonstrated the advantage of his œsophageal feeding tubes. The patient was exhibited at the Clinical Society, London, on January 23, 1885.[2]

(X) STOMACH.

FOREIGN BODIES.

Scarf-pin in the Stomach; Gastroscopy; expelled by Vomiting.

Private Specimen exhibited by Mr. Claud Woakes.[3]



FIG. 43.
(Natural size).

[1] "Descriptive Catalogue of the Pathological Specimens in the Museum of Guy's Hospital," 1894, 3rd ed., i, p. 192.

[2] Vide *Trans. Clin. Soc. Lond.*, 1885, xviii, p. 155.

[3] This specimen was previously shown at a meeting of the Section of Laryngology on November 1, 1918. See *Proc. Roy. Soc. Med.*, 1919, xii (Sect. Laryngol.), p. 10.

Description.—A scarf-pin measuring 53 mm. ($2\frac{1}{16}$ in.) in length, the diameter of its head being 9 mm.—accidentally swallowed by a girl, aged 8, whilst sucking it; vomited up three hours later.

History.—A skiagram showed the pin lying obliquely in the stomach, with the head towards the umbilicus, and the point directed upwards, towards the right axilla. Three hours following the accident an œsophagoscopic tube was passed down to the cardiac orifice of the stomach, but the pin could not be seen. Further examination was prevented by the endoscopic tube becoming blocked with semi-digested cabbage which welled up from the stomach. Whilst arrangements were being made for further treatment the pin was vomited up with a bolus of cabbage.

Section of Laryngology.

President—Mr. E. B. Waggett, D.S.O., M.B.

An Adeno-fibro-myxoma of the Nares and Nasopharynx removed by Lateral Rhinotomy.[1]

By C. Firmin Cuthbert, F.R.C.S.Ed.

(Introduced by Mr. Douglas Harmer.)

W. H., a male, aged 30, was first seen on August 22, 1917, with a tumour protruding from the right anterior naris, occupying also the left anterior naris, and projecting into the nasopharynx. A distinct swelling was noticeable over the right maxillary antrum. Patient complained of intense headache and dyspnœa. For over a year he had spent the nights in sitting in a chair. He has had several severe hæmorrhages.

On August 23 both sides of the neck in line of the external carotids were infiltrated with novocaine, and both external carotids were dissected out, whilst ¼ in. tapes were passed beneath them ready to control them with Crile's clamps. Laryngotomy was next performed under novocaine anæsthesia followed by a general anæsthetic through the laryngotomy tube. The pharynx was plugged. Incisions were made over both supra-orbital ridges and carried down both sides of the nose to the level of the alæ nasi. The flaps were retracted upwards and downwards; the nasal processes of both superior maxillæ cut and the nasal bones were removed, together with the inner part of both lachrymal bones, care being taken not to encroach upon the lachrymal sacs. Thus a modified Moure's operation was carried out giving a good exposure of the nasal cavities. Crile's clamps were next adjusted on the external carotids.

The tumour mass was found to fill up the cavity with extensions into both maxillary antra, both frontal sinuses, whilst a large portion

[1] At a meeting of the Section, held February 6, 1920.

rested against the body of the sphenoid. The tumour was easily shelled out of all these cavities without any hæmorrhage, after which a large mass of the tumour was seen extending into the frontal cavity of the skull and giving a very distinct communicated pulsation from the brain. The mass extending about half way up the frontal bone was easily removed. The dura mater was uninjured so far as could be seen, and there was no escape of cerebro-spinal fluid.

After removal of the growth Crile's clamps were released, and it was only necessary to ligature one or two vessels. The cavity was packed with a long strip of gauze passed out through each anterior naris. The soft parts were replaced and co-apted by sutures. The laryngotomy tube was retained until after the packing had been removed two days later, and after that the cavity was douched with peroxide of hydrogen.

On September 6 the patient returned home apparently well.

Microscopical Report.—An adeno-fibro-myxoma, with no evidence of malignancy. (Clinical Research.)

A photograph of the patient, also the tumour mounted in formalin jelly, are exhibited.

Exophthalmic Goitre: Thyroidectomy.

By C. Firmin Cuthbert, F.R.C.S.Ed.

P. C., a male, aged 19, was admitted to hospital on May 6, 1912, suffering from enlarged thyroid, polyuria, with intense thirst—no sugar, profuse sweating and diarrhœa. He was 6 ft. 1 in. in height, and weighed 9 st. 4 lb., having lost 4 st. during the last two years. No leucopænia or lymphocytosis (Kocher). He was discharged from the Army in 1911, on account of palpitation, and in May, 1912, the late Sir Victor Horsley advised immediate thyroid operation. On June 3 removal was carried out under novocaine and adrenalin anæsthesia. The blood-pressure was taken at frequent intervals and saline infiltration continued all the time of the operation. The pulse-rate during the operation varied between 180 and 200.

The patient progressed well after operation and comparative recession of the eyeballs followed. In December, 1913, patient wrote that he felt very well, and a photograph he sent showed that he looked very well.

For two years, from November, 1915, to November, 1917, when he was killed, he served a very active life as a lieutenant on active service in France. Previous to rejoining the Army he had grown into a very fine fellow and his weight had reached 13 st.

Exhibit: A photograph of the patient is shown, also specimen of the removed thyroid mounted in formalin jelly. It looks especially velvety and vascular.

DISCUSSION.

Mr. TILLEY: In the first case there was obviously a very extensive tumour, and the risk of removal was considerable. Where does Mr. Cuthbert consider the tumour had its origin? He says a "portion rested against the body of the sphenoid." Does that mean that it grew from the basi-sphenoid? As a rule these tumours have a very definite adherence to the part from which they take origin, and it requires considerable force to separate their attachments. On many occasions I have shown a large fibro-angioma which I found grew from the posterior wall of the left maxillary antrum. The older text-books of surgery state that these tumours grow from the basi-sphenoid, but generally their origin is from the ethmoidal region. The surgeon can get splendid access to these tumours without performing lateral rhinotomy—namely, by turning up the upper lip and cheek and removing the ascending maxillary process. That means that the patient must either be laryngotomized or else have the anæsthetic administered by the direct method, with the tube in the trachea in order that the mouth may be closed and so permit the upper lip to be raised. The operation will be easier if the surgeon cuts through the lower part of the septum. By such a method there is no external scar. However well the unilateral Moure's operation may be done, unless the patient is old and has a natural fold in the skin, a scar is visible afterwards. The operation I have outlined does not necessitate exposing either external carotid.

Sir WILLIAM MILLIGAN: In the first case, did Mr. Cuthbert, instead of performing laryngotomy, which I think was unnecessary, think of using a Kühn's peroral intubation tube? Kühn's tube is valuable in those cases. The tube is easily passed and saves a further cutting operation: the tube is also easily withdrawn. The exophthalmic goitre case is particularly interesting, and the photograph is remarkable. In one of the cases I understand Mr. Cuthbert performed ligature of the superior thyroid arteries, but nothing was done to the thyroid itself. That raises a very important point, because we know there is considerable risk in any long and extensive removal of the thyroid gland in an exophthalmic goitre case. Here, however, the result of the ligation is surprisingly good. I have had experience of only one case of the same kind. Some years ago I did that operation, but the result was not as good as in Mr. Cuthbert's case. Is Mr. Cuthbert in the habit of ligaturing the superior thyroid arteries in preference to removing the thyroid altogether?

Mr. W. STUART-LOW: I have had a number of cases similar to the first case and have shown some of them here: extensive tumours which have infiltrated up into the ethmoidal and encroached on the frontal region. I agree with Mr. Tilley that the route through the antrum is an exceedingly good one, but I seldom perform laryngotomy, the hæmorrhage encountered having always been controlled by pressure. With the patient's head well raised it is marvellous what one can do by the antral route and how well the operator can see the whole region of operation. The procedure in this case seems to me to have been too severe for the slight risk of hæmorrhage. If others less skilful had attempted the same they might not have had such a happy result.

Mr. E. D. D. DAVIS: I congratulate Mr. Cuthbert on having controlled both external carotids. Owing to free anastomosis, I think controlling of one carotid only in the case of a growth of the maxilla does not make any appreciable difference to the amount of hæmorrhage. We have been taught to attack the bleeding point, and it is not worth the time spent and the risk of a septic wound to ligature a vessel at a distance. Ligature of both carotids may be a help. During the war one saw many cases of injury to the face and jaws in which it was scarcely ever necessary to tie a carotid artery. In secondary hæmorrhage from those injuries the external carotid artery was ligatured as a forlorn hope in a few cases, but with no result. It is difficult to tell whether the hæmorrhage is more or less during an operation, because it is purely a matter of impression influenced by the fact that a large artery has been ligatured. I prefer to attack the bleeding point to ligature at a distance.

Dr. WILLIAM HILL: Mr. E. D. D. Davis suggests that the carotid arteries in this case were ligatured. This was not so, Crile's clamps were put on, and were afterwards released, a very much better proceeding I think. I believe Mr. Trotter sometimes uses temporary clamps instead of ligaturing.

Dr. P. WATSON-WILLIAMS: While congratulating Mr. Cuthbert on his excellent result in a good operation, I think the point Dr. Hill has emphasized is a very important one—namely, the use of Crile's clamps as a temporary ligature of the external carotid, temporarily to occlude the circulation in that artery. Yet I should be sorry if it were to go forth as accepted that ligation of one external carotid was never of use—e.g., ligation before operation for removal of malignant growths of one tonsil has had marked inhibiting effect on the bleeding in some of my cases. In a case of persistent sphenoidal sinus hæmorrhage, ligation of one external carotid saved the patient. He had been repeatedly bleeding, and would have died, but hæmorrhage ceased after ligation of the corresponding external carotid. If we were to accept the idea that ligation of one carotid was useless we might neglect a very useful means of saving some desperate cases.

Mr. DOUGLAS HARMER: We should remember that in some of these cases the tumour proves to be a fibro-angioma; the literature of such cases proves that they are extremely vascular, and that operations have often

been fatal, solely on account of the hæmorrhage. Therefore in any tumour of this region, unless one is certain that it is not of a vascular type, I agree with Mr. Cuthbert it is a safer procedure temporarily to ligature the external carotids. Some years ago I reported a tumour of this size which sprang from the basi-sphenoid and from part of the ethmoid immediately in front of it. It had a fairly large pedicle attached to the base of the skull, and to get it away the bone had to be removed, leaving an opening in which the dura mater was exposed. The tumour was a fibro-angioma, and the man wrote to me regularly for about seven years to say he had had no recurrence, and I expect that is still true or he would have told me. I do not think the scar matters in the least, and the route of approach depends on the size of the tumour. It is essential to secure free exposure, and to be certain that all the growth has been removed.

Mr. C. W. M. HOPE: With regard to hæmorrhage, I removed a large naso-pharyngeal tumour which had been operated on before abroad. Microscopical examination of a portion before removal showed it to be a fibro-angioma, with enormous blood spaces. I tied both external carotids before attacking it, but the hæmorrhage was so profuse that I had to stop and do transfusion. I had to wait a fortnight before completing the operation.

Mr. G. W. DAWSON: I have never seen large hæmorrhage from a tumour in this region unless it has been an angio-fibroma. Sarcomata in this locality seem to bleed very little.

Dr. IRWIN MOORE: Why does Mr. Cuthbert call this a nasopharyngeal tumour, as he reports that the main mass of the tumour was in the nares and accessory sinuses and a portion only *projected* into the nasopharynx? It is important, for indexing purposes, that tumours should be named according to their origin. I suggest that the title of this case should be altered to adeno-fibro-myxoma arising in the nares and extending to the nasopharynx. Recently I have looked up the literature of adenomatous growths of the larynx, and the present case is a unique one. A pure adenoma of the nares is practically a histological impossibility except at the nasal orifice. A few cases only of mixed tumours have been reported. Some members have referred to angio-fibromata, but this is an adeno-fibro-myxoma originating from adenomatous tissue, probably from the middle turbinal or ethmoid region, the chief site of such growths. The only other similar case I have seen recorded is one by Emyl Mayer,[1] of New York, in 1902, and the microscopical report was the same as the case now reported. No ligaturing of the carotids in this case was carried out nor were Crile's clamps used. There was practically no hæmorrhage, and it is not usual in growths of this character. Though with benign growths we need not expect much bleeding, it is well to be on the safe side and adopt the measures carried out by Mr. Cuthbert.

[1] *Trans. Amer. Laryngol. Assoc.*, 1902, p. 80.

Mr. CUTHBERT (in reply): In answer to Mr. Tilley, this growth was protruding from the nares, and extending down the pharynx. The antra were not translucent: therefore I performed a bilateral Moure's operation. The exposure was ample when I had removed it from the antrum and pharynx; some part seemed to originate in the ethmoid, as the cribriform plate was destroyed. There was a mass in the anterior fossa of the skull, which, I thought, bore a communicated pulsation: it looked thick and dull, and not like dura mater. Sir William Milligan suggests a Kühn's tube. I use a laryngotomy tube of about 18 in. in length. Neither external carotid was ligatured in this case, but the patient was infiltrated on each side in the line of the external carotids and ¼ in. tape put under them as a sling previously to applying the Crile clamp. I called it a nasopharyngeal tumour because it inhabited both nares and nasal region of the pharynx. My interest in the case was revived by reading, in the *Proceedings* of the Section of Laryngology, August, 1919, a case of angiofibroma brought up by Mr. Dawson, in which Mr. Harmer referred to an operation on a similar case sent to him some years ago by Sir Henry Butlin. I saw Mr. Harmer operate, and Sir Henry Butlin told me that all cases of this kind (which bleed from their mucous surface, like some of these benign tumours) operated upon before 1909, died of hæmorrhage, either during or shortly after operation. I did not have the difficulty in controlling the carotids which Mr. Harmer had: in his case the left common carotid divided very high up. After the removal in my case there was no bleeding when the clamps were relaxed. The rapidity of recession of the exophthalmos in my second case was remarkable, for within a fortnight it was hardly noticeable. I should here like to raise the question of tying the superior thyroid arteries, and as to the percentage of cases which are permanently benefited by the tying of these arteries instead of removing the thyroid itself.

Pharyngeal Diverticuli; Report of Two Cases.

By HORACE LAW, F.R.C.S.Irel.

BOTH these cases came to me about the same time in February, 1919, and each presented points of similarity, but the outstanding feature of their story was the increased salivation and the difficulty of disposing of the saliva. This was evidently a reflex from the œsophagus due to irritation in that tube, as, in the one case the removal of the pouch, and in the second case the removal of the foreign body cured this annoying symptom. I would like to emphasize this point in the case of abnormal salivation sometimes complained of by elderly persons, and to suggest that œsophageal lesions should be considered, though no other sign may point in that direction.

My two cases both occurred in elderly people. Both had their

opening just at the lower border of the cricoid cartilage. In the case of the man the distance was 7 in. from his front teeth; in that of the woman 6 in., as measured by a bougie when it caught in the pouch. In the man's case the opening was in the posterior surface and somewhat to the left. In the woman's it was exactly posterior.

Case I.—Male, aged 53. He always had enjoyed good health, and never had any particular trouble in his throat till recently. The first abnormal sign he could recall was about sixteen months ago, when he noticed a slight gurgling in the throat after swallowing liquids, and saliva collecting in his mouth in undue quantities, which seemed to be difficult to swallow and get rid of. There was never any pain. He had two or three attacks of choking when swallowing pieces of meat or anything of hard nature, and had to be very careful to masticate thoroughly, and only swallow small pieces. As these symptoms increased he came up and consulted me. I passed bougies, which all stopped at 7 in. from the teeth, and then the œsophagoscope, which similarly was caught by the pouch. Major Harvey then kindly X-rayed him, and the barium meal was most instructive. Each mouthful was caught in the pouch, which became distended, and then on repeated swallowing the pouch was gradually emptied, and we could follow the course of the barium up out of the pouch and then on down the œsophagus, so that this examination rendered the provisional diagnosis certain. I admitted him to hospital and operated on February 21. The main difficulty in the operation was to find the pouch, as after we had reached the œsophagus it was not visible, being quite empty and collapsed. By passing a bougie into it, it was easily identified and separated from the surrounding structures. The walls were so thin and the position so deep in the neck that it was not possible to sew it up by layers, and one had to be content with transfixion, double ligature and excision. A drainage tube was inserted right down to the ligatured stump, and the wound closed. The wound healed by first intention, and, as was natural, he had some difficulty in swallowing, which gradually moderated, and after ten days he was able to take solid food without much pain. He got up on the thirteenth day, and on the fifteenth day was able to eat fish, pudding and bread. He left hospital on the eighteenth day after operation. Since leaving he developed a sore throat, and had some difficulty in swallowing, but by putting him on slops again this gradually passed off, and he swallows without discomfort. In November he reported himself as practically normal.

Case II.—Lady, aged 70. The sole symptom that brought her to me was the fact that for seven months saliva was constantly collecting in her mouth, which she found difficult to swallow and get rid of with comfort. Having just seen the other case with a similar symptom I was naturally inclined to the feeling that this was another pouch, and this was rather borne out by the fact that she told me her mother had suffered from one in the latter years of her life. I passed bougies, which stopped at 6 in. from the teeth. I then put down the œsophagoscope, which at once encountered a hard mass which was greyish in appearance, with some red points, and gave one the impression of a fungating tumour. All my instruments stopped dead and did not go any farther down. I removed two pieces from this mass and told the patient I would communicate with her when they had been examined, indicating that it was probably a serious trouble. Naturally these appearances entirely put me off the track of œsophageal pouch. The microscopical examination made by Captain Speares proved the piece removed to be cabbage, so I at once passed the œsophagoscope again, when I found that I could get it easily past the former obstruction. Investigation proved that there was a mass of granulation tissue on the posterior wall of the œsophagus, with some débris still present. The latter I thoroughly removed and cleared up through the œsophagoscope, and she was sent away to rest for a fortnight. At the end of that time the third œsophageal examination showed a slit about ½ in. long in the posterior wall, without any breach of surface or granulations, and a probe passed about ½ in. into it, which would be the measure of the depth of the pouch. Her symptoms were greatly relieved after the first examination by the œsophagoscope, and steadily improved, till at the last examination she expressed herself as quite well. Under these conditions, and on account of the smallness of the pouch, I decided that no operative interference was necessary at present. The only explanation I can give of this case is that a foreign body of a semi-hard nature, probably a piece of the stalk of a cabbage, lodged in the pouch and was not sharp enough to pierce the walls but its constant pressure set up a growth of granulation tissue, which gave the appearance of a fungating cancer. The forceps evidently dislodged this mass while a piece was taken off it for microscopic examination, and hence the immediate improvement and cure. How a patient could have a foreign body stuck in the small pouch in the œsophagus for seven months without suffering more pain and discomfort than this lady did is hard to conceive. In November she reported herself as normal.

Exhibit: Section of the foreign body, also lantern slide were shown.

DISCUSSION.

Dr. KELSON: The result in these remarkable cases is excellent. I would like to hear the size of the pouch in the first case; it seems to have been very small. Mr. Law says that in both cases he passed a bougie, and afterwards the œsophagoscope: is not that putting the cart before the horse? Again, he says that the difficulty at the operation was to find the pouch. In all these cases I think it is most important that a guide should be passed either before or immediately after the anæsthetic is given, because many of these patients are in bad condition, and much unnecessary time is wasted in trying to find the pouch without a bougie. Was there any leakage of the œsophagus in the first case?

Dr. W. HILL: I agree that in some of these cases there is a very thin sac wall, and, unless the patient is too emaciated, it is a great advantage to try and preserve some of the areolar tissue on the true coats of the pouch when diverticulo-pexy is performed. Then one can put the stitches, not through the thin pouch wall, but through the areolar tissue. The note says: "A drainage tube was inserted right down to the ligatured stump, and the wound closed." Was the tube buried in the neck and the wound closed, or was the wound closed except that part at which the tube emerged? And did the whole of the wound really heal, as stated, by first intention? And, considering there was just the tying the mouth of the sac instead of sewing it up, was there any leakage? Also, why was a drainage tube put in unless it was thought there would be leakage? The method I have adopted—i.e., diverticulo-pexy—in two cases is to dissect the pouch out free and instead of cutting it out fix it high up in the neck with stitches in order to avoid the chance of leakage and mediastinitis. Nothing is said in regard to the second case as to the X-ray findings as to whether it was a pouch or not. The measurements and the description given do not appeal to me; there may have been only a dilated pharynx. A skiagram would have settled the whole thing.

Dr. IRWIN MOORE: The feature of increased salivation and difficulty in disposing of it is one connected with either diverticula of the pharynx, malignant growths or strictures. Hence Mr. Law's remarks in these cases, emphasizing the importance of regarding increased salivation in elderly people as an indication of an œsophageal lesion are important. If there were closer co-operation between the general physician and the laryngologist in cases of abnormal salivation, we should see these cases earlier, when the pouches are not so dilated as to require removal. I have two female patients, aged 64 and 78, one first seen in 1912, the other in 1916, each with a small early pouch, which I diagnosed. No operation was advised. The patients were recommended to eat soft food, to masticate it well in the mouth before swallowing, to eat slowly, and take small meals at frequent intervals. The

latest report is that they are no worse than when they originally saw me. We do not habitually examine these cases first with the œsophagoscope, on account of the extreme thinness of the wall in many of the cases, and the danger that the least pressure may send the œsophagoscope through into the posterior mediastinum. It is a question whether there is any need to pass the œsophagoscope in these cases, in view of the present perfection of X-ray work. The second case is evidently an acute diverticulum caused by the foreign body. It does not follow that there was a commencing diverticulum present which was the cause of the impaction of the lump of vegetable. I have come across the report of a case by Hoffmann,[1] in which an acute diverticulum was caused by swallowing a piece of broken china. At the autopsy a diverticulum of the pharynx was found lined with mucous membrane, with the foreign body inside.

Dr. DUNDAS GRANT: I would remind the Section of a case, which I described some years ago, in which the symptoms of a pouch were present in an elderly woman, the chief one being that food returned which was known to have been swallowed several hours before. X-rays showed only an attempt at pouching. The symptoms disappeared after a large bougie had been passed through the mouth of the œsophagus two or three times. Probably Mr. Law's second case is much of this kind. It is important that the skiagram should be taken without much delay: it should precede any attempt to pass an œsophagoscope. With regard to increased salivation, we have often met with cases of so-called water-brash, in which large quantities of fluid are brought up, as if by vomiting. It is found to be alkaline, and this is probably due, in chief part, to saliva collecting above a spasmodic contraction of some part of the œsophagus. In such cases it might often be good practice to pass an œsophageal tube so as to coax the spasm out of existence, and allow the fluid to pass into the stomach.

Dr. W. HILL: I think a further report ought to be made on this second case, as it may be traumatic. It may be a unique case of traumatic diverticulitis, or it may be merely a dilatation of the lower part of the pharynx, with a foreign body in it.

Mr. HORACE LAW (in reply): There was no leaking in the first case: it went on without further complication. The tube was put in as a precaution, and it projected through the skin, and after forty-eight hours was removed, and the wound allowed to heal. These were private patients and it is not always easy to get them to submit to X-ray examination unless one can give a good reason for it. The symptoms were not typical of a pouch in the ordinary way at the beginning. In the second case, X-rays would have shown nothing but some of the barium or bismuth remaining behind near where the foreign body was. I agree with Dr. Irwin Moore as to the danger of using the œsophagoscope, and I

[1] *Deutsch. med. Wochenschr.*, No. 19, 1889; Abstract: *Journ. Laryngol., Rhinol., and Otol.*, 1889, iii, p. 293.

appreciated that much more after I took the pouch out. It should be possible to avoid passing instruments in these cases, but it is not easy to know where one's diagnosis begins. I had not diagnosed the condition until an instrument had been in the pouch. I do not know whether the second case should be called acute: she had symptoms seven months, and the foreign body must have been present for some time, as the picture showed a grey mass surrounded by granulation tissue. What you saw on the screen was supposed to be the tumour, but it turned out to be cabbage: it was a long time before my friends at Trinity College accepted the fact. Later, one could see the site where it had been: it was still ulcerated, and there were granulations round it. I did not see the use of employing X-rays when I had reached that stage. I shall be glad to send a further report if I can get the old lady to come up again.

Perithelioma (Alveolar Sarcoma) of the Frontal Bone.

By H. Lawson Whale, F.R.C.S.

F. W., a male, aged 54, was admitted to the London Temperance Hospital on August 16, 1919, with a hard swelling in the region of the left eyebrow, and the globe displaced outwards and downwards. The onset of this condition had extended over three years. More recently the cornea had become ulcerated. Ocular movements were limited outwards. [*See* photographs.] Nasal examination was negative, and skiagraphy inconclusive.

Operation, August 20, 1919: Access through a double Killian frontal sinus incision; on the left side the whole eyebrow was incised; on the right only the inner half. The growth was followed round the left supra-orbital margin along the orbital roof, to within 1 in. of the sphenoidal fissure. The supra-orbital fat was removed intact with the growth, leaving this aspect of Tenon's capsule bare and clean. The growth had eroded the outer wall of the frontal sinus for an area of 2 sq. cm., but no perforation into the sinus existed. All this part of the sinus-wall was freely removed, a Killian bridge being left. The frontal sinus was now discovered to be full of polypi; accordingly the anterior part of the middle turbinate was removed, the infundibulum burred out, and the sinus drained by a tube through the nose.

Convalescence was uneventful, but the corneal ulceration was progressive, and hypopyon appeared. On September 2 Mr. Loosely eviscerated the globe. Recovery uneventful.

Dr. Sanguinetti considers the growth is an alveolar sarcoma, ? perithelioma.

Photographs and a microscopic slide are shown.

Case of Rhinoplasty.

By H. Lawson Whale, F.R.C.S.

The photographs illustrate what has been done. The nose was re-made three years ago, in France, and the patient has kindly come up from Liverpool expressly to show himself.

DISCUSSION.

Dr. W. Hill: Mr. Whale has secured a splendid cosmetic result: no one could wish for a better.

Mr. Lawson Whale (in reply): This case has stood the test of time. It is four years since I carried out the operation. The bridge of the nose has kept its shape. I think his forehead ought to be improved: that will be an easy matter.

Malignant Disease of Upper Jaw; Lateral Rhinotomy (Moure's Operation).

By C. W. M. Hope, F.R.C.S.

W. S., male, aged 49, was sent to me from the casualty department at King's College Hospital on July 24, 1919, with marked swelling of the left maxillary region, and a fluctuating area pointing just below the outer canthus; a sinus also led from the alveolus into the antrum. The patient gave the history of having had an upper molar on that side extracted a week previously; and that the swelling had come on since. The left antrum was dull on transillumination. The same afternoon an incision was made over the pointing area, and on putting in my finger I found the antrum was filled with a soft mass, apparently growth; much pus also escaped. The microscopic report of pieces removed showed the growth to be an epithelioma. A section cut by Dr. Emery is shown.

On August 13, 1919, under intratracheal ether anæsthesia, Moure's operation was performed, after the external carotid artery on the affected side had been ligatured. The growth was found to have originated apparently in the ethmoidal region, and to have involved not only the ethmoid and antrum, but also the bony orbital floor, the

malar bone and the soft tissues in the pterygoid region. It was removed as completely as possible without taking into account the anatomy of the parts. A drainage tube was left in for two days, and then removed. Hæmorrhage was practically *nil*, and the patient made an uninterrupted recovery.

Radium or X-ray treatment has since been given from time to time. Up to now (January 14, 1920) there has been no sign of recurrence. The removal of the malar bone has led to a good deal of deformity, otherwise the patient is in good health and is carrying on his ordinary work.

This case has been shown mainly to demonstrate the advantage of this operation over the older anatomical removal of the upper jaw in growths in this region.

DISCUSSION.

Sir StClair Thomson: A fortnight ago I had a case in which I did this operation, and I have brought the specimens for exhibition by the epidiascope. It shows what good access can be got by this operation in malignant disease of the nose and accessory sinuses.

Dr. D. R. Paterson: I would make a practical point in connexion with this operation—namely, that the patient's permission should be obtained beforehand to sacrifice the eye if it should be found necessary to do so. One may find the trouble has spread behind the eye. I have done this operation many times: it was introduced to me by Sir StClair Thomson, and I do not think there is any operation better adapted for removing these growths.

Mr. G. W. Dawson: I do not think epitheliomata and sarcomata about the face have the same degree of malignancy as those in other parts of the body, and after removal the patients often remain well.

Laryngeal Case for Diagnosis.

By Andrew Wylie, M.D.

Patient, female, aged 47, consulted me in July, 1919, complaining of slight difficulty in swallowing for fifteen years. This difficulty has increased during the last twelve months. Patient is anæmic and has recently lost weight through want of solid food. There is no hoarseness, no cough and no spitting. Examination of chest reveals nothing. Wassermann test negative. Epiglottis and vocal cords are normal, but

there is swelling and œdema of both arytænoids and in the posterior region there is an ulcerated spot. The condition has continued almost the same since July except that the ulceration seems to be deeper and larger. The ulceration appears to be of a tubercular nature, but there are no other symptoms of that disease.

DISCUSSION.

Sir WILLIAM MILLIGAN: I do not think this is correctly described. It does not seem to be a laryngeal case, but malignant disease of the post-cricoidal or hypopharyngeal region, with, probably some perichondrial affection. The larynx and the movements of the vocal cords seemed normal. I draw Dr. Wylie's attention to this, and ask him if he thinks the case is correctly described.

Mr. TILLEY: I agree with Sir William Milligan. I do not think there can be any doubt about the diagnosis. I think the more one sees of these post-cricoidal cancers, the more one notices that these patients have had more or less long-continued histories of some difficulty in swallowing. I think there is nothing to be done for this patient from a surgical point of view.

Mr. LIONEL COLLEDGE: This patient has some very hard enlarged glands on both sides of the neck.

Dr. D. R. PATERSON: This case illustrates the subject of a paper I read before the annual meeting, in which I described a clinical type of dysphagia with a long history of difficulty of swallowing. Such patients have a smooth glossy tongue, which is also patchy, with cracked angles of the mouth and changes in the mucosa of the pharynx and hypopharynx. In the case under discussion there is a fifteen years' history of difficulty in swallowing. The clinical appearances in the mouth and pharynx I have described, and the termination in malignant disease in the upper part of the gullet, a not unusual occurrence. After my paper appeared, Dr. Guthrie, of Liverpool, stated in a letter in the *Journal of Laryngology* that this sequence was a very common one in his experience.

Actinomycosis (?) of Tonsil.

By COUBRO POTTER, M.D., and MARRIAN PERRY, Major R.A.M.C.
(introduced by Mr. COUBRO POTTER).

THESE infections have commonly been termed "actinomycosis" on the assumption that all infections of this type were caused by *Actinomycosis bovis* of Ballinger; in point of fact this particular species of

streptothrix is an unusual cause of human infection which arises through invasion with other species of the Streptothricæ.

Gunner G., driver in the Royal Field Artillery, reported sick on October 30, 1919, at Catterick, complaining of ulcerated throat. He was treated for tonsillitis. Swab taken: no Klebs-Loeffler bacillus. Wassermann reaction negative. No evidence of any sore on genitals; no glands, no skin lesion. Deep ulceration of left tonsil, and left side of uvula, with loss of tissue. No Vincent's bacillus present.

Later, January 8, 1920, he was sent to another hospital where the submaxillary glands were noted as enlarged. The ulcer on the tonsil was here noted as sloughing with indurated edges. Sarcoma of tonsil was suspected. The patient was transferred to Millbank.

February 2, 1920: A small piece of the left tonsil was examined by Major Marrian Perry, R.A.M.C., and was found to contain the streptothrix.

When I first saw the case the tonsils were both enlarged, the left being twice the size of the right; it was deeply ulcerated, and this ulceration extended on to the anterior pillar of the fauces. There was a swelling in the middle line between the tonsils; this, I think, is an enlargement of the post-pharyngeal gland, probably a direct extension of the disease from the tonsil. A piece of this has not yet been examined under the microscope. The submaxillary glands are enlarged. No correct diagnosis could be made from the clinical appearances. The man stated that he always kept a piece of straw in his mouth while driving. Large doses of potassium iodide have been given for the last ten days, but no great improvement has taken place.

The case will be shown again with fuller notes, and a coloured photograph of the slide, and a coloured drawing of the throat will be exhibited.

Pathological Report by Major Marrian Perry, R.A.M.C.—A small portion of tonsillar tissue was submitted for examination as to epitheliomatous or sarcomatous change. Sections showed the typical colonies of a streptothrix organism, the tangled and radiating arrangement of the mycelial threads being particularly evident. These colonies were situated in the superficial layers of the tissue, showed evidence of degeneration and necrosis in their neighbourhood. In the deeper layers of the section the ray-like appearance of the colonies had been lost, the parasite in this situation assuming the hyphal and "bacillary" forms which originate from the mycelium. The whole of the tonsillar tissue exhibited the pathological changes associated with a chronic

inflammation. It is impossible, on morphological grounds alone, to determine the exact species of streptothrix responsible for the infection, cultural tests with this end in view have to be carried out. Bacteriological examination of swabs from the affected tonsil demonstrate the presence of various organisms arising as a result of secondary septic infection, *Bacillus fusiformis* and Vincent's spirillum being predominant in films.

DISCUSSION.

Mr. TILLEY: Have large doses of iodide of potassium been given, or are they to be given, or what other treatment is proposed for the patient?

Dr. HILL: Dr. Potter told me he wanted to enucleate the tonsil to-day, and it is because the patient refused that operation that he is able to show him. He hopes to succeed in getting the patient's consent for removing it, and then he will give him iodide of potassium.

Case of Lingual Cancer in a Man, aged 65, treated with Copper Alanine.

By JAMES DONELAN, M.B.

SHOWN at previous meetings. The progress of the disease appears to be arrested at present.

DISCUSSION.

Mr. G. W. DAWSON: I have given 2 c.c. of copper alanine; in some cases there was a good deal of benefit, in others none.

Dr. DAN MCKENZIE: We have several cases under treatment by this method, and I have been struck by the cleaning up of the malignant ulcers by it. It occurs to me that in this and even in the diathermy treatment, much of the benefit received is due to the sterilizing of the tumour of septic organisms. I am not speaking about operable cases, where diathermy is used to excise the whole of a growth or ulcer, but of inoperable cancers, where the ulcer and growth are attacked directly with diathermy without any hope of radical removal. The malignant area then undergoes a remarkable reduction in swelling and the pain is reduced, but several weeks later the swelling returns again, and suddenly. What happens, I suggest, is that the diathermy sterilizes the disease area, and that it becomes again infected. It is probably the same effect that we see resulting from the copper treatment. I do not think that the cancerous growth itself is influenced by this treatment.

Mr. DOUGLAS HARMER: I think Dr. McKenzie is right. I believe cancer cells can proliferate much more quickly in the presence of sepsis, and that this is true not only of primary growths in the throat, but also of glands in the neck. If you destroy a growth in the mouth, there is evidence that the glands in the neck do not progress at anything like the same rate.

Mr. W. STUART-LOW: I have always emphasized the great importance of attending to the asepsis of the mouth and throat. We know how horribly septic these mouths are, and the first thing I do is to have the teeth put right or taken out, and to use antiseptics assiduously. Cleanliness of the mouth and throat assists the throat condition in a marked manner.

Dr. DONELAN: The credit of recommending copper alanine is due to Dr. Shaw-Mackenzie, who has been working for many years at this subject.

Case of Lupus of the Tongue and Larynx.

By T. B. LAYTON, F.R.C.S.

A. S., AGED 20, was first seen at a Pensions Board, where he stated his throat had become bad after being slightly gassed in January, 1918. Further inquiry elicited the fact that he had had symptoms as early as May, 1917. When first seen the condition was much as it is now. Wassermann's test was negative. Dr. G. Marshall could give no evidence of tuberculosis of the lungs. A small piece removed from the tongue under cocaine anæsthesia showed tuberculous granulation tissue.

Microscopical section shown.

Case of Atrophic Catarrh of the Trachea.

By LIONEL COLLEDGE, F.R.C.S.

PATIENT is a male, aged 36. He complains of slight hoarseness and discomfort in the throat. Examination shows that the posterior wall of the trachea is lined with dark crusts from the subglottic region down to the bifurcation, which cannot at times be seen easily. Patient has been under observation for three months, and shows very little improvement under treatment with expectorants and iodide of potassium. No crusting has been observed in the nose or nasopharynx or on the vocal cords at repeated examinations. The larynx is somewhat congested.

Intratracheal injections of menthol in paraffin are suggested, unless any member can propose some other form of treatment. Wassermann's reaction is negative.

DISCUSSION.

Mr. STUART-LOW: This reminds me of a case which was shown here by Sir Felix Semon in which very large crusts were present. The patient died during an attack of dyspnœa. Dr. Lack has also shown similar cases here. I have had two such cases under treatment for years; they are both much benefited by treatment, but are still subject to recurrence. It is vital to the successful treatment of this man's case that he should be got out of the dusty environment in which he works, as the accumulations are always accentuated if dusty material be inhaled and the patient works in a dry atmosphere. I strongly recommend elixoid mucin solution which should be sprayed on the pharynx and larynx, or better still, painted directly on the affected parts. I have found this most effective in the amelioration of this singular condition which under other treatment I have found intractable. Menthol is not good, as it has a desiccating effect. I object to the nomenclature, "atrophic catarrh." There are such conditions as serous catarrh, mucous catarrh, and purulent catarrh, but the term "atrophic catarrh" has no real meaning. The study of this subject so impressed me that in my book, "Mucous Membranes" I made a new classification and nomenclature including such conditions as I put under the heading of "paramyxia," thus indicating an alteration in the quality of the mucus, which is rendered so adhesive that, instead of moving on in the usual slimy condition, it remains and adheres to the surface of the mucous membrane and accumulates in masses. This is the essential pathological change.

Mr. J. F. O'MALLEY: If any drug is going to be used, I would recommend small doses of vinum ammoniale. It causes the mucous membrane to sweat, by stimulating the secretion of the glandular apparatus.

Dr. DAN MCKENZIE: I also have invented a new name, the simple one, "dryness." You can, if you wish, make it more impressive by saying "xerasia" (as, for example, "rhinoxerasia"). It covers all these conditions, and does not commit to any theory of causation.

Dr. KELSON: I think this case differs from those spoken of by Mr. Stuart-Low. In those cases there were big crusts, and one patient died asphyxiated because a large crust dropped down and blocked the trachea.

Dr. IRWIN MOORE: I looked up the cases which have been reported, and amongst them I find one recorded by Lambert Lack[1] and another by Mollison.[2] The latter described his case as "tracheitis sicca," which seems to me a good

[1] *Proc. Roy. Soc. Med.*, 1916, ix (Sect. Laryngol.), p. 52.

[2] Loc. cit., p. 109.

term. The majority of the cases appear to have been treated with oily solution of menthol, &c., but without success. I advise that ammonium chloride in solution should be used in this case, sprayed into the trachea by means of a powerful atomiser. We know its influence on mucous membrane in encouraging normal secretion and in preventing the formation of mucus and dissolving it when thick and tenacious. Dr. de Havilland Hall[1] has used ammonium chloride internally in a similar case with marked benefit.

Mr. C. W. HOPE: At the next meeting I hope to show you the specimen from a man, who recently died at King's College Hospital, with a terrible condition of atrophic tracheitis. Some of the crusts in the trachea were half an inch long, and later the condition progressed to the stage of necrosis of the trachea and bronchi, and large masses of cartilage were shed. Death occurred from pneumonia.

The CHAIRMAN (Mr. MARK HOVELL): Iodine has been used in these cases; collosol iodine has been rubbed in with benefit.

Mr. COLLEDGE: I suggested that the man should change his occupation, but of course there are difficulties in the way of this. The condition is primary in the trachea, and I was in some difficulty as to finding a suitable name. I took the name from Sir StClair Thomson's book, as it seemed to fit.

Laryngo-fissure for Carcinoma of Larynx.

By G. W. DAWSON, F.R.C.S.

MALE, aged 49. Complained of hoarseness for several months. Laryngoscopic examination showed a whitish growth, which involved the greater portion of the right vocal cord, and caused slight immobility of that cord. Thyro-fissure was performed on October 8, 1919, preceded by a tracheotomy. In order to excise the growth thoroughly it was found necessary to remove a portion of the right arytænoid. Troublesome hæmorrhage followed, which was controlled by application of the galvano-cautery. The tracheotomy tube was removed in twenty-four hours. A profuse discharge of pus, which appeared to come from the larynx, exuded from the tracheotomy wound and lasted for fourteen days. In spite of this the patient felt very well. The voice at times is good.

[1] Loc. cit., p. 52.

DISCUSSION.

Sir STCLAIR THOMSON: It is a very satisfactory case. Was there any difficulty in swallowing a few days after the operation? Did the patient have to be fed with the tube?

Dr. IRWIN MOORE: I condemn the use of the galvano-cautery for stopping hæmorrhage in laryngo-fissure. It was advocated, years ago, in America, and with serious results. In this case, as a result of sloughing caused by the cautery, a profuse discharge of pus continued from the larynx for fourteen days. After cauterization hæmorrhage may recur, the surgeon may not be on the spot, and the patient may be lost before one can get to him. Again as a result of the inflammatory reaction and cicatrization which may follow the cautery, stenosis may occur and the voice may be damaged. I hope to show at the next meeting some sections of the larynx showing the exact position of the main bleeding point which occasionally gives rise to troublesome hæmorrhage in the thyro-fissure operation.

Mr. TILLEY: I support Dr. Irwin Moore's suggestion. You can stop bleeding from that vessel by pressure. If not, then by turning back the skin from the outer wall of the thyroid cartilage, you can secure the vessel just before it enters the larynx. With regard to injuring the arytænoid, I have occasionally purposely removed a portion of arytænoid, when the tumour approached it closely, and for two days we had to feed the patient by rectal enemata. I think there is always difficulty in swallowing for a few days if the arytænoid region is much encroached upon.

Dr. W. HILL: The difficulty in swallowing is due to the enormous œdema. In one of my cases the œdema lasted longer. I made a couple of incisions with Mackenzie's "guardian" laryngeal knife, and the œdema disappeared.

Mr. G. W. DAWSON (in reply): There was not much difficulty in swallowing. For the first twenty-four hours he was fed by the rectum. The galvano-cautery application was very slight; I do not think the suppuration was due to that. I saw mucus coming into the larynx during the operation.

Cyst of the Floor of the Nose.

By G. W. DAWSON, F.R.C.S.

GIRL, aged 21. A large cystic swelling is seen on the floor of the right nostril, causing considerable obstruction, and some swelling of the right side of the nose and cheek. Aspiration revealed clear straw-coloured fluid.

DISCUSSION.

Dr. BROWN KELLY: This is a typical case of a cyst of the floor of the nose. Some writers maintain that all cysts in this situation are connected with an underlying tooth. They appear to be unaware of a variety which arises from a duct in the mucous membrane of the floor of the nose, and which has nothing to do with a tooth.[1] This patient's teeth are perfect. Mr. Dawson might be able to confirm the nasal origin of the cyst in this case if he were to dissect it out from the gingivo-labial fold.

Dr. IRWIN MOORE: A few years ago I showed a case of retention cyst[2] in the floor of the nose, which I had intended to dissect out, with the idea that this was the only way to cure it. But the patient went to a general hospital to have a tooth drawn, and the dental surgeon incised the cyst sublabially, packed and repacked the cavity for three weeks with gauze and cured it. The cyst caused considerable swelling of the cheek and side of the nostril.

Mr. J. F. O'MALLEY: At first sight, on looking at the swelling, one would think it to be the ordinary type of dental cyst encroaching on the floor of the nose, but on palpation only a narrow strip of hard area from the lateral alar cartilage to just below the infra-orbital margin can be found. This suggests that there is a little bony ridge superficial to the cyst, that would lead one to suppose it has some connexion with a dental fang in the upper part of the alveolus.

Mr. DAWSON (in reply): I do not think this is like a dental cyst. In nearly all the dental cysts I have seen there have been bad teeth and some swelling which can be traced up to the root of a tooth. I think it is a gland which has been blocked up. I hope to get a piece of the wall for examination.

Case of Papilloma (?) of the Larynx.

By H. BUCKLAND JONES, M.B.

J. M., AGED 41, demobilized soldier, first complained of loss of voice at Bouzecourt, in France, early in 1917. He was sent home to England where he remained in hospital for five and half months. Though his voice was no better, he was again sent to France, where he was in the trenches for five or six months. He was again sent to two or three

[1] *Journ. Laryngol.*, 1898, p. 272.

[2] *Proc. Roy. Soc. Med.*, 1917, x (Sect. Laryngol.), pp. 31-32.

hospitals for short periods before evacuation to England. As his voice did not improve he was demobilized in February, 1919. On examination, the whole length of the right vocal cord is seen to be covered by a rough whitish sessile growth especially prominent in the region of the vocal process. The left vocal cord is irregular, thickened and congested with hypertrophy of the ventricular band on the same side. There is no adenitis. No attempt has yet been made to remove any portion of the growth for examination purposes as the patient has only been under observation for two weeks. Opinions are invited as to the nature of the growth, especially on the question of malignancy, and also as to the best method of removal.

Microscopical Sections of Growth removed from a Case of Papilloma of the Larynx with a Malignant Tendency, with Recurrence after a Period of Two or Three Years as an Epithelioma of the Opposite Cord.

By H. Buckland Jones, M.B.

D. R. R., A MALE, aged 75, came to me at the end of December, 1912, complaining of aphonia of seven years' duration. He caught cold in the autumn of 1905, this being followed by loss of voice. As there was no improvement in his condition he went to Bournemouth in the spring of 1906, where in addition to other treatment he was put on absolute silence for three months. His sputum was examined for tubercle bacilli, with negative result. His voice remained in more or less the same condition till he came to me.

On examination, the anterior two-thirds of the *left* vocal cord was covered by a rough whitish sessile growth (very similar to that in the former case) which was diagnosed as a papilloma. The same area on the *right* cord was composed apparently of fibrous tissue. A portion of the growth was taken for microscopical examination and later the whole growth was removed by the indirect method at three sittings. Later there was a slight recurrence and the last small nodule was removed in August, 1913.

He was sent to Mr. Cortlandt MacMahon for lessons in voice production with excellent results, as in the course of a few weeks his voice

became normal. He was last seen in July, 1914, when his cords were both slightly swollen and congested, but his voice normal. In June, 1916, he came back complaining of difficulty in speaking, though sometimes his voice was quite good. On examination, both cords were freely movable, the *left* was pale and normal, the *right* swollen, congested and somewhat nodular over its whole length. There was no adenitis. A portion of the growth was removed for examination which the pathologist reported to be epithelioma. The growth was dealt with at several sittings by the indirect method, the last small nodule, which gave a lot of trouble, being removed on August 5.

He was seen for the last time on August 8, when the cord appeared normal except for a small crust over the area from which the last nodule was removed. The patient went to the seaside and should have returned in a month for examination but has not been seen since. In August, 1919, I heard indirectly that his voice was as good as ever, but his general health was bad.

No attempt was made to remove this growth by laryngofissure, as this was contra-indicated owing to the patient's general health. He suffered from chronic Bright's disease, and as his doctor advised that a general anæsthetic and a big operation would probably prove fatal, an attempt was made to remove the growth by the indirect method.

REPORT OF PATHOLOGIST (DR. A. N. LEATHEM).

(1) Section of piece of growth removed from left vocal cord on January 30, 1913. This looks like a papilloma. The epithelial processes show either central cores of connective tissue or central spaces, the cells of the deeper layer of the rete mucosum remaining columnar and sharply cut off from the underlying connective tissue.

(2) Another section from the same piece. This section shows three small masses of epithelial cells, cut off from the surface epithelium, which look suspicious. I think this growth was a papilloma, becoming epitheliomatous.

(3) Section of piece of growth from right vocal cord removed on June 12, 1916. This section shows a typical cell nest, several epithelial masses cut off from the surface, from some of which separate epithelial cells are becoming detached and invading the connective tissue. It appears to be a definite epithelioma.

DISCUSSION.

Dr. ANDREW WYLIE: I shall be glad to know what the specimen turns out to be. I suggest removing it by the indirect method, under cocaine, with Mackenzie's forceps. Perhaps Mr. Buckland Jones will report later.

Sir STCLAIR THOMSON: I think it will be difficult to diagnose even then. It might reveal simple papilloma and yet be malignant at its base. In a New York case a growth existed for thirteen years and then proved to be malignant. It fits in with Sir Felix Semon's description—that of a field of grass which has been snowed upon. I think the case should be carefully watched.

Mr. DAWSON: The corresponding arytænoid does not move, and I should not be surprised if the case proved to be malignant.

The CHAIRMAN: I had a somewhat similar case at the Golden Square Throat Hospital, and it turned out to be malignant.

Mr. WHALE: I had a case which Mr. Wyatt Wingrave thought was carcinoma, but he could not be sure. He advised me to do thyro-fissure, and when I did so it was found to be malignant.

Mr. BUCKLAND JONES: I only wanted an opinion as to whether to do thyro-fissure or to remove the growth by other means.

Ulceration of Pharynx and Larynx.

By W. H. KELSON, M.D.

J. A. D., MALE, aged 65, came complaining of difficulty in swallowing for two months; hoarseness one month. No history of syphilis; no tubercle bacilli to be found in his expectoration, which is muco-purulent. Present weight, January 21, 10 st. 9 lb. On examination the left arytænoid and left ventricular band are seen to be distinctly œdematous, overlapping the left vocal cord; in the corresponding portion of the pharynx there is an ulcer. Enlarged glands are to be felt. Patient's general condition is fair. Suggestions as to treatment invited.

Section of Laryngology.

President—Mr. E. B. Waggett, D.S.O., M.B.

Ulceration of the Pharynx in a Woman, aged 33.[1]

By W. H. Kelson, M.D.

Mrs. H., married; no children nor miscarriages. Complains of pain on swallowing of six weeks' duration. She states that the same trouble occurred last August but gradually subsided. She says that otherwise she has always enjoyed good health.

On examination, an ulcer, the size of a shilling and covered with yellow discharge, is seen behind the posterior pillar of the fauces on the right side. It has the appearance of a breaking down gumma. There is no induration, and no enlarged glands can be felt.

This patient, shown at the last meeting but not discussed, now shows the effect of iodide of potassium given in doses from 5 to 15 gr. Note that the ulcer has disappeared.

Description of a Laryngo-stroboscope.

By E. W. Scripture, M.D. (a visitor).

A disc with forty slits is mounted on the axle of an electric motor. The light from an arc lamp is concentrated by condenser lenses so that it passes at a focus through the slits. The beam is caught by another

[1] At a meeting of the Section, held March 5, 1920.

lens; it falls on a flat head mirror and is reflected by the throat mirror to a focus at the glottis.

The singer is told to sing the same tone as that produced by blowing through the slits of the disc. His vocal cords then vibrate with the same frequency as the flashes. As soon as he sings slightly off pitch the cords appear to execute slow vibrations, whose character can be studied.

My own observations may be summed up as follows: For the chest register the vibrations take place along the entire glottis; for the head register the cartilaginous glottis is closed and the vibrations take place along the muscular glottis only.

The singer distinguishes between dull (or back) tones and front (or bright) tones. For both dull and bright tones there is usually a distinct line to be seen on the top of each vocal cord parallel to its edge. This line becomes more marked for bright tones. These are pressure lines produced by the pressure of the two masses of muscle meeting in the middle. For dull tones the cords touch lightly, for bright tones they are pressed tightly.

The tone at the glottis is produced by the yielding of the vocal muscles mainly sidewise so as to emit a series of puffs of air. In dull tones the puffs are longer and more gradual; they are separated by shorter intervals of silence. In bright tone the cords emit short sharp puffs separated by longer intervals of silence. The quality of the tone is regulated by the nature of the laryngeal action.

The observations lead to the conclusion that the masses of muscle which constitute the vocal cords act like cushions that yield to compression and not like membranes. The larynx resembles neither a reed pipe nor a membranous pipe; it is really a cushion pipe.

DISCUSSION.

Dr. SCRIPTURE: Most of us have enjoyed a stroboscope as a toy during childhood. We have all heard of the German laryngo-stroboscopes and a few have seen them. I have yet to find any writer on the action of the voice or the vocal lips who has grasped the meaning of the puff theory of vocal action and who has drawn the unavoidable conclusions. Willis, of Cambridge, in 1830, propounded a theory of the vowels in which the glottis was supposed to act by emitting a series of puffs. This theory was killed by Helmholtz. Sixty years later Hermann of Koenigsberg showed that no other theory would account for the results of analyses of German vowels traced from phonograph records; shortly afterwards I showed it alone could explain the curves of English vowels ("Studies from the Yale Psychological Laboratory," 1899).

The theory of Willis was translated into terms of glottis action in my "Study of Speech Curves" (Carnegie Institution, 1906). It is impossible to enter into details, but any one who admits the puff action of the cords in producing the voice tone can in no way escape the conclusions that inevitably follow: (1) The cord tone can in no way be altered by the cavities above it; (2) the vocal cavities cannot act as resonators reinforcing certain overtones of the cord tone but on the contrary have absolutely independent tones of their own which they add to any cord tone. This work with the laryngo-stroboscope is only a small part of a much larger field of the analysis of speech curves, of the production of artificial vowels by the puff siren with water resonators, &c. All investigations lead to the same conclusions.

Dr. DONELAN: We are much indebted to Dr. Scripture for giving us an opportunity of reviving our recollections of the elements of our knowledge of the functions of the larynx. These opportunities are rare in our Section because it is fundamentally clinical and we deal rather with pathological than with physiological conditions. Members specially interested in this side of our work will remember that Dr. Scripture's writings already occupy a well-marked place in bibliography. I think, however, that in his description of his exhibit he does himself less than justice by his omission of all reference to previous workers. He leaves us in the dilemma of having to regard him as a somewhat late claimant to be the inventor of the stroboscope or that he is assuming that our clinical pre-occupations have made us lose sight altogether of the physiological work done in Europe during the past half-century. It may, therefore, be well to recall briefly the history of the stroboscope and its employment as a help in the elucidation of the problems of voice production. There has been some discussion as to who was its first inventor. Stampfer published his account of one in 1832. This was two years before the celebrated Belgian blind physicist, Plateau, published the results of several years' experiments in 1834. The last edition of the "Encyclopædia Britannica" says unhesitatingly that we owe it to Plateau. I am content to take that view of the matter. It was, however, Mach, of Prague, who, in 1873, showed its value in the study of vibratory motion. It was then taken up by Carl Müller, who was the first to apply it as a laryngo-stroboscope to the study of the movements of the vocal cords. He published his earliest observations on the vibrations of membranes having one free side in 1877. He was followed by Oertel, Rethi, and several others. We have the first description of a specially constructed laryngo-stroboscope from Albert Müsehold, whose paper in 1898, in Fraenkel's "Archives" will be remembered by many of our senior members. The forty equal, narrow slits described by Dr. Scripture are apparently adapted from those used in the horizontal stroboscopes of our childhood with their black, moving silhouettes. Judging by the photograph shown they appear to give a more accurate definition than the openings of various sizes employed by Müsehold. I do not think the method by which the singer is allowed to deviate from the pitch is so reliable for observations as keeping him to the note

by means of a musical instrument and making the screen revolve a little faster by the accurate adjustment of the rheostat as recommended by Müsehold. It will be noticed that Dr. Scripture's observations with this instrument merely confirm those of previous investigators not only with the stroboscope but with the laryngeal mirror. Beyond this confirmation and the possibility of observing and recording by photographs any phase of laryngeal action we are not indebted to the stroboscope for any increase of our previous knowledge. Whether it will ever prove useful in the investigation of laryngeal neuroses remains to be seen. Dr. Scripture makes a special point that the larynx does not resemble a reed, nor a membranous pipe, but a cushion pipe. The terms long and short reed have been used in descriptions of the chest and head registers since the days of Garcia. Morell Mackenzie however in his popular book, "The Hygiene of the Vocal Organs," says they are only convenient descriptive terms, but he especially warns readers that no instrument, the work of men's hands, can be compared with the perfect mechanism of the larynx. As for the cushion metaphor, it was used as long ago as 1741 by Cirillus, and Müsehold, in the paper mentioned, described his investigations by means of his stroboscope on the cushion action of the trumpeter's lips as seen through a glass mouth-piece. The mechanism of this cushion action will be familiar to all here from a consideration of the individual and balancing interactions of the cricothyroids and internal thyro-arytænoids. I regret that owing to the nomenclature (dull or back tones, &c.) being unfamiliar to most of us who use European systems I am unable to follow Dr. Scripture in the details of his observations. Generally, as already remarked, they are nothing new and in fact only express agreement with previous findings. Hidden away, *anguis in herba*, amongst these conclusions is, however, one of the greatest importance. I should like to ask Dr. Scripture if he really seriously invites us on his bare assertion to regard the larynx as the sole regulator of quality of tone? Are we to cast aside, without the smallest proof to the contrary, the results of the classical investigations of Johannes Müller on the larynx, both in anatomical connexion with the resonators above the vocal cords which gave, even in the dead subject, the closest possible imitation of the human voice and the accompanying experiments on the exsected larynx which show that the vocal cords of accomplished singers could yield only the same disagreeable squeak as those of persons who could not sing at all? Are we to set aside the results of our own observations in cases where pathological interference with the resonators constantly deprives the voice of its quality of tone?

Dr. JOBSON HORNE: The present communication reminds me of work on laryngo-stroboscopy by Dr. Müsehold of Berlin, and published by him in the *Archiv. f. Laryngologie*, vii, 1898. In the previous year I was in Berlin and witnessed the experiments, and at the request of Dr. Müsehold I made a communication on the subject to the Laryngological Society of London in December, 1897.[1] Professor Oertel, of Münich, contributed a paper on the

[1] *Proc. Laryngol. Soc., Lond.*, 1897-98, v., p. 15.

same subject to the *Archiv. f. Laryngologie*, iii, p. 1, 1895. The conclusion arrived at by Dr. Scripture does not differ, so far as I can gather, from the observations demonstrated to me by Dr. Müsehold in 1897 excepting in the terminology.

The PRESIDENT: I did not gather from Dr. Scripture that he was under the impression that in this he was showing us anything new, either in regard to the instrument, or the conclusions arrived at. I thought he was going to show us a particularly good stroboscope.

Dr. SCRIPTURE (in reply): The book of Müsehold's lies in the corner of the room here: it is my private copy, which I have had for many years. My stroboscope dates from 1890, and has never been described before. This has a powerful arc light, which comes like a beam of sunlight on the glottis and the œsophagus. The image depends on the relation of the narrowness of the slit to the interval of darkness. It is only by using the most powerful arc light that you can get it so fine that the images come out clear. I agree that the stroboscope is not new: we knew it in our childhood. With regard to the laryngo-stroboscope, Oertel was the first to draw conclusions from it, but he made the mistake of supposing that the cords vibrated like membranes. My conclusions are rather different from Müsehold's. He claims that the internal thyro-arytænoid muscle contracts and governs the pitch in the chest notes, but in the head register the outer fibres contract and form a support for the internal fibres. There is a different kind of action in the two cases. I will not enter into the theory now, because it is too complicated. I shall be glad to hear of anyone who agrees with my view of the cord action. It disposes at once of all notion that the vocal cords produce tones as a series of overtones. What Caruso gets his high salary for has nothing to do with the resonators. It is easy to show that these vocal resonators cannot respond to any overtone, or modify the tones from the glottis: they are absolutely independent. The tone which comes from the glottis is something of its own. You can add these other tones, but it does not change the action of the glottis lower down. The theory of vibrations, as set out by Lord Rayleigh, shows that no resonator of the size of the vocal cavities can have any effect on sound waves of the length emitted by singers. To suppose that a small cavity can affect a long wave is nonsense. The theory is revolutionary. It is now becoming better known that this entire theory of the action of the glottis and the action of resonators and the perception of tones by the ear prevailing at the present day is quite erroneous. The "dull" and "bright" tones are also known to the singers as "front" and "back" tones. Their quality is fixed at the larynx. Vocal training to produce good voice tones must be entirely a training of the larynx.

Hæmorrhage during or after Thyro-fissure in the Removal of the Vocal Cord for Intrinsic Cancer of the Larynx, and the Chief Vessel concerned; and its Control.

By Irwin Moore, M.Ch.

Sir Henry Butlin[1] stated that in his experience during this operation he had never seen bleeding which could occasion the least anxiety, whilst Sir Felix Semon[2] records that he had only lost one patient from secondary hæmorrhage, followed by pneumonia, and this he attributed to the use of adrenalin. Those of us, however, who have had a larger experience than these surgeons have realized that hæmorrhage may not only at times unexpectedly occur, and give rise to serious anxiety, but also tax the ingenuity and skill of the operator.

Hæmorrhage may occur either during removal of the growth, immediately following removal, or some hours after operation. During removal of the growth a considerable amount of bleeding may occur, but this can generally be easily controlled by gauze pressure. Persistent oozing frequently occurs after separation of the muscular attachment round the arytænoid, and a small vessel may be found between the arytænoid and lateral wall of the thyroid cartilage which occasionally spurts and may give considerable trouble in picking up with pressure forceps and ligaturing. However carefully any bleeding may be controlled before closure of the larynx a reactionary or secondary hæmorrhage may possibly occur in from four to six hours. It is more likely to take place in full-blooded or alcoholic subjects, or in those cases where the growth has extended deeply into the subglottic or posteriorly into the arytænoid regions, necessitating considerable excision of muscular tissue. Secondary hæmorrhage may be due to a general oozing from the raw surface and be severe enough to necessitate re-opening and gauze-packing of the larynx, or it may arise from the vessel in the arytænoid region and be due to slipping of a ligature applied before closure of the laryngeal fissure.

Eight cases of serious hæmorrhage have been reported with four deaths. In most of these cases bleeding was observed during operation to occur from a spurting vessel in the arytænoid region.

[1] "The Operative Surgery of Malignant Diseases," 1900, p. 191.

[2] Discussion on E. D. D. Davis's Case of Laryngo-fissure for Epithelioma of the Right Vocal Cord. *Proc. Roy. Soc. Med.*, 1914, vii (Sect. Laryngol.), p. 198.

With a view to ascertaining the exact position and course of this vessel so as to permit of better control during operation, I begged the assistance of Professor Shattock, and will now place before you, by means of the epidiascope, the result of our investigations.

Fig. 1 shows a dissection of the larynx made from the left side with the arteries injected. (Specimen No. 195, Anatomical Series, from the Museum of the Royal College of Surgeons.) This has been specially drawn for me by kind permission of Professor Shattock and under his supervision. He has also supplied me with the following description :—

The left ala of the thyroid has been removed. The posterior crico-arytænoid, and above this, the thyro-arytænoid muscles are shown passing to their insertion into the arytænoid cartilage. The ventricle of Morgagni has been opened from the external aspect, and above it lies a portion of the sacculus. The artery of which the upper end is cut across is the superior laryngeal, and from it there arise in order from above downwards: (1) From the inner and posterior aspect, a short branch which subdivides into one branch directed upwards to the mucous membrane, and the other crossing the site of the arytænoid cartilage, and distributed to the mucosa, &c., behind the posterior crico-arytænoid muscles. The tortuosity of this vessel is noteworthy and may be referred to the rotatory movements of the arytænoid cartilage over which it runs. (2) Proceeding downwards, there is a branch, cut short, from the front of the main artery followed by (3) a long offset passing forwards and downwards to the thyro-artyænoid muscle. From this, a short way from its origin, there is a branch which is cut short and probably represents the anastomotic branch between the superior and inferior laryngeal arteries.[1] Next in order comes: (4) A branch passing directly inwards to the thyro-arytænoid muscle in the neighbourhood of its insertion. And finally, the vessel terminates by bifurcating to supply the posterior part of the lateral crico-arytænoid muscle.

Fig. 2 represents a horizontal section of a normal larynx, from a man aged 24, who died, cyanosed, with heart disease; the veins are distended with blood. This specimen was specially prepared by Professor Shattock for the purpose of this demonstration. The section is made through the processus vocales, and vocal cords, and shows the mass of the thyro-arytænoid and lateral crico-arytænoid

[1] A coloured illustration showing the distribution and anastamoses of the laryngeal arteries will be found in Luschka: "Der Kehlkopf des Menschen," 1871. Taf. viii, fig. 1.

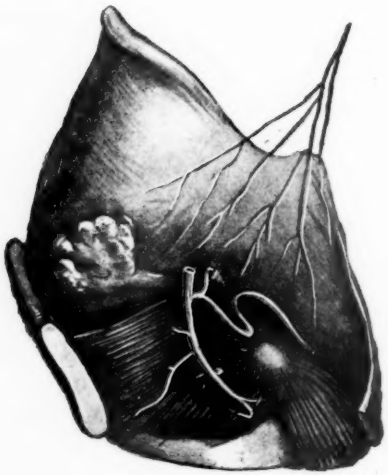


FIG. 1.

Dissection of the left side of the larynx with the superior laryngeal artery and its branches injected. (Natural size.)

(Specimen No. 195, Anatomical Series, from the Museum of the Royal College of Surgeons.)

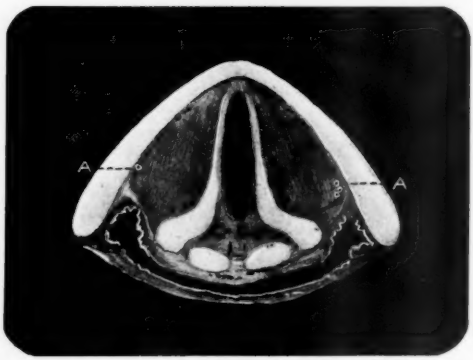


FIG. 2.

Horizontal section of a normal larynx. A, represents the superior laryngeal artery or its branches. (Natural size.)

(Specially prepared for this demonstration by Professor S. G. Shattock, F.R.S.)

muscles occupying the space between the cords and alæ of the thyroid cartilage. In the more posterior part of the muscular mass there is shown on the left side the transversely divided superior laryngeal artery lying close to the perichondrium, and on the right side, in a corresponding position, but somewhat more deeply in the muscle, there are three small arteries derived from the same source. The veins in the thyro-arytænoid muscle in the angle on the outer side of the processus vocales are conspicuous in number.

The conclusion we may draw from these investigations is that in those cases in which it is necessary to cut through a considerable amount of muscular tissue close up to or including the arytænoid cartilage, the superior laryngeal artery may be cut or injured. Since the vessel in this situation lies close against the perichondrium, further separation of the perichondrium from off the thyroid ala is indicated so as to allow the vessel to be more easily seized by pressure forceps and securely ligatured. I think we shall find this practical point of considerable service in future cases which come under our care.

Dr. W. HILL: I had a death at St. Mary's in a case of this kind. I had operated in the afternoon, and the patient was all right at 8 o'clock at night, but he died of hæmorrhage, which the house surgeon was not competent to deal with, at 10 o'clock. It was an early secondary hæmorrhage. The case had given trouble during the operation. Dr. Irwin Moore was there and helped me. We had a difficulty in picking up a bleeding vessel. I did not then know that the right thing would be to separate the perichondrium, so that we could tie the vessel deeply. We did tie it, but the tying was inefficient. If I had kept the tracheotomy tube in, the house surgeon would not have hesitated to re-open the laryngeal cavity and plug, and that is a good reason for retaining the tracheotomy tube if there is not likely to be an expert there to stop hæmorrhage. I was surprised to hear Dr. Moore say there had been several deaths from early reactionary hæmorrhage.

Bilateral Chronic Empyemata of the Frontal Sinuses: Two Patients on whom the External Operation has been Performed.

By HERBERT TILLEY, F.R.C.S.

THE chief points of interest are: (1) The large superficial extent of the sinuses; (2) the fact that, after thorough pickling with a liquid compound of bismuth, iodoform and paraffin (B.I.P.), the external wound was sutured at the close of the operation.

(The following notes are written from memory.)

Mr. T., aged about 60, had the left frontal sinus opened by the late Mr. Bucknall (University College Hospital), some fifteen years ago to relieve headache and a left-sided purulent discharge. The operation gave relief, but the discharge never ceased entirely. During the past five to six years I have seen the patient many times, because of some pain over the *right* frontal sinus. This was relieved from time to time by intranasal irrigation and removal of ethmoidal granulations from the lower end of the fronto-nasal canal. During the later months of 1919 the pain became intolerable, and I opened the right sinus through the eyebrow—an extensive sinus, filled with pus and large œdematous granulations, was revealed. The interfrontal septum was defective and pus could be seen coming from the left sinus. Hence I turned up the soft tissues of the lower forehead by making an incision over the root of the nose, thus joining the inner ends of the old and my new incisions. The anterior walls of each sinus and the pyogenic membrane were removed, the exposed surfaces anointed with B.I.P., and the soft tissues replaced and sutured. The patient made a rapid recovery; he is now free from all pain and nasal discharge, and the scarring is very slight.

Miss D., aged 38. A long history of frontal headache and purulent nasal discharge. Both frontal sinuses were found to be diseased, as also the ethmoidal regions and antra. November, 1919: The patient came to hospital suffering from great pain, temperature 102° F., and appearing to be very ill. The soft tissues over the lower forehead were red, swollen, and œdematous. An operation was carried out identical in technical details with that already described, but the convalescence was less rapid. A small point in the right eyebrow incision continued to discharge a clear serous fluid for three or four weeks, and it was intensely painful to the gentlest touch. The patient has now recovered, and is free from all symptoms. The sinuses extended into the temporal fossa on each side.

Such cases as these appear to the exhibitor to demand external operation rather than the intranasal method, and he has been impressed by the rapid healing of the large surfaces when they have been treated with B.I.P. He has employed this method with complete and rapid success in five cases.

Mr. H. Lawson Whale, F.R.C.S., exhibited a "Specimen of Adult Frontal Bone showing Small-sized Frontal Sinus."

Multiple Sinusitis with Necrosis of Frontal Septum.

By JAMES DONELAN, M.B.

THE patient, a man, aged 27, was first seen at the Italian Hospital in the autumn of 1917. He gave the following curious history: From the year 1900 to 1905 he had mucous "polypi" removed from both nasal fossæ at several London throat departments. From 1905 to 1915 he attended the special department of a large hospital and had his polypi cleared on an average three times a year. He was in the Army from 1915 to 1917 and underwent repeated operations for the same purpose.

When seen at the Italian Hospital both nasal fossæ were packed with these growths, and some of them protruded behind the soft palate into the pharynx. A thin stream of pus made its way on both sides and in both directions. He had some very carious molars in both upper jaws. The exhibitor removed the polypi by snare on repeated occasions as a preliminary to further treatment, and a diagnosis of suppuration in both maxillary antra was made. These were cleared by intra-nasal operation on January 10 and February 7, 1918, and though packed with polypi contained but little pus. X-ray photographs gave no positive indication of suppuration in the accessory sinuses, nor did the puncture of either antral cavity. Further radiograms showed a dark area corresponding to the position of the frontal septum, and as the patient complained of pain and swelling at the root of the nose and to the left of the middle line it was decided to open the left frontal sinus. In the course of the operation it became evident that a more extensive procedure was necessary. The frontal septum and the interior table of the frontal bone corresponding to it had become carious. The frontal sinuses were of very large size and packed with polypi. The left nasal bone was necrosed along its anterior edge and formed a separate small abscess, which at that time discharged into the nose. Before the removal of the anterior wall of the right sinus could be completed the patient showed signs of collapse.

Both sinuses were packed with gauze through the incision over the left frontal sinuses, and a large rubber fronto-nasal drain was put in the floor of each cavity.

The patient made a good recovery, retarded only by a further sequestrum from the left nasal bone. In view of the extent of bone removed on both sides there is very little deformity.

Frontal Sinus Suppuration: Extradural Abscess.

By Norman Patterson, F.R.C.S.

Female, aged 34, first seen May, 1919, has always suffered from colds. History of blood-stained discharge from the right nostril for two or three years. For the last six months the right eye has been nearly closed. Pain over eye and diplopia.

Examination showed ptosis of the upper right eyelid, swelling in the region of the floor and anterior wall of the right frontal sinus; no redness nor œdema; no fluctuation obtainable. There was discharge coming from the middle meatus, and the right middle turbinate was much enlarged. The patient was admitted to hospital and the anterior end of the right middle turbinate removed with a view to improving drainage, but the condition remained *in statu quo*.

On June 1, 1919, the usual incision was made below the right eyebrow and a large quantity of pus was evacuated.

Bacteriological report: Cultures show groups of Gram-positive cocci, probably staphylococci.

On further exploration the inferior and posterior walls of the sinus were found to be completely destroyed, and the collection of pus represented a large extradural abscess. In order to ascertain the extent of the abscess, an incision had to be carried up into the forehead, and the whole of the anterior wall of the sinus was removed. The abscess was found to be bounded posteriorly by a very œdematous dura. The cavity was packed and left somewhat widely open.

On July 28 another operation was necessary as a fresh accumulation of pus had taken place. The site of the original abscess was freely opened up and granulations removed. A large opening was made into the nose.

The patient went on fairly well for a time, but on September 3 she had to be admitted again with recurrence of pain. The wound was again opened up freely and granulations removed; no attempt was made to close it at any point, but packing was introduced between the dura and the tissues of the scalp which composed the flap.

The patient is now in a satisfactory condition save for the fact that there is still a large opening present and pulsating dura can be seen at the bottom of the wound. The cavity shows no signs of becoming obliterated by granulation tissue.

The exhibitor is anxious to have the opinion of members of the Section with regard to subsequent treatment.

DISCUSSION.

Dr. DAN MCKENZIE: I would like to hear, from Mr. Tilley and others, what their experience of the intranasal frontal sinus operation has been. We are now in a position to judge as to its value. My experience of the intranasal operation has been, to some extent, very successful. It is not a very difficult operation to perform, nor is it severe for the patient. It gives immediate relief to acute symptoms, and I have found it perhaps most useful in acute frontal sinusitis of short duration. But I regret to say I have had one death following the operation from osteomyelitis: and mine is not the only case. It is a point we must consider in estimating the value of the intranasal as compared with the external operation, because, I take it, it was the fear of osteomyelitis which induced us to adopt the less severe operation. Yet when we find that the milder operation may also be followed by osteomyelitis we must revise our notions as to its safety. It is probably correct to say that one is not justified in operating on the frontal sinus at all unless for the relief of serious symptoms, and in that case I for one would now be inclined to operate externally, save in very early and acute cases.

Mr. W. STUART-LOW: I am glad that Mr. Tilley now gives preference to the external operation of which I have often shown many successful cases at this Section. I have always emphatically condemned the internal operation, having called it "blind surgery" to attempt to enter the frontal sinus upwards through the nasal passage. Inflammatory septic conditions of the frontal sinus are either acute or chronic from the point of view of treatment, and acute cases are always relieved very much and sometimes cured by removal of the lower half of the middle turbinal: this region of the nose should first be well cocainized by spraying with a 20 per cent. solution of cocaine and then packing it with wool soaked in this solution. This constringes the mucous membrane, effectively removes the intense congestion, and reduces the swelling, which in itself gives great relief and often allows and is followed by a profuse discharge from the sinus and surrounding cells. This should be the procedure in all acute cases, followed by removal of the greater part of the middle turbinal. I have in many former cases reflected adversely on the blemishing and disfiguring facial result of Mr. Tilley's methods, as seen in the scar in his external frontal sinus cases. These two patients are an emphatic illustration of this—the scar is thick, almost cartilaginous, feels and looks like keloid, and is very unsightly. I object to the use of iodoform emulsion or "B.I.P.," which he is very given to employ. The iodoform is a very poor antiseptic, and is, I believe, too irritating to the tissues: it irritates the wound, and, I think, accounts for this condition. The fatty particles in the "B.I.P" get between the edges of the wound and delay healing by first intention, which it is essential to obtain in all face surgery. These cases were five months under treatment, whereas with my methods three weeks was an average time, the scar being almost invisible. The depression in these two instances of Mr. Tilley's is very considerable—

this I have always managed to avoid by the employment of the cage which I utilize to cover the wound in order to prevent all bandage pressure. This cage, which I have often exhibited before the Section, also facilitates rapid healing of the skin incisions by its rubber-covered edges pressing on the soft surrounding structures, acting as a Bier's band.

Mr. G. W. DAWSON: I have no great belief in the intranasal operation: in most of the cases I have had, I did the external operation. I have seen why, in my cases, the intranasal operation did not give relief. There are narrow galleries or clefts scarcely thicker than the back of a knife, which are filled up by pyogenic membrane, and which no lotion can reach. Therefore the intranasal operation cannot quite succeed. We scrape the Eustachian tube and expect it to close, yet when we scrape the frontal sinus opening we expect it to get larger as a result, which does not seem logical.

Dr. D. R. PATERSON: In earlier days I was a strenuous advocate of the external operation, and I did nearly forty cases before I got one which developed osteomyelitis. I was very pleased with the results of the external operation, but since that time the experience which has been given us by Dr. McKenzie in the *Journal of Laryngology* has rather put one off it. But I have not much faith in intranasal operations in extensive sinus disease; on the contrary, I have seen ill-effects from it in my own practice, and also in the practice of some of my colleagues, in which the operation has been done *secundum artem*. The rasping away of the mucosa of the fronto-nasal canal is too often followed by cicatricial contraction. One has felt that if the external operation could be made safe from osteomyelitis, it is the operation we should choose in such cases. I do not know whether Mr. Tilley has secured a preventive from osteomyelitis by his dressing, and I hope he will let us have his mature experience later. Another point concerns the proper drainage of the frontal sinus. One of the difficulties is the frequent closure of the fronto-nasal canal which takes place. Even a very large opening made between the frontal sinus and the nose frequently contracts, so much so that it will hardly admit a probe. If we can solve that difficulty, it will enable us to advance very considerably. Lately I have been trying, with some success, the use of a large size tracheotomy cannula, the flange lying on the external wound, and retaining it there for some weeks. It may be necessary to control it for two or three months, but when one is satisfied that the communication is well made and sustained, it is easy to close the outer opening. The scar it produces is very slight.

Dr. PEGLER: An important item in treatment by the intranasal route which has not been mentioned, is the teaching of the patient to wash out his own frontal sinus, just as we are in the habit of doing in the case of the maxillary antrum. He is supplied with a cannula that has been properly adapted to his own fronto-nasal canal, and I have rarely found any difficulty in getting the patient into the way of it. Change of solutions may be necessary,

but time is the chief essential and perseverance on the part of the patient himself. The cases seem to disappear from observation, and I have always had reason to believe that a cure has resulted.

Dr. W. HILL: I appreciate that the argument is in favour of those who are against the internal operation. When the subject was brought up before by Mr. Tilley and Dr. Watson-Williams the testimony was striking, and as one could not ignore what they said, I tried this method. I was surprised to find that frontal sinuses which were suppurating, and sometimes causing pain, cleared up under what I regarded as a most imperfect operation. I do not say it succeeds in every case—I have had to do the external operation in some, but very few. When one has to do the external operation because the other has failed, the reason is obvious; but I think the same obtains in the cases which have apparently healed up. So much have I been impressed, in spite of what I regarded as my better judgment, that I never dream of doing an external operation unless for some special circumstances, such as a fulminating attack, until I have given the other a good trial. It is not a very difficult operation; my assistants can do it very well, and they go on with the after-treatment. The results, on the whole, are not bad. Two-thirds of the cases go on fairly well and do not need further operation. I have not done the external operation within the last five years; I have not had occasion. I have used not silver but flavine; I do not know whether of itself it does good, or acts by mere drainage. I know the operation must be partial. I do not know whether the granulation tissue shrinks. It looks as if bold measures would be needed to remove it.

The PRESIDENT: Everyone will, I think, agree that success or failure with the internal operation depends upon the anatomy of the parts, and therefore a stereoscopic skiagram is essential as an aid to decision. With regard to osteomyelitis, one must always be apprehensive concerning it, and I believe it to be a prudent plan to insert a drain tube in the outer angle, which is the most dangerous part. If it be left there five or six days it causes no scar. In cases done by the external operation, without the aid of a skiagram I strongly recommend the use of the Doyen perforator rather than a trephine. If dura is exposed with the Doyen instrument no harm is done. With regard to B.I.P., Mr. Davis and I are using it extensively in mastoid operations, and I have asked Mr. Davis to publish the first case of iodoform poisoning. Hitherto we have had none. With regard to the scar, I often think it is well to avoid in these double cases the spectacle-cut across the bridge, and I think it often can be avoided. This portion of the scar is somewhat disfiguring. One of the cases has got a curtainlike scar in front of the internal canthus. I suggest that if the patient will massage it for twenty minutes a day it will disappear in three months.

Mr. LAWSON WHALE: Mr. Norman Patterson asked me to say a word about his case. He wants to emphasize the fact that this girl was well, and spent the whole Whitsuntide holiday with a frontal sinus full of pus. On her

return when she was examined the wall was found necrosed; there had, therefore, been no pressure and no pain. The pulsating dura bulges forwards over what was once the upper end of the infundibulum; and Mr. Patterson is very anxious for suggestions as to treatment.

Mr. CYRIL HORSFORD: I would like to refer to two serious cases of complications that I have had. About Christmas I was asked to see an old patient for whom I had done a double antrum operation for acute frontal pain. I removed the middle turbinal, enlarged the fronto-nasal duct, and got to the frontal sinus in the usual way, and it cured her, although the pain had lasted some weeks. Months afterwards she developed the same condition on the other side, and I did the intranasal operation there with success. But within a week of the second operation she complained of pain in the chest, though we could not find anything wrong there. Eventually it was proved to be a septic embolus in her lung, which resulted in an abscess there. It may have been the result of the sinus operation. The second case occurred at the end of last week. The man had had a gunshot wound through the orbit, which had destroyed the right eye, and it passed into the neck. Before I saw him he had had fifteen operations for the removal of pieces of necrosed bone, and plastic operations to improve the eye. He was sent to me because of recurring abscesses in the eye sockets. Believing that the frontal sinus was infected, I passed a cannula and washed it out, and thick pus came out through a small opening in the lid. The pain was considerably relieved, and in a few days I repeated the process. I washed it out four times, and on the last occasion there was no pus and no pain. Next day I heard he was playing in the grounds of the hospital and had struck his eye-brow. That evening his temperature was 102° F., and the next day he was seriously ill, and had something of the nature of epileptic fits: apparently he had meningitis. I at once opened the frontal sinus externally, but found no evidence of fracture or injury, nor any communication suggesting direct infection to the meninges. The cerebro-spinal fluid withdrawn by lumbar puncture was very turbid. He improved very much, and two days afterwards I repeated the lumbar puncture; the fluid was purulent, and he died next day. He was found to have had a tremendous frontal abscess, which had burst, and the pathologist considered he had had the abscess for a long time. This was evidently a case of latent frontal abscess—producing no symptom up to a few days before death.

Mr. J. F. O'MALLEY: I think both the internal and the external routes have their place in frontal sinus operations, and, with a certain amount of care exercised, one can determine which is the better one to adopt in a given case. In the acute case, where there is pain, and where on examination one finds there is interference with drainage of the frontal sinus, I agree with what Mr. Stuart-Low said, that removal of the middle turbinal is often sufficient. Chronic cases belong to a different category, and I would divide them into two kinds. One is that in which, after shrinking of the middle turbinate there is a free

escape of pus from the frontal sinus and no polypi present in the ethmoidal region. The second chronic class consists of those cases in which there is a free formation of polypi in that region. For the first class, I think that if one takes away the turbinate, and opens the ethmoidal cells and clears the area as far as the upper part of the infundibulum, the intranasal operation will succeed. But if there is much polypoid formation in the middle turbinate region, affecting all the lining of the ethmoid cells, it is practically certain that the same condition exists in the frontal sinus itself, and it is then one has to resort to the external operation. The whole secret of curing sepsis is free drainage, and when I open the sinus externally my great object is to get ample drainage into the nose, leaving the tube in for several weeks. I feel then that I have some sort of smooth lining to the passage, and that it is not likely to contract down sufficiently to become permanently blocked. If you cut away a portion of hard bone the microscope shows that the bone in the immediate neighbourhood always separates and dies. If there is ample drainage into the nose there is practically no risk, even if portions of the edges of an opened frontal sinus should disintegrate and cause a little sepsis.

Mr. TILLEY (in reply) : I operated upon the female patient during Eastertime, 1913, and we found extensive disease in the sinus. There was no question of doing the intranasal operation. She had very large sinuses, and we took away the whole anterior walls of each frontal sinus, and I am certain no foci of suppuration were left. The wound healed and remained well until November last. Then the forehead became swollen, red and œdematous, and the patient was in great pain. We opened the old incisions and found the site of the former sinuses filled with pus and granulations. An interesting condition now revealed itself which I have not seen mentioned hitherto, namely, that when recurrence of suppuration takes place in a case which has apparently been cured for some years, and you reopen at the old wound, you find that in some part of the sinus, generally in its outer extension, little bony recesses have formed in the frontal bone. There were three of these in this patient. These were opened up and obliterated. I did not suture the wound immediately because it was in an acutely inflamed condition. I put stitches in the outer part and left the inner halves open. With regard to the recesses mentioned above I had seen them before, and I attributed their presence to faulty technique on my part. But I think what had happened was that some non-pyogenic organisms had been lying there perhaps all those years, and had absorbed the bone and thus produced little cavities which were revealed when pyogenic infection took place and operation for its relief was instituted. I think the question of intranasal *versus* extranasal operation is such a large one that it would be well to have a special debate on it now that we have a large experience of both methods. In every case I deal with we have a skiagram taken, in profile and in the frontal aspect. I have done two or three hundred cases by the intranasal operation and I always prefer it where it is possible. In the type of cases we have seen to-day it is obvious that the intranasal operation would have been bad surgery and would have broken all the canons of

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escape of pus from the frontal sinus and no polypi present in the ethmoidal region. The second chronic class consists of those cases in which there is a free formation of polypi in that region. For the first class, I think that if one takes away the turbinate, and opens the ethmoidal cells and clears the area as far as the upper part of the infundibulum, the intranasal operation will succeed. But if there is much polypoid formation in the middle turbinate region, affecting all the lining of the ethmoid cells, it is practically certain that the same condition exists in the frontal sinus itself, and it is then one has to resort to the external operation. The whole secret of curing sepsis is free drainage, and when I open the sinus externally my great object is to get ample drainage into the nose, leaving the tube in for several weeks. I feel then that I have some sort of smooth lining to the passage, and that it is not likely to contract down sufficiently to become permanently blocked. If you cut away a portion of hard bone the microscope shows that the bone in the immediate neighbourhood always separates and dies. If there is ample drainage into the nose there is practically no risk, even if portions of the edges of an opened frontal sinus should disintegrate and cause a little sepsis.

Mr. TILLEY (in reply): I operated upon the female patient during Eastertime, 1913, and we found extensive disease in the sinus. There was no question of doing the intranasal operation. She had very large sinuses, and we took away the whole anterior walls of each frontal sinus, and I am certain no foci of suppuration were left. The wound healed and remained well until November last. Then the forehead became swollen, red and œdematous, and the patient was in great pain. We opened the old incisions and found the site of the former sinuses filled with pus and granulations. An interesting condition now revealed itself which I have not seen mentioned hitherto, namely, that when recurrence of suppuration takes place in a case which has apparently been cured for some years, and you reopen at the old wound, you find that in some part of the sinus, generally in its outer extension, little bony recesses have formed in the frontal bone. There were three of these in this patient. These were opened up and obliterated. I did not suture the wound immediately because it was in an acutely inflamed condition. I put stitches in the outer part and left the inner halves open. With regard to the recesses mentioned above I had seen them before, and I attributed their presence to faulty technique on my part. But I think what had happened was that some non-pyogenic organisms had been lying there perhaps all those years, and had absorbed the bone and thus produced little cavities which were revealed when pyogenic infection took place and operation for its relief was instituted. I think the question of intranasal *versus* extranasal operation is such a large one that it would be well to have a special debate on it now that we have a large experience of both methods. In every case I deal with we have a skiagram taken, in profile and in the frontal aspect. I have done two or three hundred cases by the intranasal operation and I always prefer it where it is possible. In the type of cases we have seen to-day it is obvious that the intranasal operation would have been bad surgery and would have broken all the canons of

treatment of a suppurating bony cavity. With regard to the question of exposure of the dura mater, I have seen a patient who walked into the hospital and asked for the eye department because he "saw double"; that was his only symptom. Mr. Flemming sent him to me to see if I could explain the proptosis. The frontal sinus was full of pus, and we found the dura mater over its posterior wall exposed. The patient had never even had a headache. I have seen four cases of frontal lobe abscess which have been undiagnosed, and they died from acute cerebral symptoms. Frontal lobe abscesses have been found secondary to ethmoidal and frontal sinus mischief, and the cerebral lesion is difficult to diagnose, because it occurs in the "silent area" of the brain. These are further points worthy of discussion in connexion with suppuration in the frontal sinuses.

The PRESIDENT: That will be suggested to the Council.

Treatment of Sarcoma and Endothelioma of the Naso-pharynx by Radium.

By W. DOUGLAS HARMER, M.Ch., and N. S. FINZI, M.B.

Sarcoma of Tonsil.—Female, aged 29. Swelling of right side of neck six weeks, tumour in throat three weeks, gradual loss of voice. On admission found to have a large hard swelling, 4 in. long, below the angle of the right lower jaw. Internally elongated retropharyngeal swelling behind the right tonsil, covered by normal mucosa, extending above the level of the soft palate down to the region of the arytænoids. This tumour caused marked obstruction in the pharynx with dysphagia and slight dyspnœa. The right vocal cord (seen with difficulty) was fixed and the right part of the tongue atrophied. Tumour explored by incision in neck and found to be non-encapsuled, very extensive and adherent. A fragment removed was submitted to Dr. Andrews who reported a small round celled sarcoma, involving muscles. January 5, 1920: Radium bromide 230 mg. buried in growth for twenty-four hours. January 12, 1920: External swelling much less, internally half original size. February 15, 1920: Radium repeated, tumour has almost disappeared.

Endothelioma of Pharynx.—Male, aged 69. Two months alteration of voice; three weeks tumour in throat; recently dysphagia, no dyspnœa; losing weight. On admission, region of left tonsil occupied by a very large tumour, involving the palate, pharynx and the posterior part of the tongue. Ulceration of a third of its surface. Section

showed a new growth, either an alveolar sarcoma or endothelioma. March 14, 1918: Radium 150 mg. imbedded in tumour twenty-four hours. April 4, 1918: Tumour a third of original size. May 2, 1918: Tumour has disappeared. October 24, 1918: Small tumour seen in region of right tonsil, radium inserted for twenty-four hours. October 8, 1919: Massive growth as large as a fist below angle of right lower jaw. Throat found normal. Mass explored and found to be encapsuled and adherent. External carotid artery temporarily ligatured and growth removed with difficulty. Has since been under X-ray treatment.

Tumour of Palate, Mixed Parotid Type.—Male, aged 50. One month tumour of palate. On admission rounded swelling involving right side of soft palate and right tonsillar region. Section showed a mixed parotid tumour. December 19, 1918: Radium 160 mg. inserted into growth for twenty-four hours. This caused extensive bruising of the palate and neighbouring tissues. Swelling produced by hæmatoma subsided slowly. December 20, 1919: Later, radium again buried in throat for seventeen hours. Still has a small hard mass in right side of palate.

Tumour of Palate: Endothelioma.—Female. Nine years before cyst removed from roof of mouth. 1914: Treated by radium; two applications for recurrence. 1919: Growth suddenly returned. December 18, 1919: Radium, 100 mg., inserted for seven hours. February 2, 1920: Radium repeated. Section shows endothelioma; growth shrinking rapidly (originally filled perforation in hard palate).

Tumour of Orbit: ? Carcinoma.—Female, aged 57. Six years growth of right upper eyelid; five operations for removal at the Western Ophthalmic Hospital. Had had radium treatment for four months. July 31, 1918: Removal of large growth from upper eyelid by diathermy. Sections showed masses of squamous epithelium, anaplastic in character, with many blood-vessels but no cell nests. From the type of cell and the character of the vessels the growth is presumably malignant. January 17, 1919: Radium, 50 mg., inserted into orbit; 75 mg. applied externally in front of the ear. June 10, 1919: Partial excision of upper jaw and removal of structures in orbit, later, of glands of the neck. This case, unlike an endothelioma, did not improve with radium.

Naso-pharyngeal Tumour: Fibro-angeioma.—Male. Nine months swelling of left cheek, nasal obstruction and proptosis. On admission found to have a massive tumour (nasopharyngeal) filling up the left

nasal fossa, antrum, orbit, nasopharynx, and causing great swelling of temporal region. Section showed fibro-angeioma. May 15, 1919: Radium bromide, 147 mg., inserted in the growth for twenty-four hours. July 2, 1919: Swelling half original size; radium again inserted for twenty-two and a half hours. This treatment caused the growth to shrink rapidly, but it was followed by extensive necrosis of the maxilla with perforation of the hard palate. August 8, 1919: Explored by external incision and scar tissue and many sequestra removed, no evidence of growth discovered. Later the cavity continued to discharge freely and healing was slow (radium burn). January 1, 1920: Developed great weakness owing to multiple peripheral (toxic) neuritis which is slowly abating. The case illustrates the danger of necrosis after radium is buried in the upper jaw.

DISCUSSION.

The PRESIDENT: These remarkable cases are worthy of having a couple of meetings devoted to them. Does anyone know what is the condition of one of these tumours in a state of arrest? Has anyone had opportunities of examining them microscopically after radiolization? What is there known about the repetition of radiolization practised in order to prevent that recurrence which is commonly seen? Also, may we be told whether radium solution is of any value? Has anyone been trying the injecting of arteries with radiolized fluid?

Dr. D. R. PATERSON: I have had a number of these cases myself. I have tried radium for these nasopharyngeal tumours with admirable results. In three or four the results have been as encouraging as in Mr. Harmer's. With regard to its effect on carcinoma, my results coincide with his; there has been very little advantage in such cases. For nasopharyngeal sarcoma I do not think there is anything to equal radium, and the results are extraordinarily good.

Mr. DOUGLAS HARMER (in reply): I brought these cases forward to show the difference between the results obtained by means of radium in cases of endothelioma, sarcoma, and carcinoma. In the first two the results have been so striking, and the shrivelling of the tumours so rapid, that it is questionable whether we should ever operate before radium treatment has been tried. It is extremely important that, wherever possible, the radium should be actually buried in the growth. At the Radium Institute they often use external applications, but, in my experience, these have not produced such good results. Secondly, wherever possible, the radium should be buried by an aseptic operation, from outside the neck, rather than through the pharynx. In the case of an officer whom I treated, with Dr. Finzi, some years ago, there was a large recurrence

in the retropharyngeal glands, and owing to previous operations on his neck and to scarring, it was impossible to approach the site from without; therefore we buried the radium in the pharynx. The wound became septic, and the whole of the tumour sloughed out in one core, leaving a large cavity, which extended down to the vertebral column. There was no hæmorrhage, and no bad result. Still, one can foresee that, if radium was not used carefully, some serious hæmorrhage might occur. With regard to after treatment, I think we have learned that it is essential not only to give these patients one or two radium treatments, but also to keep them under very careful observation. Nearly all of them are submitted to X-ray treatment for one or two years, and they are asked to report regularly, so that if disease is still present the condition can again be attacked with radium after a suitable interval. The results obtained with radium in the treatment of carcinoma of the upper air-passages have not been nearly as good, but it is often possible to delay the progress of the disease, and in some instances the shrinking of the growth has been so marked that an operation could afterwards be successfully performed.

Dr. FINZI (in reply): I hope this series of cases is only the first of a couple of other series which Mr. Harmer and I hope to show on subsequent occasions. For that reason, I trust you will not draw from the present series too many conclusions as to the types of disease in which we have good results. It has been charged against radium that one cannot tell what the result is going to be in a case before starting to treat it. That I do not think is correct; one can get a very fair idea of the correct type of case to treat with radium. I place first of all round-celled sarcomata as being favourably influenced by radium radiations, and in that form I say the surgeon should not operate, they should always be treated by radium in preference to surgery. The endotheliomata come next, and they do not respond quite so favourably, but usually one gets good results in them from radium. I do not say these should not be operated upon; I think it is a choice between the two methods, which will be influenced by the extent of the disease and the size of the operation which would be needed in surgery. But one should use radium or X-rays as a prophylactic measure, even if operation is done, in order to prevent implantation. In epitheliomata one does not get such good results from radium; in certain classes the results are better than in others. I shall be glad to discuss these cases more freely when we have shown other cases. I associate myself with Mr. Harmer's remarks as to the necessity of burying the radium aseptically in the tumour. You give a large dose to the cells which are near to the radium, and it is necessary to keep the tissues aseptic, otherwise there is a big chance of their sloughing; I have lost a case from secondary hæmorrhage owing to the wound becoming septic. With regard to the condition of the tissues after radium treatment, we have had the opportunity of examining a number of these cases, some time after the treatment, and what we found was a considerable quantity of fibrous tissue, with a number of small islands of carcinoma cells in this fibrous tissue. If you go on treating a case and it gets completely well,

you can apparently get rid of these islands; much of the fibrous tissue also becomes absorbed. I expect recurrences are due to some small island having been left behind. Mr. Harmer will remember we had a case some years ago in which we obtained sections which showed these islands, and the man remained well about a couple of years, and then had a recurrence. Presumably our prophylactic treatment from the outside had failed to get rid of these islands. I always consider it advisable, after no microscopic evidence of disease remains, to give at least two more radium treatments, or else go on for a long time with filtered X-rays. You cannot bury radium in the tissues for any length of time when there is no growth there. It would cause sloughing. With reference to radium solutions, they have been used by Haret in France, and he has reported some results which were perhaps good, but he has not reported anything on the matter for nine years, so I take it there is not much in it. *A priori*, one would not expect good results from radium solutions, because in treating carcinoma one must filter the rays, and you cannot filter the rays from the emanations of radium solutions when they are in the tissues; you are then getting all the rays, the alpha- the beta- and the gamma-rays, that is to say, rays which you do not want as well as those which do good.

Child suffering from Retained Tracheotomy Tube.

By W. Douglas Harmer, M.Ch.

Opinions invited as to the best methods of treatment.

The patient, a girl, aged 5, had crico-tracheotomy performed, for diphtheria, two years ago.

DISCUSSION.

Dr. Dundas Grant: Has Mr. Harmer thought of the advisability of making a low tracheotomy, and allowing the upper one to subside, a very good principle to start with, as it gives the upper part a complete rest. On one occasion I found that intubation with a tube intended for a much older patient made it possible to remove the tracheotomy tube. Then there is the question of choice of tracheotomy tube. I used in one case what I considered was the best, namely Durham's, which is curved almost at a right angle. The anterior margin of the lower end ploughed into the anterior wall of the trachea, causing an ulcer, with development of granulomatous tissue which produced obstruction and led to a fatal issue. If a tube of a more obtuse angle had been used, that distressing result would have been avoided.

The President: I have a somewhat similar case in which I lined the cavity with a skin-flap about three months ago. The flap faded away, after having taken, and I have been reduced to putting in an intubation tube, anchored by a pair of midwifery forceps clips.

Mr. G. W. DAWSON: I have a similar case, in which the larynx is completely occluded. I thought of doing a laryngostomy as is practised in France. The larynx is split open, a tube inserted, and the skin stitched to the mucous membrane on each side, so that there is an open wound for some months.

Mr. HOWARTH: About six years ago I treated a similar case by laryngostomy. The after treatment is a very elaborate one and requires great patience. Dr. Chevalier Jackson has done a great many similar operations and details are mentioned in his book. I have lost sight of my case but will try to bring it before the Section.

Mr. HARMER (in reply): I hoped I might have received more suggestions. This child had a low tracheotomy performed eighteen months ago, and has steadily improved in general health; but the larynx remains completely occluded, and not even a bristle can be passed through it. The difficulty in children is how to maintain a passage after dilatation, and intubation is often difficult. I wondered whether anybody had performed laryngo-fissure, such as Moure advises, in a child as young as this patient. It might be possible to clear out the inflammatory tissue and skin-graft the site.

Case of Lupus of the Palate.

By W. M. MOLLISON, M.Ch.

S., AGED 35, was advised by Dr. Bentley to attend the throat outpatient department at Guy's Hospital. The man complained of a sore throat, especially on swallowing, for about five weeks. In 1911 he had gonorrhœa; this was followed by a sore throat, but no other symptoms of lues. His wife died of tubercle some months ago.

Examination shows "mouse-eaten" ulceration on the left side of the soft palate and uvula; the ulceration involves the tonsil and can be seen to reach down the lateral wall of the pharynx to the pyriform fossa. The nose, nasopharynx and larynx are normal. The diagnosis of lupus contracted from his wife modified by lues was made. The Wassermann reaction is positive. No tubercle bacilli found in sputum.

Mr. HARMER: I have just completed the treatment of a case like this. I treated it by the Copenhagen method: large single doses of sodium iodide internally, and an hour later ionization of the palate. The man was completely cured within a month.

Case of Streptococcal Ulcer on the Arytænoid.

By W. M. Mollison, M.Ch.

A sister at Guy's Hospital was taken ill with what appeared to be laryngitis; she had, however, in addition to loss of voice, pain on swallowing, a high temperature, and definite dyspnœa, preventing sleep. I saw her on the third day of the illness; she then had a temperature of 103° F., hoarseness and much pain on swallowing. Laryngeal examination showed a superficial ulcer on the right arytænoid, somewhat triangular in shape, covered with white slough and surrounded by a red zone. The cords were reddened but moved well. As the patient expressed herself as feeling better than on the previous day it was decided that the condition was almost certainly not diphtheritic. She was given a sleeping draught. The following day, however, as the symptoms had not abated and the ulcer appeared *in statu quo*, 20 c.c. of antistreptococcic serum were injected subcutaneously. The result was satisfactory; in twenty-four hours the temperature had fallen to normal and the patient felt well: the local condition improved and swallowing was easier. In another two days the larynx had returned to its normal condition.

Case of ? Neoplasm of Larynx.

By W. M. Mollison, M.Ch.

E. H., aged 71. Loss of voice since two months. Has had a cough for some time. Left mamma removed thirty years ago. Left false cord red and bulging over the true cord; the left cord does not move so well as the right. No tubercle bacilli in sputum.

Is this an epithelioma?

DISCUSSION.

Dr. Dundas Grant: The immobility of the left vocal cord seemed to be due to some extent to a portion of the growth which dropped between the cords, but it would be difficult to say it was epithelioma. Still, I regard it as extremely suspicious. No doubt a portion of the growth could be removed to settle it. The vocal cords themselves seemed to be fairly normal underneath the curious growth in the ventricular band. It may be a cyst.

Dr. W. HILL: I had an apparently similar case, which turned out to be a fibroma, and it was removed by laryngo-fissure. In this case I think it is best to take a small portion away, as it might be malignant.

Mr. J. F. O'MALLEY: Has the Wassermann test been done? [Mr. MOLLISON: Yes, it was negative.] It looks as if it might be granuloma, but I think it is more likely to be epithelioma.

Fibroma of the Left Vocal Cord in the Subject of Incipient Tuberculosis of the Larynx.

By J. DUNDAS GRANT, M.D.

I. D., A MIDDLE-AGED man, seen on December 2, 1919, suffering from pain in the chest and aphonia of a year's duration. There was a rounded typical fibroma of about 2 mm. in diameter attached to the junction of the anterior and middle thirds of the left vocal cord. The growth was removed by means of Whistler's forceps, and the voice was immediately restored. There was evidence of pulmonary tuberculosis, and on a recent inspection there was found a slight infiltration of the sub-glottic portion of the left cord, with an antero-posterior linear slit, also moderate infiltration in the inter-arytænoid region. The surface of the right cord is also somewhat irregular.

Fibroma of Right Vocal Cord in an Exceptionally Intolerant Subject.

By J. DUNDAS GRANT, M.D.

J. D., SEEN on December 2, 1919, complaining of dryness of the throat, hoarseness and shortness of breath. Fibroma found at edge of right vocal cord. The patient was exceptionally intolerant, and the larynx was drawn down spasmodically on every attempt to introduce the forceps. He was ordered small doses of bromide of potassium and directed to return again in a week with a view to the employment of the direct method. On his return, however, I was able to remove the growth by means of Whistler's forceps without any difficulty.

Minute Sessile Fibroma destroyed by Means of the Galvano-cautery.

By J. Dundas Grant, M.D.

J. O., a middle-aged man, attending the West End Hospital for Nervous Diseases, and referred to me on account of concomitant hoarseness. I found a small conical sessile fibroma near the middle of the edge of the right vocal cord. I endeavoured to remove it by means of forceps, but it eluded the grasp, and I was able to destroy it completely by means of the galvano-cautery. There has been no recurrence.

Small Globular Fibroma causing Extreme Hoarseness in a Voice User.

By J. Dundas Grant, M.D.

Mrs. N., referred to me on account of persistent and increasing hoarseness. I found a minute pediculated fibroma of about the size of a pin-head, growing from the junction of the anterior and middle thirds of the left vocal cord. There was also a minute nodule on the corresponding point of the right vocal cord. The patient had been occupied in reading very long type-written communications for checking. I removed the growth by the indirect method by means of my own so-called "safety endolaryngeal forceps." The voice is now excellent, and the patient is practising the familiar "pmawing" exercises devised by Holbrook Curtis.

Acuminated Sessile Fibroma at the Middle of the Right Vocal Cord.

By J. Dundas Grant, M.D.

Mrs. C., first seen on February 24, 1920, on account of loss of voice, which she said had developed suddenly fourteen days previously. There is a history of the occurrence of loss of voice on previous

occasions. The growth was gelatinous-looking and bright in colour, and it is possible that the sudden development of the loss of voice may have been due to the rupture of a minute blood-vessel. I punctured it with a fine galvano-cautery point, and the voice was at once restored. Before examination the case simulated one of functional aphonia. The patient is the subject of pulmonary tuberculosis.

Specimen of Papilloma removed from the Left Vocal Cord of an elderly Clergyman.

By J. DUNDAS GRANT. M.D.

THE patient suffered from weakness of voice owing to the escape of air producing "breathiness." The papilloma was smooth and shiny, and appeared to be œdematous. I removed the middle part of it by means of Rosenberg's punch forceps, and the other two remaining tags by means of my own and Whistler's, and finally cauterized the base with the galvano-cautery. When he was seen a few days later the voice was comparatively normal.

DISCUSSION.

Dr. ANDREW WYLIE: It would be more interesting to us if Dr. Grant had shown these cases before the growths were removed. I congratulate him on his dexterity and on the thorough way in which the removal is done; but why use Whistler's forceps, which are difficult to manipulate? Mackenzie's or his own safety forceps are excellent, or I will lend him a pair which I showed at this meeting about a year ago. Forceps should be as steady as possible. Several years ago I advocated the galvano-cautery for destroying laryngeal growths, but have given it up as I had several serious after-results; one case nearly died from dyspnœa. I have had many successful cases, but I do not think it is a mode of treatment to advise generally, except in practised hands.

Mr. CYRIL HORSFORD: I am glad to hear Dr. Wylie severely censure the galvano-cautery, because some years ago I took a similar attitude in regard to one of his cases. I am in accord with Dr. Grant in regard to Whistler's forceps. I think they are very good for such cases, because they are not very large, and they do not obscure the view: moreover, they have rounded margins, which favour delicate manipulation. My great objection to the galvano-cautery is, that one cannot limit its action. It might destroy the surface mucous membrane, but it might also so devitalize the tissues beneath that there is some scarring of vocal cords, with damage to the voice.

Dr. DUNDAS GRANT (in reply): I thank Dr. Wylie for his genial references to his old colleague. These cases seem to me to show the advantage of selection in the method of dealing with the growth, according to its position, shape and size. They also illustrate my ideas in regard to practice, but it is now too late this evening to enter into the various questions which arise. There is, for instance, the extraordinary degree of hoarseness in the patient who sat near the door, with a minute pediculated fibroma the size of a pin-head, removed by my own forceps, following which removal the voice was completely restored. She has incipient singer's nodules, and I have recommended her to practise Curtis's exercises. I shall be happy to show the forceps if members are not familiar with them.

Gumma in the Vault of the Nasopharynx causing Obstruction to Nasal Breathing and Catarrh of the Right Middle Ear.

By J. DUNDAS GRANT, M.D.

THE diagnosis was confirmed by the Wassermann's test and the swelling completely subsided under specific treatment.

Dense Growth in the Nasopharynx simulating Adenoids and producing Extreme Nasal Obstruction and Dullness of Hearing in the Right Ear.

By J. DUNDAS GRANT, M.D.

A SMALL portion removed by means of a snare through the nose was stated to consist of simple adenoid tissue. A further specimen has been sent for examination and the report is awaited. It will probably be found to be sarcomatous, or, from its extreme density possibly chondro-sarcomatous.

I should be glad to have the opinions of members with regard to it.

Dr. GRANT: With regard to the naso-pharyngeal growth, I should have liked the opinion of members. It is very dense, and seems almost cartilaginous. It may be a sarcomatous growth, with some osteophytic thickening. Apparently it grows more from the back wall than from the basi-sphenoid,

as most do. I intend to have a skiagram taken, and shall show the patient again. The microscopical examination gives a rather doubtful result. It may be a case for radium treatment.

Cyst of the Floor of the Nose.

By J. F. O'Malley, F.R.C.S.

Quartermaster-sergeant G. F. W., aged 32. Swelling below left nasal orifice since 1915. Two operations (1915 and 1919) were done under general anæsthesia and the swelling opened and drained under upper lip. The swelling still contains much pus.

DISCUSSION.

Dr. Dundas Grant: This is close to one of the incisor teeth, and I recommend that the case be skiagraphed, to see whether the root of a tooth is a cause of the trouble.

Mr. O'Malley (in reply): I wished to emphasize the point that although the cyst looks simple, a capable surgeon in Brighton made two attempts to deal with it, but he did not succeed.

Ulceration of Deep Pharynx for Diagnosis and Opinion as to Treatment.

By Cyril Horsford, F.R.C.S.

The patient has a deep irregular ulcer with firm margins occupying the right pyriform fossa, invading the right side of the larynx and the right side of the epiglottis. Little impairment of mobility of the right vocal cord and no glands to be felt. History of difficulty in swallowing for about three months. History of syphilis denied.

DISCUSSION.

Mr. O'Malley: I thought this was an epitheliomatous ulcer.

Mr. Douglas Harmer: I think this is a malignant growth, and that it is inoperable, because one has not had very encouraging results from lateral

pharyngotomy. I do not think diathermy is practicable here, because the larynx is involved. When the cartilages of the larynx are destroyed by diathermy, my experience is that perichrondritis supervenes and the patient is very miserable afterwards. Growths involving the epiglottis can be freely destroyed without damaging the larynx.

Mr. HORSFORD (in reply): My own opinion is that it is malignant disease: I did not mention that in the notes, as I did not want the patient to know. And I wanted to hear what operation would be recommended. I am obliged to those who have spoken.

Fistula and Cicatrization in connexion with ? Thyroglossal Tract.

By ARCHER RYLAND, F.R.C.S.Ed.

P. C., AGED 10. History (mother's statement): "Has had a discharge from front of neck for years. It sometimes dries up and afterward breaks out again. An operation was performed on neck two years ago. The contraction began to appear previous to the operation, but has become more marked since the operation." Larynx and base of tongue: nothing to record.

Section of Laryngology.

President—Mr. E. B. Waggett, D.S.O., M.B.

(Chairman: Dr. A. Brown Kelly.)

Caseous Rhinitis.[1]

By Dan McKenzie, M.D.

This patient, a man aged 61, came complaining of obstruction in the nose, on the right side of four months' duration, and on the left side of two weeks' duration. Pain had been present in the right supra-orbital region for two months, but had been relieved by an epistaxis a couple of weeks before I saw him. There was an ill-defined though distinct swelling of the right cheek, and a suspicion of some enlargement or bulging of the superior maxilla in this region. The patient had lost 2 st. in weight since the onset, and there was a foul-smelling discharge from the nose. On examination, a fleshy, vascular "growth" was seen inside the nose, bulging from the ethmoidal region of both sides. Its extent could not be determined, but at first I was inclined to agree with the diagnosis made elsewhere that there was a malignant growth present. But on trying to secure a piece of the tissue for the pathologist its true nature was revealed, the nasal forceps ploughing through non-resistant tissue and setting free a quantity of caseous, ill-smelling débris. Thereupon steps were taken to remove the material by means of syringeing, curetting, and the opening up of pockets, but six months elapsed before the nasal cavities were finally cleared. The patient is now well with the exception of a little discharge from the right frontal sinus. The amount of destruction has been considerable: there is a large perforation in the ethmoidal septum; the right lateral wall of the nose has been bent outwards into the antrum,

[1] At a meeting of the Section, held May 7, 1920.

but that cavity does not seem to have been opened into; both ethmoidal labyrinths have to a great extent disappeared; and a pocket was also found leading towards the orbital cavity on the left side. A skiagram (by Dr. R. Knox) showing a probe in this last extension is on exhibition.

I am indebted to Dr. Battersby Jobson for the notes on the case.

Efforts were made to obtain some information on the pathogenesis of the disease, but without success. No foreign body was found. The Wassermann reaction was negative. The tissues in the neighbourhood of the caseous material showed nothing more than inflammatory reaction with some œdema. There was no trace of capsule formation such as is found in cystic distension and the caseous material was composed only of amorphous, granular lipoids swarming with bacteria. Is the destructive process due to the chemical action of the caseous material or to pressure by its mass?

DISCUSSION.

Dr. KELSON: I showed a patient with this disease in 1904, a boy aged 9 years. It is a striking point that although this condition resemble malignant disease in the extensive destruction which takes place, there is a dissimilarity in that the condition does so well after operation, and recurrence is very rare. These cases get well if they are thoroughly cleaned out. I watched my case for years, and there was no recurrence, a history which is borne out by other recorded cases. I would like to hear more about the bacteriology: information on that point is scanty. In my own case not only were there many staphylococci found, but the Klebs-Loeffler bacillus also. As the pathology of the condition is obscure, any contribution on the point would be of interest.

Mr. W. STUART-LOW: We do not see so many instances of this disease now as we used to do. Patients were mostly middle-aged, and the conclusion I came to was that the enlargement of the nasal cavity was due to pressure, just as in cholesteatoma of the ear. The treatment found most useful was excavating the whole collection out week after week. The nasal passages were then sprayed with a weak solution of alcohol.

Mr. NORMAN PATTERSON: Dr. Kelson has referred to the likeness of this condition to malignant disease. The two conditions may be associated as was the case in an old man and a young girl who came under my care. At the first examination both were diagnosed as cases of caseous rhinitis. On further investigation there was discovered in both these cases a malignant tumour situated behind the caseous material. In the man, it proved to be a carcinoma, in the girl, a sarcoma.

Mr. ARCHER RYLAND: I have lately seen a case closely resembling this one, with extensive destruction. The antro-nasal wall on the left side was broken down and practically the whole of the wall above the inferior turbinal was deficient. Limited ethmoiditis was present, with a single polyp. After removing the polyp the patient lowered her head and blew down from the left nostril a large quantity of caseous material. By digital examination the extent and limitations of the disease were defined. The tip of the little finger passed into the antrum and could be carried forward on to the anterior antral wall. No exposed or carious bone could be felt, this region having a smooth soft covering. The findings pointed rather to destruction by chemical process than by pressure. The pathologist reported the presence of streptothrix, as well as other organisms, but the streptothrix was present in such abundance that it appeared to be the causal organism.

Mr. FREDERICK SPICER: This condition does not usually recur, but it does sometimes. Lately I have had a case, in a patient 70 years of age, in which it has recurred six times.

Dr. BROWN KELLY: I have seen about a dozen cases of rhinitis caseosa, leaving out of account those due to rhinoliths and foreign bodies. None of these cases was so marked as Dr. McKenzie's. In one, I thought at first that malignant disease of the antrum was present owing to the bulging inwards of the nasal wall and the amount of granulation tissue. Rhinitis caseosa is not, I believe, a morbid entity: but the term is suitable for a condition in which, owing to mechanical causes, the secretion is "bottled up," stagnates, and undergoes physical and chemical changes. The condition is found most frequently in association with rhinoliths, foreign bodies or sequestra, such as I think may have been present in the case mentioned by one speaker. Stagnation may also occur in one of the accessory sinuses. I have had several cases in which the frontal sinus was affected; in one the floor was bulged downwards and the posterior wall eroded. In the antrum I have found it associated with dental disease. I have seen it several times in the hiatus semilunaris, causing bulging inwards of the uncinate process and the development of granulations around the edge of the process so as to produce a closed cup-like recess. I have also met with rhinitis caseosa in the inferior meatus, where owing to deviation of the septum to the same side and enlargement of the anterior and posterior ends of the inferior turbinal the collection was walled in. In that case it led to erosion of the middle third of the inferior turbinate and of the naso-antral wall. In another case the cheesy collection was amongst polypi, but in this patient's nose there was complete stagnation owing to his inability to force air through it because the soft palate had become adherent to the posterior pharyngeal wall. Therefore, I think that the essential ætiological factor in these cases is mechanical obstruction. Once the secretion is firmly lodged it gradually becomes more inspissated until it forms a firm mass on which calcareous salts may be deposited, the presence of which excites more discharge. Much work has been done in the bacteriology

of this condition, particularly by Italians, but in none of the cases examined for me has anything been found to throw light on the condition. A strong point against its microbic origin is the fact that once the pultaceous material is spooned out and the parts well ventilated and drained, there is no further trouble. When the discharge does persist this is probably due to the presence of a small piece of dead bone or of a rhinolith.

Dr. W. HILL: I have not seen more than about ten of these cases in thirty years, and in nearly all I have found some obvious cause. In at least three of the cases there was necrosed bone and a sequestrum; in two the patients were syphilitic. From a fourth case I removed a rhinolith, which was associated with rhinitis caseosa: this case was reported in the *Proceedings* of the old Laryngological Society about twenty-five years ago. In two cases in children I found no obvious explanation. There may have been some ordinary nasal obstruction in some of my cases, but I have always sought for some more irritating factor, e.g., necrosis or a foreign body, though a specific microbic cause was formerly postulated. I was once called upon to operate on the frontal sinus of a horse which was a "Grand National" winner. I found the large frontal sinus full of caseous material: there were also present granulations, pus, and friable bare bone; the latter perhaps due to pressure. The horse could not gallop for some months before the operation, but afterwards it could, and proved a serviceable hunter, though its racing days were over.

Dr. DUNDAS GRANT: I have seen a few cases of rhinitis caseosa, and I agree with Dr. Brown Kelly that the cause is some sort of obstruction. I think it is, in a way, an accident: the turbinated body may be curled so as to form a recess, and a slight inflammation, or a foreign body, may convert it into a narrow-mouthed cavity, in which this formation is very apt to occur. In March, 1897, I referred to a case in which a piece of blotting-paper was found. In a case of Dr. Hill's there was a rhinolith. The question is whether the antrum is affected. One's first idea was that it was a kind of cholesteatoma of the antrum, but such a condition is most exceptional. In a very well marked case of caseous rhinitis which I showed in 1909, I punctured the antrum and found it clear. A caseous mass had formed in the middle meatus. As Dr. McKenzie has said, the first view of such a case suggests malignant disease, and it is usually sent to one as such. Last July I had a very marked case of rhinitis caseosa in which there was puffiness over the cheek: it was obviously an antral case. I think it had started in the middle meatus, and by pressure had caused atrophy of the inner wall of the antrum. The opening was large, but was covered by large granulomatous masses which filled up the nasal cavity. Dr. Wingrave found the tissue consisted of "cavernous spaces and glands, mingled with granulomatous elements, in which plasma cells predominated," the latter indicating that there was no malignant disease. He said the débris consisted of blood clot, amorphous granules, no shed or deformed epithelial squamæ, and only septic bacteria. It is good that such

cases should, from time to time, come before this Section, because they are very instructive, and one may be a good many years in practice without seeing them. They are most hopeful cases.

Dr. DAN MCKENZIE (in reply): We may be relying too much upon pressure as the cause of the destruction: there may be a chemical agent at work which acts as a solvent of bone, rather than a massive pressure. One or two features support the chemical idea. If there were much pressure, one would expect the caseous mass to evacuate itself easily: but in this case the material continued to form long after the pressure was relieved. I saw a pocket actually in process of formation between two visits with a fortnight's interval. There was no pressure inside that pocket, because it was open. That suggests the presence of some agent which is converting the nasal secretion into the matter which we speak of as caseous material.

Dental Abscess of the Nasal Septum.

By DAN MCKENZIE, M.D.

ON a former occasion I reported before this Section a case of septal abscess which had tracked down and opened in the gingivo-labial recess of the mouth. The present case showed the reverse state of matters, and is, so far as my own experience goes, unique.

The patient is a male, aged 19. The history was clear. A week before he came to hospital on March 1, 1920, he developed an alveolar abscess in connexion with the root of the right upper lateral incisor. After a few days this broke and discharged, but at the same time his nose became absolutely blocked so that he was quite unable to breathe through it.

When first I saw him the presence of a septal abscess was quite plain and unmistakable, and its connexion with the dental abscess was obvious from the fact that I was able to squeeze pus down from the nose into the mouth where it emerged from the opening in the gum. The septal abscess was at once incised and a quantity of pus with a "toothy" odour was set free. The septal cartilage which was crumpled, soft, and pulpy, was removed, and is on exhibition. There was no history of trauma.

DISCUSSION.

Mr. J. F. O'MALLEY: I have never seen a case of the kind when the process was active in the septum: but I have seen at least two in the last few years in which I found a sinus from the situation of the lateral incisor into that

part of the nasal septum. These cases are difficult to treat. My cases were not due to any specific disease: they were septic. There is a nasal deformity due to sagging of the septal cartilage, and I think Dr. McKenzie can improve the nasal appearance in this patient by running a small piece of cartilage from the tip of the nose as far as the nasal bone, and bringing the ridge up to the proper level.

Dr. McKenzie (in reply): I do not know whether emphasis has been laid on the particular deformity which follows removal of the septum, that is to say, a dimple just below the end of the nasal bones. In atrophic rhinitis you get the same depression, a dimple from the atrophy of the cartilaginous septum. It seemed difficult to know what to do with the cartilage in this case, but as it appeared to be dead I removed it. Recently I have seen one or two cases of septal abscess: one in a lady, in whom it followed influenza. In her case there was decided deformity, and it is best to warn the patient that deformity is liable to follow the malady independent of the treatment adopted.

Sequestrum of the Floor of the Nose with Intact Palate.

By Dan McKenzie, M.D.

The specimen is evidently a portion of the hard palate. It was removed from the back of the nose under a general anæsthetic and with very great difficulty, as it lay right across the nose, traversing the septum, which was (and remains) very greatly deflected to the right. And it was not until I managed to break the sequestrum in two that its removal was accomplished.

The history was that of nasal discharge, obstruction, and cacosmia. The Wassermann reaction was negative, and the case was at first supposed on that account to be one of rhinolith.

The interesting feature in the case is the absence of any perforation of the palate, and, indeed, even of deformity or scarring. The bone of e hard palate is intact.

Presumably the necrosis of the palate—almost certainly luetic in spite of the negative Wassermann—left the periosteum of the palate bone uninjured, and from this was formed the new bone which in the long course of many years slowly pushed the sequestrum up into the nose.

Fibroma of the Nasal Septum.

By DAN MCKENZIE, M.D.

THE tumour, which was about the size and shape of a small pea, was attached to the right side of the cartilaginous septum on Little's area. Its removal under cocaine was easily effected although it was so firmly adherent to the cartilage that I concluded it was a chondroma.

The pathologist's report is as follows: "The tumour is the size of a pea and when cut across the diameter measures 8 mm. The appearance of the cut surface is smooth, white and glistening and looks almost like cartilage. Sections were cut through the middle of the tumour and stained by Van Gieson-Curtis's method. The tumour consists of connective tissue surrounded by a covering of stratified squamous epithelium. This connective tissue is firm, non-vascular and in places strands of wavy fibrous tissue can be seen running through the tumour. In places the epithelium invaginates for a short distance into the body of the tumour but there are no cell nests or any evidence of malignant change. The tumour is remarkably non-vascular but here and there a few minute new-formed blood-spaces can be seen. Very few nuclei are found. Owing to the non-cellular character of this tumour, which would appear to be a fibroma, attempts to stain sections with the more delicate stains were unsuccessful."—KENNETH ECKENSTEIN, Pathologist to the French Hospital, London.

(Half of the original tumour and microscopic sections are on exhibition.)

A Tooth removed from a Right Secondary Bronchus.

By FRANK A. ROSE, F.R.C.S.

THE patient was a lady, aged 63. The tooth was known to be loose and disappeared during the administration of gas before any attempt had been made to extract it. An X-ray examination showed that it had entered the right lung. The right bronchus was explored by means of the bronchoscope passed through the mouth. The main bronchus was empty; there were, however, glimpses of a pale object in a secondary

posterior bronchus. No forceps were introduced because of the uncertainty. A second attempt was made with the help of X-rays; the pale object was then identified with the tooth. It was not difficult to grasp it with forceps, but a firm pull was required to dislodge it.

DISCUSSION.

Dr. IRWIN MOORE: Mr. Rose is to be congratulated on his success in this case. Those who have had experience in the removal of foreign bodies from the lungs can realize the amount of skill which must have been employed in removing this tooth from a secondary bronchus. One point of great interest and importance is that the tooth was removed per-orally and not by the lower route through a tracheotomy wound. Sir StClair Thomson[1] in 1918 reported before the Section of Odontology the case of a girl aged 10 years, who following a dental extraction, aspirated a tooth into a secondary bronchus. Two attempts to extract the tooth by the per-oral route were unsuccessful. The failure of the first attempt was due, in my opinion, to lack of suitable forceps with which to remove an incisor impacted tightly in a bronchus, and the failure on the second occasion was due to shock and respiratory inhibition caused by the unavoidable leverage on the lung in straightening out the respiratory passages during passage of the endoscopic tube. In consequence of the patient's condition the anæsthetist advised, for the time being, no further attempt. Some days later, in consultation with colleagues, a tracheotomic bronchoscopy was carried out. By this time suppuration had occurred around the tooth, which was lying loose surrounded by pus, and was easily seized and removed. Sir StClair Thomson remarked, on the authority of an American author, that the passage of a tooth and other dental débris was a common occurrence during dental extraction. From statistics which I had collected[2] from the literature on the subject, I was able to show that, although one would expect the accidental aspiration of a tooth during dental extraction to be a common occurrence, yet it very rarely occurred in this country, which showed the great skill and care with which our dental surgeons performed their work. I would like to see the title of the case enlarged so as to indicate that the per-oral method was employed, and I hope Mr. Rose will supplement his notes with more detail.

Mr. ROSE (in reply): I did not succeed in getting the tooth out at the first attempt. It lay in a secondary bronchus, and during inspiration I got a glimpse of it, but during expiration, which occupies by far the larger portion of the respiratory cycle, it was invisible, and I was doubtful whether I was on it or not. To decide that, I passed a metal probe down to the tooth, hoping to get a sensation of grating, but that gave me no help. On the

[1] *Proc. Roy. Soc. Med.*, 1918, ii (Sect. Odont.), p. 100.

[2] Discussion on Sir StClair Thomson's case, op. cit., pp. 08-111.

second occasion I had the patient on the X-ray table, and Captain Berry helped me greatly by telling me where the probe was, by means of the screen. To anyone who has difficulty with this kind of case I recommend a similar method.

A Case of Frontal Sinusitis.

By T. B. Layton, F.R.C.S.

Mr. S. was referred to me last July by Mr. Eason with a swelling of the right orbit and diplopia. In the nasal fossa no pus was seen, but the mucous membrane covering the middle turbinal was œdematous and polypoid when compared with the other side. At the operation a cavity the size of a duck's egg was found occupying the space of the frontal and anterior ethmoidal air cells. It contained polypoid masses of mucous membrane, which were thoroughly scraped away. A large opening was made into the nasal fossa and the cavity packed with ribbon gauze, the end of which was brought out through the anterior nares. This was removed thirty-six hours later. The wound was sutured in its whole length.

At the time of writing these notes I have not seen the patient since a short time after the operation, but he writes under the date of March 24: "I have a difficulty in keeping my nose quite clear . . . but the left nostril is quite as troublesome in this way as the right. My diplopia has not entirely disappeared, though it is better."

DISCUSSION.

Dr. P. Watson-Williams: It was interesting to see, in a patient who had a cavity the size of "a duck's egg," that there is so little deformity. Partly from the notes, and partly from the patient having told us that he had not suffered much from headache or nasal discharge, I conclude it was probably a mucocele rather than what one regards as frontal sinusitis. Probably the mucocele had existed longer than was suspected, and attention was not directed to it till the contents were undergoing degenerative changes, and the secretions were perhaps being more rapidly increased. There must have been also absorption under pressure, the cavity occupying the supposed frontal sinus and anterior ethmoid cells and extending downwards, as mucoceles do. I understand the anterior wall had been largely absorbed before the operation, and therefore the operation was more or less restricted to simple opening and drainage, and that no question of a Killian operation was possible. Secondly, the cosmetic result is wonderfully good, considering it was an obliterative operation with a large cavity, without retention of any bridge or plastic

flap, or anything of that kind. It makes one feel it is possible to exaggerate the importance of the bridge in getting a cosmetic result, even in cavities of this size.

Mr. ARCHER RYLAND: If this case has been one of suppurative sinusitis, and not of mucocele, as Dr. Watson-Williams has suggested, the patient has done extraordinarily well. The nasal septum is considerably deviated to the right, this making it difficult to secure adequate drainage. Mr. Layton told me that he found caries of the inner part of the upper orbital rim. This indicates in my view an infective sinusitis rather than a mucocele.

Case of Supraglottic Tumour, which proved to be a Cavernous Angeioma.

By NORMAN PATTERSON, F.R.C.S., and NORMAN PIKE, M.B.

MRS. L., aged 47. She gives the history that for five or six years there has been trouble with the voice, which sounds weak and toneless, and some difficulty in swallowing. The breathing becomes noisy at times and the patient states that she breathes better when the head is turned to the left side. She thinks that the symptoms have become rather worse lately. The patient also states that she used to weigh 9 st. 5 lb., and now weighs only 7 st. 2½ lb.

On examining the larynx a large smooth tumour of a bluish pink colour is seen to occupy the supraglottic region. It extends from the neighbourhood of the left pyriform fossa over the left arytænoid and aryepiglottic fold, hiding from sight the greater part of the larynx. The attachment appears to be pedunculated and situated on the left side. The whole tumour moves upwards on phonation.

Opinions as to diagnosis and treatment are invited.

DISCUSSION.

Mr. W. H. JEWELL: I think this is a cyst connected with the epiglottis and left aryepiglottic fold, and might be removed by suspension laryngoscopy and a snare.

Mr. W. STUART-LOW: I have had a number of cases such as this which I believe to be cystic, and found repeated galvano-cautery puncture effects a wonderful and rapid improvement, the fluid contents being evacuated and the cyst collapsing and shrivelling up.

Mr. CYRIL HORSFORD: I think it is an intralaryngeal cyst, but do not think galvano-cautery puncture will cure it. I have tried it in such a case,

and it recurred. Mr. Trotter excised it by removing a window in the thyroid cartilage. It was an aryteno-epiglottidean cyst, which spread downwards into the larynx. The operation was successful. I think an operation will be necessary in this case.

Mr. NORMAN PATTERSON (in reply): I shall attempt to deal with this tumour by suspension laryngoscopy, and at the next meeting of the Section I shall further report on the case.

Left Frontal Sinus Suppuration; Radical (Killian) Operation.

By IRWIN MOORE, M.Ch.

PATIENT, a boy, aged 16, was first seen in consultation with Dr. Thomas, of Welwyn, on February 20, 1919. On February 11, following a cold, the temperature reached 105° F., and patient complained of severe pain over the left eye and temporal region. Gradually increasing œdema of the left upper eyelid followed, along with a swelling over the centre of the forehead. There was no purulent discharge from the nose, but on February 19 a severe hæmorrhage occurred from the left side. Between February 11 and 20 the temperature had varied between 102° and 105° F. When first seen on the latter date, there was extensive œdema of both the right and left eyelids, together with a large, tense and painful swelling over the centre of the forehead. It was concluded that pus had burst through the anterior wall or floor of the left frontal sinus. On opening the left frontal sinus, which was found full of pus, no connexion with the external abscess could be traced. The right frontal sinus was opened for inspection and found healthy. A radical (Killian) operation was performed on the left frontal sinus, including removal of the ethmoid cells. Patient made an exceptionally rapid recovery due, in the exhibitor's opinion, to B.I.P. dressing.

The case is shown to demonstrate the safety with which, under proper technique, the frontal sinus may be opened externally and drained or obliterated in cases of suppuration.

DISCUSSION.

Dr. P. WATSON-WILLIAMS: I understand the boy had a large frontal sinus. Will Dr. Irwin Moore give particulars as to the way in which the B.I.P. was applied in his case and in Mr. Tilley's cases? Does Mr. Tilley pack the cavity with the B.I.P., or merely apply it and allow it to find its way out? And does he pack it with gauze?

Mr. J. F. O'MALLEY: Will Dr. Irwin Moore tell us in what way he uses the B.I.P. dressing? The usual method is to staunch the bleeding and then touch with spirit, to dry it still more, and wipe the raw surface over with B.I.P., then shut the wound up. That was the method taught and used during the war, and the method I used myself. Has Dr. Irwin Moore found a better means of application?

Dr. IRWIN MOORE (in reply): Mr. Tilley applies the B.I.P. freely to the cavity by means of gauze, and then closes up the cavity. I have been criticized for using the term "proper technique" in my notes of this case. Sir StClair Thomson in discussion has frequently referred to the danger in this operation of the "dead space" under the bridge, which is the chief drawback. In view of Mr. Tilley's experience with B.I.P. in both mastoid and sinus operations, it appears to me that, added to our usual technique in carrying out the complete radical or Killian operation, either by closure of the wound at the time of operation, or by keeping one corner open for drainage, we have a much safer procedure than we had formerly if in addition we use B.I.P. instead of merely leaving the parts to drain. In my case it was not until the third day after operation, when the wound was breaking down, that I packed the cavity on three occasions with gauze saturated with B.I.P. I was much struck with the unusually rapid healing which followed.

Baby with Depressed Bridge of Nose.

By W. DOUGLAS HARMER, M.Ch.

No abnormality was noticed at birth; the infant was breast-fed for three weeks, had marked snuffling, difficulty in breathing through the nose and occasional discharge. The bridge of the nose began to drop when the child was three weeks old and the deformity had steadily increased. X-rays show that the nasal bones are depressed, but there is no evidence of destruction or fracture. The child is well nourished, has had no rash nor enlargement of the spleen. Wassermann reactions: Mother negative, child negative.

Suggestions are invited as to the cause of the condition and its treatment.

DISCUSSION.

Dr. P. WATSON-WILLIAMS: It is difficult to form a conclusion now as to the cause, but I think the child might have had a septal abscess due to traumatism at birth, and developed the signs three weeks after that. The mother perhaps did not notice anything until obstruction was developed and a discharge came away. As to treatment, one can only wait, and see what will happen as the child gets older.

Mr. L. COLLEDGE: I suggest that Mr. Harmer leave the child alone as there is no nasal obstruction. If, when it reaches the age of 10 or 12 years, it is obviously deformed, a piece of rib could be put under the skin.

Dr. KELSON: I do not think a few hydrag. cum creta powders will do any harm to this baby.

Sir STCLAIR THOMSON: I had cases like this in the pre-Wassermann days, and I remember watching them. One caught me out. It was that of a girl of 3 years. I watched her nose slowly sinking. The mother and the other children were healthy: there was no history of miscarriages. There was only a slight atrophic rhinitis. When the child shed its milk teeth and got its second set they were most typically Hutchinsonian. We should always search for the most likely cause, and I agree with Dr. Kelson's suggestion. I would put a flannel bandage over the abdomen smeared with mercury for weeks.

Mr. J. F. O'MALLEY: To those who have seen infants of a few weeks old, with depressed nose and snuffles, this fills in the picture, but as Mr. Harmer has not discovered the usual taint here we must look for other reasons. But I do not think we can be absolutely tied down by the result of the blood test, to exclude syphilis.

Dr. BROWN KELLY: We do not always get evidences of syphilis or destruction of the septum in these cases. As to treatment, it is far too early to think of doing anything locally.

Case of (?) Laryngeal Tuberculosis.

By LIONEL COLLEDGE, F.R.C.S.

THE patient, a young woman, was sent up by a tuberculosis officer on account of a growth in the anterior commissure uniting the vocal cords. The growth was considered to be a papilloma. However as there is active tuberculosis in the lungs and some slight ulceration on the vocal cords it seems probable that the growth is really an unusual form of tuberculoma.

Note.—The Wassermann reaction was subsequently reported to be negative.

DISCUSSION.

Mr. CYRIL HORSFORD: I think this is typically tuberculous, a tuberculoma growing in an unusual situation and undergoing ulceration. The cords also suggest tubercular infiltration immediately above the tumour.

Mr. STUART-LOW: This larynx shows the result of prolonged irritation, being vascularized. The patient works in a clothing factory, and is constantly

inhaling dust. There is also marked deviation of the septum to the right and the filtering function of the nose is therefore interfered with. I would advise that she be removed from such a dusty environment and that the septum be resected. These patients derive much benefit from cod-liver oil and open air life.

Mr. O'MALLEY: I do not agree it is typically tubercular, because most of these conditions are posterior, either in the interarytænoid region, the arytænoid, or the vocal cord, in this order of frequency. Here the granulation mass is in the anterior commissure, joining the anterior ends of the cords together, with a ledge underneath, and the appearance suggests firmness. I think that there is much in the suggestion put forward by Mr. Stuart-Low. Mr. Colledge states that there are signs in the chest, but the condition of the larynx is not typical of tubercle.

Dr. P. WATSON-WILLIAMS: While I agree this looks like tubercle, I thought it might be gummatous. If the patient has pulmonary tuberculosis, that does not exclude syphilis. She says she has a constant discharge from the nose, and there is an objective odour. She does not smell anything abnormal herself. I think syphilis should be excluded.

Dr. DUNDAS GRANT: I do not think the fact of the growth being at the anterior commissure should be held to exclude the possibility of it being tuberculous. I have shown before the Section an old man with a papillomatous growth in the anterior commissure. I removed it, and the microscope showed it to be definitely tuberculous, although there was no other evidence of it being such.

Mr. ROSE: I agree in considering it is more likely to be tubercle than anything else, and that the situation is not against this. I recall the case of a young woman, aged 30, who had a small rounded tumour in the anterior commissure, which I regarded as a fibroma, and removed it in that belief, but on coming to cut sections I found it was a tuberculoma. This patient has ulceration of the cords, and that, combined with the subglottic swelling, leaves very little doubt, I think, that it is tuberculous.

Dr. KELSON: I feel sure this is tuberculous. It is another example of a common disease in an unusual situation. Perhaps the patient is sent up because the sanatorium doctor has not seen tubercle in that situation before, other parts of the larynx being unaffected. The lungs, however, are tuberculous.

Mr. COLLEDGE (in reply): I am inclined to regard this as an unusual form of tuberculosis of the larynx. The fact that she has active disease in her lung is presumptive evidence in favour of that view. The blood is being examined, but I have not got the answer yet. The expert in tuberculosis who sent the patient up considers it is papilloma, but I think it is better to leave it alone: he sent her with the idea that the growth should be removed.

Section of Laryngology.

Mr. MARK HOVELL (Vice-President) in the Chair.

Specimen and Report of a Case of Supraglottic Tumour of the Larynx.[1]

By A. L. MACLEOD, M.B.

J. D., A MALE, aged 57, was admitted to the Leicester Royal Infirmary on April 7, 1920, complaining of difficulty in swallowing for two months. He looked very ill and suffered from spasms on drinking and defæcation. April 9: An œsophageal bougie was passed and a barium fluid feed was given in the X-ray department. Patient collapsed.

A fusiform aneurysm of the arch of the aorta was diagnosed.

First seen by exhibitor on April 14, when laryngoscopic examination showed a supraglottic swelling completely hiding the right side of the larynx. The Wassermann reaction was strongly positive. It was intended that tracheotomy should be performed, but the patient died suddenly the same morning.

Post mortem: General syphilis. Two aneurysms at first and third part of arch. A tumour, slightly pedunculated, the size of a walnut, attached close up to the right aryepiglottic fold.

Pathologist's report: Carcinoma of a spheroidal-cell type.

[1] At a meeting of the Section, held June 25, 1920.

Tumour of External Nose; Degenerative Adenoma of Lachrymal Duct.

By H. Lawson Whale, F.R.C.S.

F. J., a coal porter, aged 56, had noticed for twelve years a gradually increasing swelling of the nose. No inconvenience resulted until recently, when he consulted me because the mass was encroaching on his nasal fields of vision. On March 23, 1920, I found a tumour, the size of a duck's egg, growing from the nasal bridge, leaving free the glabella above, and the lobe and alæ of the nose below. Laterally, the mass bulged so as to obstruct the nasal fields of vision, especially on the left. The tumour was uniform, elastic, non-fluctuating, and of a regular oval shape; the veins were translucent in the superficial skin, which was stretched and thin; the mass was absolutely translucent on transillumination (photographs exhibited). On palpation, the right bony and cartilaginous wall seemed intact; the left was partly concealed by the tumour. A lateral skiagram (exhibited) showed erosion of the nasal bones. Internally, the nose was natural on the right, but on the left the vestibule was narrowed by bulging of the tumour; the alar cartilages were intact on both sides.

A provisional clinical diagnosis was suggested of sebaceous cyst, dermoid cyst, or of chondroma, which since it was entirely translucent to light had probably undergone myxomatous change.

Operation, April 1: Preliminary aspiration of the tumour yielded nothing. An incision was made along the left side at the junction of nose and cheek; thence forwards to the mid-line, arching over the ala. The mass was enucleated in its capsule, which, however, was torn at one point, exhibiting a homogeneous myxomatous consistency. The incision was sewn up in its entirety, except for a fine drainage-tube between the vertical and horizontal cuts, left *in situ* for twenty-four hours. The present appearance of the patient is satisfactory both to himself and the exhibitor.

Pathological examination (section exhibited): In formalin the tumour shrank progressively, and through the rent in the capsule strings of mucoid material continuously came away. Microscopically the mass appeared to be myxomatous tissue. Mr. Herbert Perkins could for some time not decide what to call it. But finally he found some

degenerated ciliated columnar epithelium (*see* section), and suggested, as a diagnosis, advanced mucoid degeneration in an adenoma of the lachrymal duct.

Mr. LAWSON WHALE: I am not responsible for the diagnosis in this case, which is very likely wrong. It looked like a sebaceous cyst, but the pathologist said he could not be sure. He showed it to others, one of whom found ciliated epithelium in it. I saw the epithelium too, but I cannot find it again. He said the only structure lined by ciliated epithelium in this region would be a nasal duct or lacrymal sac, probably the first of these, but he thinks it may be accessory. Mr. Somerville Hastings has looked at it, and he cannot see any ciliated epithelium.

Suprasternal Swelling.

By W. H. KELSON, M.D.

A. L., AGED 12, schoolgirl, suffers from faints and headaches, but is otherwise healthy. Just above the sternum is a soft, well-defined swelling, oval in outline and about 2 in. in length: it does not move on swallowing. Her mother first noticed the swelling at the age of 4. Opinions are invited as to diagnosis and treatment.

Case of Hoarseness in a Man, aged 36.

By W. H. KELSON, M.D.

PATIENT is a packer, and fought with the Welsh Guards in the late war. He states that he has been hoarse for two years, and that since leaving the Army he has got worse. On examination the vocal cords are seen to be congested, but move fairly well; in the interarytænoid space is a well-marked thickening: post-nasal catarrh is present. Physician reports: "Slight dullness and prolonged expiration right apex; probably tubercle of right lung." No tubercle bacilli found in sputum; no history of lues.

DISCUSSION.

Dr. KELSON: With regard to my second case, his foreman, who poses as a throat specialist, diagnosed "consumption of the throat," the result being that the other workmen object to working with the man. I ask opinions on that diagnosis: it is a practical point.

Dr. P. WATSON-WILLIAMS: I suggest that this man may have nasal sinus trouble. He has a post-nasal muco-purulent discharge, which has persisted a long time. Often in these cases the lung is involved, and before this man was gassed he had some post-nasal catarrh, and since the gassing it has been constant. He is always clearing out his nose in the morning, and I suggest it might be as well to explore the sinuses and see whether any of them are infected and require draining.

Mr. HERBERT TILLEY: Last week I opened a chronic empyema of the left antrum in a young girl who has been hoarse for years and is likely to lose her employment because of this symptom. It is caused by interarytænoid hyperplasia. I hope, by cutting off the source of infection from the antrum that the laryngeal condition may improve. The operation was only a week ago, and so I do not know what the result will be.

Dr. BROWN KELLY: When this second patient shown by Dr. Kelson phonates the false cords are brought together so as to conceal the anterior two-thirds of the true cords, and the latter posteriorly leave a small triangular gap between them. This is the picture of spastic aphonia as seen in soldiers who have suffered from nervous shock[1] and I would regard the case as one of this kind.

Mr. KISCH: I think these interarytænoid cases are fairly common. They nearly always are associated with nasal suppuration. I have no doubt that it is not tuberculous.

Nasal Deformities treated by Transplantation of Costal Cartilage. (Photographs shown.)

By DOUGLAS GUTHRIE, M.D.

Case 1.—L. L., AGED 20, fell downstairs at the age of 9 and broke her nose, causing an unsightly deformity. Paraffin injections were tried five years ago, but with little success. No nasal obstruction. Operation, March 12, 1920: Incision at root of nose; small piece of paraffin dissected out; skin undermined; costal cartilage graft resected, pared to suitable shape, and inserted into nose.

Case 2.—G. E. injured his nose two years ago. There was no deformity at the time, but nasal obstruction. This was treated by operation, presumably submucous resection of septum. Six months later, bridge of nose gradually became depressed, producing great disfigurement. The appearance suggested syphilis, but the Wassermann reaction was negative. Operation, April 5, 1920: Piece of seventh costal cartilage was transplanted into nose to form new bridge.

[1] *Proceedings*, 1918, vol. xi (Sect. Laryng.), p. 195.

Case 3.—P. M. was injured in aeroplane accident in March, 1918, and suffered from nasal deformity and obstruction. Operations, April, 1920. Submucous resection of septum. Two days later, 1 in. of costal cartilage, whole thickness, transplanted into nose.

DISCUSSION.

Mr. LAWSON WHALE: Will Dr. Guthrie show us these patients in two or three years' time, because the test of plastic nasal reconstruction is time? Some of these cases look nice for two or three months, but if you see them in a few years you are upset about them. I do not know whether Dr. Guthrie lined them with skin, but that seems to be the only permanent thing to prevent subsequent shrinkage.

Dr. DOUGLAS GUTHRIE: I shall be glad to avail myself of the suggestion that these cases be brought forward again later when we may note whether the transplanted cartilage has undergone any alteration. It is said that bone, when transplanted into soft tissues, tends to shrink and become absorbed, while cartilage remains unaltered. Moreover, cartilage may be readily pared to any desired shape, and if a cast be taken before operation, the graft may be so shaped as accurately to remedy the deformity. Foreign bodies, such as celluloid or paraffin or metal supports, may produce irritation and even require to be removed, and on this account a natural tissue is to be preferred.

Dr. LE MAITRE: I have had a large experience of cases of the kind at the Facial Repair Hospital at Vichy, and I agree it is very important to use an organic splint, rather than paraffin or such-like material. The choice rests between cartilage and bone. I agree that cartilage is easily shaped, but it is more readily infected than is bone, and if infected, it is apt to come away, whereas bone can be partially infected and yet remain. I generally take the piece of bone from the tibia, entirely under local anæsthesia, which renders the proceeding quite painless. Removal of costal cartilage is more painful, and it is a little more risky to take bone from there than from the tibia. I have also helped to "confection" noses by taking a piece of vomer from the septum. I do a septal resection; I do not introduce it from the outside; I push the vomer up between the muco-perichondrium on each side, and re-make the nose from the inside with the piece of bone.

Dr. DAN MCKENZIE: With regard to the choice between bone and cartilage, has anyone heard it stated that if bone is inserted it becomes absorbed, whereas cartilage is not absorbed? I have done a small number of cases, through the incision to which Halle introduced us at the International Congress, that is, through the vestibule, and it is quite easy to make an incision into one vestibule, to effect a separation of the tissues of the bridge of the nose, as far as the root of the nose and on either side of the bridge to the cheek, and to insert a bony or cartilaginous splint through the incision in the vestibule. In that way one avoids an external scar. I have used bone,

but I think that in the future I should be disposed to use cartilage if it is found not to be absorbed. With regard to the after-treatment of those cases, this is a very vascular area, and there is apt to be a hæmatoma of the bridge of the nose, which may become infected. To avoid the hæmatoma I apply a tight dressing with collodion over the bridge. I think we, as rhinologists, ought to impress on the general practitioner the necessity for sending cases of fractured nose for treatment early, because then the shape of the nose can be restored with very little difficulty. If seen within a few hours or days of the accident, all that is required is to mould the displaced fragments back into their normal position. Once this is done there is little or no tendency for the displacement to recur, as there is no muscle acting on the bony fragments. Consequently, splints are not required. I have been able to remove deformity in this way as late as ten days after the accident.

Mr. SOMERVILLE HASTINGS: I have treated a large number of cases of this kind during the past 3½ years, and have found that my early patients, some of whom I have seen recently, have not, so far, suffered from any absorption of cartilage. Cartilage is as permanent as paraffin, which I have also used in a good many cases. Paraffin indeed may be a little too permanent, and I have seen at least two cases in which, during periods of 9 and 20 years, the paraffin injected tissue has gone on increasing in size, so that the newly formed tissue is now larger than was wished for. To reach the rib cartilage, I make a vertical incision dividing the sheath of the rectus, then split the rectus fibres, and have the two parts of the muscle retracted. I have then found it possible to get away all the cartilage which can be seen, without evil results. Although I have taken cartilage freely, not only for noses, but also for orbits and other facial deformities, I have not had any inconvenience reported by the patient, or difficulty in breathing. With regard to Dr. Guthrie's second case, I think it is open to question whether he could not have produced an even better result if he had split the septum in front and with a tenotomy knife made a pocket down to the superior maxilla, and here inserted a vertical splint of cartilage resting on the maxilla below and supporting the cartilage forming the bridge of the nose above, and thus causing a further projection of the tip of the nose, which seemed to be rather flat. It is always wise to use the patient's own cartilage. I have seen most virulent syphilis result from cartilage having been taken from another person.

Mr. E. D. D. DAVIS: I strongly recommend the crest of the ilium for unlimited bone grafts.

Dr. LE MAITRE: Bone tends to be slightly absorbed, but I do not see any disadvantage in that. I use a somewhat larger piece than necessary to allow for that. My experience is that bone blends better with the tissues and has a more artistic effect than has cartilage.

Mr. LAWSON WHALE: With regard to the question of absorption of bone or cartilage, I have had occasion to open the nose after cartilage had been

put in, that is, months afterwards, and have had it examined. It is not cartilage any longer, but fibrous tissue containing a few cartilage cells. In a case I showed here two months ago I made the nose for the man in France in 1916, and it was a good looking hard nose. With regard to bone, I had an extraordinary case in the jaw. I had taken a graft from the ramus. The man was well for a fortnight, and then developed empyema, and he died of double pneumonia and empyema in the seventh week. I removed the jaw post mortem, and Professor Keith has it now. What has happened to the graft is a synostosis at one end, and at the other end fibrous union, while in the intermediate part the bone is still living. With regard to the use of the vomer, spoken of by Dr. Le Maitre: how can that be kept in place? It can be put in, and it has many advantages, but how can you keep the bridge of the nose up? If you splint from inside, it must be done for a long time, and there is extensive sloughing, and loss of the graft.

Notes on a Case of Congenital Deformity of the Œsophagus.

By William Appleyard, F.R.C.S.

This specimen of congenital deformity of the œsophagus was removed from the body of a 16 days old female child, who died from starvation as a result of the defect. Traced downwards from its commencement, the œsophagus is seen to end blindly after a course of about $1\frac{1}{4}$ in., but about $\frac{1}{6}$ in. above this blind end is a small orifice in the anterior wall admitting a pin, which communicates with the lower part of the œsophagus. This lower part runs downwards from a congenital fistulous connexion with the posterior wall of the trachea about $\frac{1}{2}$ in. above the bifurcation; its only connexion with the upper part of the œsophagus is by the pin-hole orifice above-mentioned, which is opposite the tracheal fistula.

Mr. Appleyard: These are not uncommon deformities. On looking through the Museum in the next room, I came across an almost exactly similar case from St. George's Hospital, except that there was no pin-hole communication between the upper and lower ends of the œsophagus. The condition is, of course, incompatible with life. The only question is, is it worth doing gastrostomy? Personally, I do not think it is justifiable to condemn an infant to feeding itself directly through the stomach, even if it survives the gastrostomy. I shall be glad to let the Section have the specimen.

Intranasal Dacryocystotomy as a Help in Atrophic Rhinitis.

By HAROLD A. KISCH, F.R.C.S.

THE object of the procedure is to increase the supply of moisture to the nasal mucous membrane, by utilizing the lacrymal secretion. The operation has been performed on the right side only, so that the result can be compared with the left side. The patient states that the right side is more comfortable and easier to clean, and declares that the sense of smell is returning on this side.

Mr. KISCH : The idea of the operation was that the patients should wash their noses out with their tears. With regard to the operation, at the time, it is necessary to curette the nasal duct, so as to make the fistula permanent. In doing this operation for epiphora, the nasal duct is obstructed, but in these cases it is essential that the nasal duct should be obstructed, otherwise the fistula will close. I only did the operation on one side, so that I might see any change in the mucous membrane. I think the membrane on the operated side is of a better colour than that on the other side. Patients themselves are very pleased with the results of the operations, and beg to have the other sides done.

Perichondritis of the Larynx.

By G. W. DAWSON, F.R.C.S.I.

FEMALE, aged 32, came under my observation in September, 1918, with a history of hoarseness for four years. On examination a large white growth was seen to occupy the whole length of the right vocal cord which moved freely. The growth was removed, and proved to be papillomatous. In November, 1919, the growth was found to have recurred along with swelling of the corresponding ventricular band, this suggesting distension of the ventricle with the growth. The movement of the vocal cord was normal. In December, 1919, the swelling of the left side of the larynx and corresponding arytænoid had increased, necessitating tracheotomy. The Wassermann reaction was negative on two occasions and no tubercle bacilli present. About this time a superficial swelling appeared above and to the right of the tracheotomy tube.

During the last four months her weight has increased, whilst the Wassermann test shows a positive reaction. Lately she has complained of difficulty in swallowing. Her evening temperature rises to 99·5° F.

Perichondritis of the Larynx.

By G. W. DAWSON, F.R.C.S.I.

MALE, aged 45, had a severe cold in October, 1918, followed by aphonia. He was subsequently in a military hospital for three months for bronchitis. He recovered his voice during the summer of 1919, but in December of the same year he lost his voice again. He was first seen on January 31, 1920, when he complained of difficulty in breathing due to swelling of the left side of the larynx. A tracheotomy was performed the same evening. The Wassermann reaction was negative. A month later he had increased 2 st. in weight, although the larynx itself showed no improvement. In March a superficial swelling appeared in front of the larynx, from which a watery pus was discharged on April 9. On April 20 the discharging sinus was opened up and pus released from the posterior and lower part of the left side of the larynx, which, owing to the swollen condition of the parts, was difficult to locate exactly. The difficulty in swallowing which had previously existed has now become less.

DISCUSSION.

Dr. BROWN KELLY: Why is this case labelled perichondritis and not malignant disease?

Mr. DAWSON (in reply): There is a history of hoarseness for a couple of years, and it did not look like malignant disease when I first saw him. He had œdema of the larynx, which necessitated immediate tracheotomy. After that, he put on 2½ st. in weight. At first the Wassermann was negative, now it is positive. Through that anterior incision I got a piece of tissue from the deep part of the wound, and it was pronounced to be syphilitic.

Round-celled Sarcoma of the Nares (originating from the Left Inferior Turbinal), Fourteen Years after Operation.

By HERBERT TILLEY, F.R.C.S.

J. B., AGED 56, male, noticed nasal obstruction on the left side in 1905. Slight yellow discharge occasionally tinged with blood.

December 6, 1905: Operated on by Mr. Stephen Paget in the Middlesex Hospital.

September 27, 1906, at University College Hospital: Examination revealed polypi of ordinary type in the upper and posterior region of the left nasal fossa and of a reddish, nodulated mass connected with the inferior turbinal. The growth bled freely when probed. Mr. Raymond Johnson made an incision along the left side of nose, curving round beneath the vestibule, and then completely dividing the upper lip in the middle line. The soft parts were reflected outwards and upwards, and the lower parts of the ascending process of superior maxillary bone removed. The antrum was found to be free from any growth, but that which involved the anterior three-fourths of the inferior turbinal was removed and the facial wound sutured into position.

January 23, 1907: Recurrence of symptoms and wound reopened. A growth was found on the posterior end of the inferior turbinal which spread round the posterior end of the vomer and blocked the right choana.

April 11, 1907: Recurrence of growth removed from region from which growth was removed at previous operation.

May 22, 1907: Mr. Raymond Johnson kindly referred the patient to me, and on opening up the wound again I found a growth involving the posterior end of the vomer. This was completely removed and the external wound sutured. The patient has had no further recurrence and it is now more than fourteen years since the first operation.

Case of Abnormally Large Superficial Veins of Tonsils.

By J. F. O'Malley, F.R.C.S.

Private W. F., aged 18. History of occasional attacks of sore throat. The tonsils are hypertrophied and the superficial veins unusually large.

Large Cavernous Angeioma of Larynx.

By Norman Patterson, F.R.C.S., and Norman Pike, M.B.

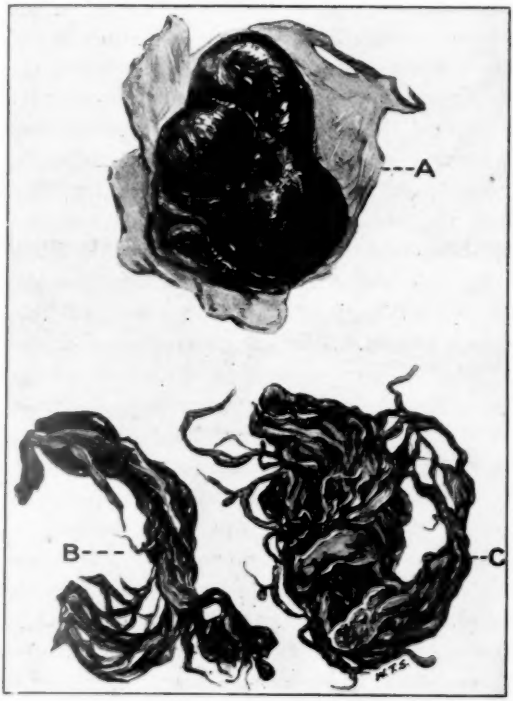
Shown at previous meeting on May 7; the patient is now exhibited again after operation.

Specimens shown: Tumour and microscopic slides.

Mrs. L., aged 47. She gives the history that for five or six years there has been trouble with the voice, which sounds weak and toneless,

and some difficulty in swallowing. The breathing becomes noisy at times and the patient states that she breathes better when the head is turned to the left side. She thinks that the symptoms have become rather worse lately. The patient also states that she used to weigh 9 st. 5 lb., and now weighs only 7 st. 2½ lb.

On examining the larynx a large smooth tumour with slightly lobulated surface and of a bluish-pink colour is seen to occupy the



A, angeioma cavernosum of the left pyriform fossa and aryepiglottic fold. Viewed from the free surface. The mucous membrane removed with the tumour is spread out around it. B and C, blood-casts from the bronchi resulting from post-operative hæmorrhage, which were extracted from the tracheotomy wound. Natural size. (Mr. Norman Patterson's and Dr. Pike's case).

supraglottic region. It extends from the neighbourhood of the left pyriform fossa over the left arytænoid and aryepiglottic fold, hiding from sight the greater part of the larynx. The attachment appears to be pedunculated and situated on the left side. The whole tumour moves upwards on phonation.

Operation, by Mr. Norman Patterson (at the request of Dr. Norman Pike), kindly assisted by Mr. E. D. D. Davis: The patient was anæsthetized by Mr. James Lyle, and the suspension apparatus introduced. A very excellent view of the tumour was obtained. In front of it a small chink was visible between the anterior extremities of the cords, and through this a catheter was introduced and intratracheal ether administered. The capsule of the tumour was seized with a volsellum, traction applied, and the attachments carefully examined. Instead of being pedunculated, as it appeared to be on laryngoscopic examination, it was found to have a very broad base of attachment. Externally the attachment reached the lateral wall of the pharynx on the left side and extended from a point opposite the posterior border of the arytænoid as far as the edge of the epiglottis. From here it descended to the left ventricular band, and passing backwards it became attached to the whole of the upper surface of the arytænoid. The tumour was dragged somewhat forcibly upwards and the mucous membrane surrounding its base was divided with scissors. It was then gradually severed from its attachment mainly by the use of scissors curved on the flat. The hæmorrhage after the removal of the tumour was persistent but not excessive, and a great deal of time was spent in applying pressure. There were few visible bleeding vessels, but general oozing continued for a long time. Eventually it seemed safe to remove the tracheal catheter. The patient was returned to bed. A few minutes later I was hurriedly summoned. I found her cyanosed and with respiration completely stopped. An immediate laryngotomy was performed, but even then the patient showed no signs of breathing. I turned her over face downwards. introduced my finger into trachea and extracted a large blood-clot. Breathing eventually was started. With the exception of a tracheitis which developed on the second day the patient has made an uneventful recovery. As events proved, it would have been wiser to perform a preliminary laryngotomy.

Pathological report, by Dr. Hubert M. Turnbull, Director of the Pathological Institute, London Hospital: Aged 47. Naked-eye examination: A portion of tissue, 5 cm. in diameter. The periphery is composed of a thin sheet of smooth, grey mucosa. The centre is occupied by an oval mass, measuring 4·5 cm. by 3 cm., and 1·8 cm. at its greatest depth. On the mucosal surface the mass rises to a height of 1 cm. The smooth mucosa extends without interruption over it. Here beneath the slaty grey mucosa a deep red purple colour is visible.

On the under surface the mass projects to a depth of 0·8 cm. This surface is covered by an uninterrupted, slaty grey membrane, to which small fibrous tags are attached, beneath which a deep red purple colour is again visible. On section much thick red fluid blood escapes from the mass. The cut surface is of spongy texture, being composed of spaces which are full of blood and separated from one another by narrow trabeculæ. Microscopic examination: The tissue is covered by squamous epithelium, which is surmounted by a very narrow stratum lucidum. In the tissue immediately beneath this are a small lymph-adenoid nodule and a mucous gland. The remainder of the tissue consists of a net of narrow fibrous trabeculæ separating very wide spaces which are full of blood. The spaces are lined by endothelium. The trabeculæ are formed chiefly of stout collagen fibres. They contain arteries and arterioles, dilated capillaries, a very few veins, a few nerves and, in places, elastic fibres and bundles of involuntary muscle. They show many small areas of infiltration with lymphocytes, plasma cells and occasional eosinophil leucocytes. The muscular fibres in some trabeculæ lie beneath the endothelium and form a circular, usually interrupted, sheet round the blood space; in other trabeculæ they lie deeper, in the form of irregularly directed bundles. The elastic fibres are arranged round some of the spaces in a manner closely similar to that in the walls of veins. Superiorly, the blood-spaces reach to within a close distance of the epidermis, and there is no sharp differentiation between the spaces and dilated capillaries in this subepidermal zone, nor between the trabeculæ and the subepidermal fibrous tissue. Inferiorly, the blood-spaces terminate in an even, curved line, the stout collagenous fibres which enclose them being fairly sharply defined from the subjacent, more delicate, areolar tissue. The above characters of the structure of the tissue show that the tumour is clearly not a vascular granuloma, but that it is a true neoplasm: a cavernous angioma composed of atypically walled veins and of capillaries.

DISCUSSION.

Dr. IRWIN MOORE: The exhibitors have recorded this rare and important case in a very thorough way. It should be an example to many of us. I have recently met with great difficulties in searching the literature and compiling my paper on "Angeiomata of the Larynx" (read to-day), because of the careless way in which so many of the cases have been recorded. Mr. Patterson kindly gave me permission to have a coloured drawing made of a microscopical section of the growth he removed, which I now show you on the epidiascope

as a typical cavernous angeioma of the larynx. The object of my paper is to arrive at an opinion as to the best methods of dealing with these growths and their several forms and varied positions. Clyde Lynch[1] (New Orleans) in 1916 suggested, in discussion on a case I have referred to in my paper, that suspension laryngoscopy would prove an ideal method of removing certain forms of angeiomata. We know it is easy to remove aryepiglottic or supraglottic growths by this means, and I think that for angeiomata in this position suspension laryngoscopy is indicated and offers a safe procedure so long as a preliminary laryngotomy is resorted to. This conclusion, to which I have come, has been confirmed by Mr. Patterson's recent case.

Mr. E. D. D. DAVIS: I had the pleasure of seeing this operation, and I was surprised at the amount which could be done by suspension laryngoscopy.

Laryngeal Case for Diagnosis.

By H. BUCKLAND JONES, M.B.

F. W. H., MALE, aged 25, demobilized soldier. He had nothing wrong with his voice till he was gassed, in March, 1918, and since then he has been unable to speak above a hoarse whisper. On examination of the larynx, a large globular swelling is seen on the right side apparently above the ventricular region and almost completely hiding the cord. On the left side there is a similar swelling smaller in size but on the same plane, and beneath it another involving the ventricular band. A glimpse only of the bases of the cords can occasionally be seen on phonation and they appear to move freely. There is no adenitis. Opinions are desired as to the nature of the growths and the best method of removal.

Specimen of Polypus of the Tonsil removed from a Female Patient, aged 20.

By H. BUCKLAND JONES, M.B.

IT is shown in order to elicit opinions as to its pathogenesis.

[1] *Trans. Amer. Laryng. Assoc.*, 1916, p, 199.

Case of Pemphigus of Mouth, Fauces, and Epiglottis in an Elderly Man.

By Sir J. DUNDAS-GRANT, K.B.E.

WHEN seen on June 3 the patient complained of pain on swallowing, which had been severe for six weeks. He had had ulcerated patches on the cheeks, gums and palate every week for five years. These patches —as described by Dr. Hern—were of "about the size of a shilling, of red colour, the mucous membrane raised by lymph effused under it, there being a distinct period of invasion during the formation of the bulla (three or five days) subsiding in seven to ten days, when the mucous membrane would peel off as a thin, greyish-looking membrane, leaving a spot extremely sensitive to the touch and to hot or cold substances. As soon as the mucous membrane had re-formed the same process was repeated."

On inspection there were seen to be several irregular patches covered with a thin adherent and transparent membrane surrounded by a red areola, while on the lingual surface of the epiglottis there was on the left half a large bleb and on the right a small one. At present the blebs on the epiglottis have disappeared, though the mucous membrane is still a little tumid. There are only two other patches, one on the right alveolar process and another on the left.

Since June 3 the patient has been using a powder consisting of anæsthesin 1 dr., orthoform 1 dr., and sugar of milk 2 dr., three times a day, and he states that he has been able to swallow very much better. His diet has been almost entirely milk, but he has during the last fortnight been able to swallow some soft solid food, and has gained 5 lb. in weight. The development of the ulceration has been diminished or at least retarded.

It is proposed to give him arsenic with small doses of opium.

The bacteriological examination revealed long chained streptococci, a few staphyloccoci, and a few *Bacillus coli*.

DISCUSSION.

Dr. IRWIN MOORE: Sir James Dundas-Grant desires me to say that he is prevented from showing this case to-day. Since the patient was seen in the Museum Room yesterday by a number of members their opinions will be welcome.

Mr. G. W. DAWSON: I have seen such a case with the pemphigus occurring on the tongue, epiglottis and neighbouring parts. Blebs come and disappear quickly, and it is not at every examination they can be seen.

Sir STCLAIR THOMSON: I have had a few such cases: they are very rare, and all tend to end in death, especially when they invade the larynx. I watched an old colonel similarly afflicted die slowly: he took two or three years. I saw one case limited to the mouth and pharynx in an American who was travelling in this country. He got well. In the present case the disease has got as far as the surface of the epiglottis. As it has existed five years, I think we should be prepared for a serious ending.

Cases of Intrinsic Cancer of the Larynx after Operation by Laryngo-fissure.

By Sir STCLAIR THOMSON, M.D.

Case 14.—Captain H. O. (private case-book, No. 13). Hoarseness for twelve months. Cupped infiltration of posterior half of right vocal cord. No immobility. Preliminary removal of portion showed epithelioma. Laryngo-fissure in 1913, seven years ago (Mr. Evans and Mr. Layton were present at operation).

Case 14*a*.—Mr. F. H. (private case-book, No. 17). This gentleman is now aged 75. He has three times been shown before the Section—before operation, November 6, 1914, and twice since. The left vocal cord was removed on November 12, 1914, and as the pathologist reported that I was not wide of the disease, it was repeated on November 20. He has remained well for five and a half years (Drs. Broeckaert, Goldsmith, Paterson, Woakes, Davis and Rylands were present at the operation).

Case 14*b*.—Commander W. E. R. M. (private case-book, No. 31). Hoarseness for one and a half years. Right vocal cord absolutely immobile. Laryngo-fissure March 12, 1917. Was shown before the Society on March 1, 1918 (Messrs. Dawson, Goldsmith, and MacLeod were present at operation).

Case 14*c*.—Mr. R. P. (private case-book, No. 34). This gentleman is now aged 78. The right vocal cord, which was quite mobile, was removed three years ago (Mr. O'Malley, Mr. Charles Ryall, Mr. King Wilson, and Mr. Cathcart assisted).

Case 14*d*.—Mr. C. T. (private case-book, No. 36). After six months' hoarseness a semi-pedunculated growth was found on a mobile right vocal cord. A portion was removed through the mouth and pronounced to be epithelioma. Laryngo-fissure in King's College Hospital two years ago (Dr. Douglas Guthrie and several of the staff of the Queen's Hospital, Sidcup, may remember seeing this operation).

Man, aged 61, Six Years after Operation through the Side of the Neck for Extrinsic Cancer of the Larynx.

By Wilfred Trotter, M.S., and Sir StClair Thomson, M.D.

This patient was shown before the Section on February 2, 1917, and again on March 1, 1918. In February, 1914, he presented himself with a reddish, slightly cauliflower growth of the left aryepiglottic fold, well limited and only extending a little way down towards the pyriform fossa. The Wassermann reaction was negative. Under cocaine a good portion of the growth was removed by the indirect method and the growth reported to be a squamous-celled carcinoma. One gland was felt under the sternomastoid, just behind left angle of jaw. The case was transferred to Mr. Trotter, at University College Hospital.

First operation, March 31, 1914: Glands removed from left side of neck and found reaching from base of skull to below clavicle. Sternomastoid removed with glands. The thoracic duct was cut and ligatured. This was followed by leakage of chyle into wound, which was a long time in healing.

Second operation, June 9, 1914: Somewhat low tracheotomy. Larynx approached through old wound in neck. Left ala of thyroid cartilage removed. The growth was found to be limited to the left aryepiglottic fold and was about the size of a threepenny-bit. This was removed with a clear area of a third of an inch all round. Pharynx sutured with catgut and drainage tube inserted. Tracheotomy tube was left in for two days. Discharged from hospital on July 3.

Although subject for many years to chronic bronchitis, and a heavy smoker, the patient remains well, with a good rough voice. In spite of the removal of the left arytænoid and the left thyroid cartilage, it is remarkable that there is no glottic stenosis. There is a good fixed cicatricial band on left side, which acts as a vocal cord.

DISCUSSION.

Dr. LE MAITRE: I am astonished at seeing these remarkable cases, because I was brought up with the idea that cancer of the larynx meant total laryngectomy. I travelled with Sir StClair Thomson in America for a number of weeks, and though he mentioned these cases, I did not regard his remarks seriously. I shall now be on the look-out for cases suitable for this operation.

Mr. G. W. DAWSON: I saw Sir StClair Thomson operate on this case; the disease had gone further than he expected, making quite a large hole in the thyroid cartilage. It would be interesting to have an expression of opinion as to how far advanced the disease can be in order to negative an operation of this nature.

Dr. IRWIN MOORE: Why does Sir StClair Thomson now remove the thyroid cartilage in all cases of thyro-fissure? If the cartilage is healthy, why should it be removed when it is believed to act as a barrier against re-infection or recurrence? Its removal for lateral pharyngeal growths such as those of the pyriform fossa is not a parallel case, because malignant disease of the pyriform fossa in its early stages lies close up against the cartilage, which is involved early, whereas in the majority of cases of intrinsic cancer of the larynx the disease commences at a considerable distance away, and may take years before it attacks the cartilage. It appears to me to be an important question to consider, whether we are justified in removing the thyroid ala in intrinsic cancer affecting the vocal cord, unless the growth has spread rapidly and involved the perichondrium or cartilage.

Sir STCLAIR THOMSON (in reply): In the case of the naval officer to which I referred in my paper of yesterday,[1] there was complete fixation of the vocal cord, and therefore I was not quite unprepared for the fact that the disease was fairly deep. On peeling back the muco-perichondrium, I found that the cartilage had gone to pieces. I said at the time that it was not a cheerful outlook, but I removed all the thyroid cartilage, and I am glad to see that it is now three years ago. Such cases are rather on the borderline between laryngo-fissure, pure and simple, and even a little more than removal of the ala. Mr. Trotter can answer for himself with regard to the case of extrinsic cancer of the larynx, which I handed over to him. I remove the ala, first to get a better view, and, secondly, because it leaves a freer glottis. In former years, when I left the ala I was once or twice disappointed in finding some narrowing. I do not know whether it is the cartilage which is a barrier to the spread of infection, or whether it is the pericondrium: I would like to ask Mr. Trotter the question. In the naval gentleman the cartilage was removed because it was infected. I see no harm in removing it in order to get a better view, and patients are left with an equally good voice.

[1] "Impaired Mobility of the Affected Cord in the Diagnosis and Prognosis of Intrinsic Cancer of the Larynx," *Proc. Roy. Soc. Med.*, xiii (Sect. Laryng.), p. 203.

Mr. TROTTER (in reply): The statement is frequently made that the cartilage acts as a barrier, and that the intrinsic cancer owes some of the relative lowness of its malignancy to this fact. I do not altogether agree with this view. When we compare the behaviour of a growth on the vocal cord with that of one in the pyriform sinus, we find that while the former does not penetrate the cartilage readily, the latter does so almost always and at an early stage. This difference, to my mind, depends on two factors, an anatomical and a pathological one. The anatomical factor is that the cord growth is not primarily in contact with the cartilage, and has to penetrate a certain thickness of soft tissues before reaching it, while the pyriform sinus growth is from the first close to the cartilage and able to attack it at once. The pathological factor, which is in my opinion the more important one, lies in the different malignancy of the two growths. This very obvious and striking difference in malignancy seems to me to be due to differences in the epithelium from which the two growths arise—the cord growth from a highly keratinized epithelium, like that of the skin, the pyriform sinus growth from an epithelium in which keratinization is much less marked. The highly keratinized cell is probably less vital and less amœboid, less able to enter the lymphatics and to travel there than is the cell in which keratin is less developed. Hence the relatively low malignancy of growths of the vocal cord and of the skin as compared with growths of the tongue and pharynx. Epithelioma of the lip, again, seems to be less malignant when it arises on the cutaneous surface than when it arises on the mucous membrane.

Section of Laryngology.

SECOND ANNUAL SUMMER CONGRESS.

Thursday and Friday, June 24 and 25, 1920.

Mr. MARK HOVELL, Vice-President, in the Chair.

Total Laryngectomy.[1]

By Sir CHARTERS SYMONDS, K.B.E., C.B., M.S.

(ABSTRACT.)

THE diffusion of a knowledge of the first stages of intrinsic cancer of the larynx should lead to early operation and it is consequently to be hoped that before long total removal of the larynx will disappear from the list of operations. The success attending local removal through laryngo-fissure renders it necessary to confirm the extent of the disease in many cases by such a preliminary exploration. I hold that unilateral disease, with fixation of the cord, is not sufficient ground for total removal. In the six cases to which reference has been made the disease either involved the whole interior or penetrated the cartilage and involved the muscles. When unilateral disease exists, even with enlarged glands and involvement of cartilage, hemilaryngectomy affords a satisfactory immunity, and a case I show demonstrates this. Where the larynx is involved by extension from a primary laryngeal disease I do not think operation worth while. In the operation I attach great importance to the omission of a preliminary tracheotomy, either some days beforehand, or as a first step in the operation. I complete all the external steps of the operation while ether is being administered in the usual way. Then the trachea is divided, sutured to the margins of the incision, and chloroform administered by Junker's apparatus.

[1] This paper is published, *in extenso*, in the *Journal of Laryngology, Rhinology and Otology*, 1920, xxxv, 9, p. 257.

In removing the larynx I attach great importance to the division of the pharynx close to the thyroid cartilage, so as to secure as small a gap as possible. Any infected mucosa can then be removed. The use of a fresh scalpel after division of the trachea, and the packing with gauze to prevent infection from laryngeal secretion, are points of importance. I also attach importance to the drainage, after closing the pharynx by the triple row of catgut sutures. A pad of four layers of gauze is laid over the sutured pharynx, a small gauze drain between the trachea and œsophagus, and a gauze-covered rubber tube is placed below the hyoid. In this way all danger of mediastinal infection is avoided, should the wound have become contaminated by the laryngeal secretion or should the sutures in the pharynx give way. Seeing that saliva must be swallowed from the first, I see no reason for withholding water as soon as the patient asks for it. When the muscles or glands are involved my practice is to remove the lobe of the thyroid gland, and clear the whole lymphatic tract with the internal jugular vein. Short notes of six successful cases are recorded, four of the patients having survived from nine to twenty-two years. One aged 70 was operated upon eighteen months ago, and the last three months ago. Two patients are shown to the meeting, and their voices can be heard by everyone.

DISCUSSION.

Dr. W. HILL: How does Sir Charters Symonds deal with a case with considerable laryngeal obstruction. Would he not resort to tracheotomy? Or would he intubate with a rubber tube? I gather he has dispensed with tracheotomy as a rule: does he avoid doing this in every case?

Sir CHARTERS SYMONDS (in reply to Dr. Hill, before having to leave): It is most important, in my opinion, to dispense with tracheotomy, and if a patient can breathe well enough to submit to operation, then, as a rule, you can get through the case without doing tracheotomy. "Condemn" is a strong word to use, but I go so far as to condemn tracheotomy which is performed three or four days beforehand: I regard this as quite unnecessary. Otherwise, I do not say one should not do tracheotomy, but without a tracheotomy the operation goes so much better, especially the external part of it. I have myself had to do tracheotomy beforehand, but, on the whole it is better to avoid it if we can.

Sir ROBERT WOODS (Dublin): I performed my first laryngectomy in 1893. It was not a success, the patient succumbed to bronchitis from inhalation of food stuffs after two or three weeks, and I gave up the operation for many years. It was not until Gluck introduced his method, particularly in regard to the feeding tube, that I again resorted to it. Since then I have had twenty-five

cases, and with that experience I may claim to say a word on the subject. Success in this operation is largely a question of the manipulative treatment of the cases afterwards. Details assume an importance in these cases which is certainly very high: therefore if I speak with regard to post-operative treatment of the cases I hope my remarks will not be considered to be out of place. Before closing the wound iodine should be freely applied. I have for a long time abandoned packs: I stitch up the pharyngeal wound, then the skin wound, draining the intermediate cavity, which should be reduced to the smallest possible dimensions by mattress sutures. Drainage from the pharynx is a matter of great importance, because I confess I have found more trouble from saliva swallowed by the patient forcing its way into the wound and separating the surfaces than Sir Charters Symonds seems to have done. In order to get over that undesirable occurrence I adopted, in my last cases, a device which, I think, is worth mentioning. That is, I inserted a drainage tube into the pharynx alongside the feeding tube, and stitched it to the feeding tube. The saliva was delivered on to the surface of the skin. When the wound was healed, I cut that stitch, and I was a little puzzled at first as to how it could best be undone: but by getting a small trephine-shaped cutter with a cutting edge which exactly fitted the drainage tube, I slipped it along, without running any risk of wounding any of the tissues. It is a very simple device for severing the stitch deep down. The trephine sticks close to the drainage tube, and cuts the suture immediately outside the tube. I heartily endorse what Sir Charters Symonds said about the anæsthetic: ether, to start with, is the right thing. But the author did not say anything about giving a preliminary injection of atropine. I think that is important in all operations about the air passages. There is one sign which I regard as a very strong indication in intrinsic cases for total extirpation in preference to attempting to get away the tumour by means of a median thyrotomy, and that indication is fixation of the vocal cord. If in an intrinsic case the vocal cord is well fixed, I regard that as a reason for doing total laryngectomy. Simple removal of the cord is very likely to prove an incomplete operation. In some cases in which I did median thyrotomy, I had to perform the total operation afterwards, where, had I allowed due weight to this sign, I should have performed total extirpation in the first instance with better prospect. With regard to the speaking capacity afterwards, that is a matter of the greatest importance, and one to which I have given a good deal of attention. It may be within the memory of some of the older laryngologists here that, some years ago, a patient travelled round to different places in the British Isles. I saw him in Ireland. He had been operated upon by Dr. Solis Cohen, of Philadelphia. He astonished everybody by the facility with which he could talk: he had a loud voice, and could make himself heard in a large hall. At first it was a puzzle to some, but it became clear that the man's method was to gulp down air into the œsophagus and then allow it to escape upwards again in a belch, seizing on that moment to articulate. After a time he became so expert that he concealed the mechanism by which he produced the sound, and his voice seemed to flow in

continuous speech. With that case in my memory, I have educated some of my patients to do the same, and with the most gratifying results. But success needs intelligence on the part of the patient. Some of my private patients I have had the satisfaction of hearing deliver speeches, talking through the telephone, and speaking as plainly, and almost as loudly, as I am talking now. The process of learning is generally slow, but in one case the patient succeeded after only half-an-hour's instruction. The devices one has to resort to in order to teach patients are various. One of mine is to administer a dose of bicarbonate of soda, followed by a neutralizing dose of citric acid, with the evolution of CO_2; the moment of belching being utilized for articulation.

Sir WILLIAM MILLIGAN: The remarks of Sir Charters Symonds have put new heart into one, because many of these cases do not do so well as his results have shown is possible. My experience is not so great as that of Sir Robert Woods: I have operated in eleven of these cases, and the patient in my best case lived ten years after the original operation. It is only a few weeks ago that he was admitted to the Manchester Royal Infirmary, and was then complaining of hydronephrosis: he only lived a few days. A post-mortem examination was held, and he was found to have a carcinoma at the base of his bladder. It is an interesting point as to whether that carcinoma could be regarded as a recurrence of the old disease, or as a fresh new growth altogether. With regard to the operation itself, I do not think there is any particular difficulty in carrying out the operation, as its technique is comparatively simple. But the details concerned with the after-treatment are very important, and it is just those details which make all the difference in the success and future of the case. I agree that if possible to avoid it, it is not advisable to do a preliminary tracheotomy. Of course, there are cases in which a preliminary tracheotomy obviously must be done: but a good working rule is, where there is no obvious necessity for it, do not do it. Another point—which neither Sir Charters Symonds nor Sir Robert Woods mentioned—is that no operation of this sort should be entertained until the patient has been thoroughly examined with the œsophagoscope: this I regard as very important. It is surprising, at times, how very much more extensive one finds the disease to be than one could possibly be led to assume from an ordinary laryngoscopic examination. I have operated upon cases which I have had to close up at once on account of the extensive downward prolongation of the disease. A case such as that is unsuitable for this operation. The result of such examination should, to some extent, determine whether operation should be undertaken at all. If the disease is fairly limited, the prospects are comparatively good. The complication which has given the most anxiety is respiratory, and the particular type of trouble has been not septic pneumonia, but acute œdema of the lungs. Whether that has anything to do with the anæsthetic, I do not know, but it has occurred in two or three of my cases: one was very rapidly fatal from acute œdema of the lungs. But the administration of anæsthetics was done as Sir Charters Symonds has described, that is, ether was given at the first stage

of the operation, and when the trachea was opened and stitched to the skin, the anæsthesia was continued under chloroform. I would have liked to hear from the author more details as to his technique so far as the suturing of the mucous membrame is concerned, because that is the difficult part of the operation. It is my experience that in some cases it is impossible to do it, and in those cases some form of drainage is essential. I have tried gauze, but I think tubes are better. And wherever I cannot suture the mucous membrane together—and the occasions have been fairly frequent—I have used drainage tubes.

Dr. LE MAITRE (Paris) (translated by Sir StClair Thomson): I will limit my remarks to two points: the question of preliminary tracheotomy, and the question of the anæsthetic. I think that preliminary tracheotomy—i.e., done fourteen to twenty-one days beforehand, has advantages, because, although there is risk of the bronchi becoming catarrhal, they, on the other hand, become tolerant, and, so to speak, accustomed to do their work through the opening into the neck, instead of with the air passing in the usual way. Therefore I am in favour of preliminary tracheotomy. With regard to anæsthesia, my last four cases have been operated upon under local anæsthesia, namely, with novocaine. They were painless. This is an advantage, because of the remarkable absence of shock after the operation in these cases. The patients were in no way collapsed, and in a few days they were in normal conditions and there was no need to have recourse to caffein or other stimulants.

Dr. VAN DEN WILDENBURGH: I have no experience of preliminary tracheotomy in these cases. My first three cases of total laryngectomy I operated upon under general anæsthesia, and one of them died. I have had seven other cases, the operation in some taking two hours, because of the time occupied in removing the glands in the neck. I only gave them a little morphia before.

Sir JAMES DUNDAS-GRANT: There is one very simple question which arose among those who dealt with these cases years ago. It was this: Is it not better to be dead, than to be alive after a total extirpation of the larynx? We have seen to-day that the operation can answer that question most satisfactorily. Such a series of cases as those of Sir Charters Symonds unquestionably proves that laryngectomy should be more frequently performed than it was when several of our pioneers dazzled us with statistics of wonderful results achieved by thyrotomy. The figures were obtained by a selection of cases, and it is a question as to what happened to the people whose cases were not considered favourable for the operation of thyrotomy. There are certainly cases in which simple thyrotomy is sufficient, and it is strange that some of the large works on the surgery of the larynx have left out mention of it altogether. In Loeb's great handbook complete laryngectomy is described as the only operation for this disease. The results of thyrotomy, in my own and others' cases, show that it has a place which it is absurd to deny it. There are, on the other hand, cases which are obviously unsuitable for thyrotomy and in

which complete laryngectomy is called for. But there are also intermediate cases, and it is these we should have constantly in mind. I have more than once regretted, when doing thyrotomy for intrinsic carcinoma, that I had not obtained beforehand the permission of the patient to do and carry out complete laryngectomy if necessary. In the case before us there is a peculiar appearance, which you can see by looking below the commissure—namely, what was a rounded fibroma-looking growth different from the rest of the epithelioma, and projecting like a polypus into the larynx. It is shrivelled up here, but was much larger before the removal. There was epithelioma on both sides of the larynx, and so extensive that it was clear what should be done in the case. In a case I lately operated upon there was also one of these fibromata, a red rounded growth, along with the epithelioma. A very much more complete extirpation of the interior of the larynx was required than was expected, and it would have been better to have done total laryngectomy. I think this rounded fibroma is an index of extensive invasion of the cartilage. It is not right to generalize on two observations, but they are so apposite that I think they cannot be neglected. In such cases before doing thyrotomy I should ask permission to remove the whole larynx if necessary. Sir William Milligan is right in regard to examination of the œsophagus. I have had such an experience as he relates; I thought I was to deal with a limited post-cricoid epithelioma, and proceeded to detach the larynx, but found extension of the epithelioma for a long way down between the larynx and the œsophagus. With regard to the stitching of the mucous membrane of the pharynx, that is a difficulty, but an attempt should be made to do it by the Lemberg method when possible.

Dr. KILLEEN (Chicago): I hardly feel competent to discuss this question. In Chicago we have been in the habit of turning these cases over to the general surgeon. Recently, however, a few laryngologists are beginning to undertake them, but though I have seen a number of these cases, the surgical results as a whole have been very poor. I think this must be due to differences in technique—more recently we are doing better with these cases than we used to. In New York, especially at Manhattan, they are doing more surgery of the head and neck. I hope that from now onwards we in Chicago will do more of these major laryngeal operations ourselves.

Dr. IRWIN MOORE: I would like to draw a comparison between the results of complete laryngectomy thirty years ago with the striking record which Sir Charters Symonds has presented to us to-day. Recently I came across the statistics of this operation up to the year 1891, a total of 163 cases collected from the literature by Tauber,[1] of Warsaw, who made a critical study of the subject. The figures showed that 67·41 per cent. died from the operation itself, or from complications within the next few days; 47 = 28·8 per cent., died in the year that the operation was performed; 32 = 19·6 per cent., were

[1] "On Extirpation of the Larynx: a Critical Statistical Study," *Arch. f. klin. Chir. u. Langenbeck*, 1891, xli, Heft 3.

"relatively cured," living over a year after; and 13 = 7·8 per cent., were living three years after the operation (one case only (Bottini's) being alive ten years after operation). Tauber refers to the fact that whilst ovariotomy had been reduced in mortality from the 70 per cent. of deaths following the first 100 operations to only 4 or 5 per cent., there had been no improvement in the figures for total laryngectomy. This author came to the conclusion that palliative treatment of laryngectomy or tracheotomy was preferable to the radical operation. In connexion with laryngectomy he quoted the remark of Stromeyer: "Humanity suffers no damage if the operation is not performed, and surgery will derive no honour from it." After hearing the paper just read, it may be fairly said that the reverse holds true at the present day.

A Method of Lateral Pharyngotomy for the Exposure of Large Growths of the Epilaryngeal Region.[1]

By Wilfred Trotter, M.S.

(ABSTRACT.)

The importance of free exposure in the operative treatment of pharyngeal tumours. The approach to the pharynx by the lateral route. Two varieties of lateral pharyngotomy—superior or transmandibular, inferior or transthyroid. Indications for the use of these. Cases for which these methods are not suited. The combined (superior and inferior) operation. Description of the operation. Indications for it. Principles of the treatment of large epilaryngeal growths.

DISCUSSION.

Mr. Herbert Tilley: I wish to ask one question, on a matter which has been raised also in connexion with malignant disease in other parts of the body. It is in regard to the preliminary irradiation of the area which is to be operated upon, also the irradiation of such an area afterwards. After lateral operations on the pharynx I have seen radium put into the wound with very disastrous results, and, in at least one case, I am certain it was the cause of death. In that instance a brilliant operation had been performed, and it was requested that radium should be inserted immediately at its close. Sloughing of the growth and infection of the wound followed. I should like to hear opinions as to the advisability of preliminary or of post-operative irradiation.

Dr. Syme (Glasgow): I think the most interesting point in Mr. Trotter's communications is the way he deals with the larynx afterwards, tucking

[1] This paper, with illustrations of the operation, will be published, *in extenso*, in the *Journal of Laryngology, Rhinology and Otology*.

the larynx under the tongue, to prevent food entering the air passage. In several cases my difficulty has been not so much in exposing the tumour as in safeguarding the larynx against the entrance of food, after removing the arytænoids, epiglottis and aryepiglottic folds. It is a gain to preserve the larynx for phonation purposes, but I wonder if there is any difficulty in maintaining what is left of it in apposition to the base of the tongue, and whether in the movement of the tongue the communication is sometimes destroyed.

Sir WILLIAM MILLIGAN: In the event of the growth being in the sinus pyriformis does Mr. Trotter think it necessary to make a transmandibular incision as well as an inferior thyroid incision? Or would the more limited incision be sufficient to secure exposure? With regard to Mr. Tilley's point as to the irradiation of these growths, the great objection I have to irradiating before the operation is, that when the operation is done, the growth is soft and more or less rotten, and hæmorrhage is difficult to control. If irradiation is to be done after operation, it should not be through the exposed tissues, but from the surface by means of applicators.

Sir STCLAIR THOMSON: We should welcome the work which Mr. Trotter has done in an allied and friendly neighbourhood. Having seen much of this work, I am a little regretful that he limited his observations to purely pharyngeal tumours, because through the same incision, I know what Mr. Trotter can do on the larynx when the malignant growth is situated on the aryepiglottic fold, or in the sinus pyriformis, or where, as Sir James Dundas-Grant said, the intrinsic growth has penetrated and perforated the thyroid cartilage. It is not fair to ask Mr. Trotter for an opinion on a specimen only, but I would ask him whether, seeing that specimen of Sir Charters Symonds, he does not think it would have been possible to remove the growth satisfactorily and yet leave the patient with a little of his larynx? That is a subject which, I think, this Congress might develop. I mean the point that the surgery for malignant growth of the larynx does not lie in a mere choice between laryngo-fissure and total laryngectomy: it might behove the rising generation to develop operations which are midway between these two. As suggested, we have too long been under the idea that we have to choose between these two. We all agree as to the importance of a precise diagnosis beforehand, otherwise we may be in the position of the surgeon I heard of in the United States last year, who removed all he could see, and a good deal that he could not see!

Dr. WILLIAM HILL: It is almost a pity the question of irradiation was brought up by Mr. Tilley, though I admit it is very important. I get the dregs of cancer cases in which the growth is ineradicable, and I have to do the best I can. The effect of radium seems to be different in Manchester from what it is in the neighbourhood of Cavendish Square, for, so far from a tumour being made soft by the application of radium to it, the effect I have seen has been a shrivelling and hardening: the process being atrophic. At Middlesex

Hospital and other places the effect has been shown to be fibrotic. So far from hæmorrhages being greater after radium, the radium is hæmostatic. It can be applied to angeioma. But radium experts have assured me that radium must not be pushed to the cauterization stage; if so, you might as well use diathermy. Therefore I think Sir William Milligan's objections do not hold unless you irradiate too much. Radium is an antiseptic, and after its application the growth will be less septic, even if it does not disappear—and very few do disappear under radium. I saw Mr. Trotter do this operation, as did two of our visitors, and they expressed their admiration not only for the devising of the operation, but the operator's great facility, for in three-quarters of an hour he did what would take some surgeons two hours. Perhaps this accounts for his low mortality.

Sir WILLIAM MILLIGAN (in further explanation): I never alluded to sepsis. I said radium made the growth softer when it had become infected, and that is supported by the statements of French and American surgeons in the last few months. I shall be pleased to send the references to Dr. Hill.

Mr. WILFRED TROTTER (in reply): With regard to radium I have no doubt that radiation of the wound immediately after operation for a pharyngeal tumour is apt to be a serious danger. In my experience the infection to which the wound is necessarily exposed is greatly increased in virulence by the effects of the radium, and sloughing may become uncontrollable. It is possible, however, that in the cases in which immediate post-operative radiation made such an unfavourable impression on me, the doses used were too large. The various kinds of plastic procedure that may be necessary after a large excision of the pharynx are difficult to explain in detail. The chief principles to remember are that repeated operations may be necessary, that in an edentulous patient there is rarely any trouble from sepsis and sloughing, and that skin implanted in the pharynx survives very satisfactorily. The chief requirement is a communication between the trachea and the pharynx that will give passage to the voice without admitting food. To function satisfactorily in these ways such an opening must usually be high up under the tongue and small. For approaching the pyriform sinus the lower lateral pharyngotomy is adequate. If the epiglottis is extensively involved the upper operation with division of the mandible must be combined with it. Sir StClair Thomson raises the extremely important question as to whether there is no possible course other than laryngo-fissure or total laryngectomy in the treatment of intrinsic cancers. At the present time, when I am called upon to operate in a case in which the growth is too extensive to be dealt with by thyrotomy, I excise the growth freely and try to maintain communication between trachea and pharynx by preserving the residue of the larynx and completing its lumen with skin flaps.

Carcinoma of the Post-Cricoid Region (Pars laryngea pharyngis) and the Upper End of the Œsophagus.

By A. Logan Turner, M.D., Edinburgh.

(ABSTRACT.)

Owing to want of time, I will confine my remarks to two aspects of the disease. (I) Some points in the life history of the tumour as exemplified both on its pathological and clinical side. (II) Certain factors bearing upon the suitability of the tumour for successful removal.

Both these aspects are closely allied.

(I) By means of a series of drawings made from post-mortem preparations of cases of tumour, I am enabled to show that, in the cervical region, the tumour may occupy the following situations:—

(*a*) The upper end of the œsophagus.

(*b*) The upper end of the œsophagus and the post-cricoid region.

(*c*) The post-cricoid region only.

The preparations demonstrate certain points in the life-history of the tumour: the extension of the disease into the soft tissues of the neck, the involvement of the recurrent laryngeal nerves, of the trachea, the larynx, and the thyroid gland. They show further that when the tumour occupies the cervical œsophagus (*a*), its lower end reaches to or passes behind the manubrium sterni and the tumour is not freely accessible to the surgeon. When the tumour occupies the cervical œsophagus and post-cricoid region (*b*), the pathological changes suggest that the growth has extended from below upwards. When the tumour is confined to the post-cricoid region (*c*) it is the most suitable for successful removal. All the preparations demonstrate how necessary it is to make an early diagnosis if a satisfactory operation is to be carried out.

The majority of the cases of post-cricoid carcinoma occur in women, and frequently at an age when the presence of malignant disease may not be suspected. It is partly owing to an insufficient recognition of these two facts, coupled with a want of knowledge of the clinical types met with, that diagnosis is delayed.

The sex and age incidence of carcinoma affecting the tongue, fauces, oral and laryngeal pharynx, larynx and œsophagus, are illustrated by the aid of a series of tables compiled by the writer from his notes taken between the years 1907 and 1919 inclusive. Out of ninety-eight cases

of post-cricoid carcinoma, thirteen are males and eighty-five females. In the œsophagus, of forty-two cases, thirty are males and twelve females.

In the tongue, fauces and larynx, there was a striking predominance of the disease in the male sex. In the oral pharynx, on the other hand, the tumour attacked the two sexes more equally. Of twenty-six cases, 57 per cent. occurred in the males and 42 per cent. in women.

In studying the *age-incidence* of post-cricoid carcinoma, the figures prove the earlier incidence of the tumour in women; fifty-three of the eighty-five women were under 50 years of age, while only three of the thirteen men affected were observed while yet under 50.

The duration of the symptoms complained of differed in the two sexes. The average duration in the men was four and half months, and the period of the complaint was probably synchronous with the existence of the tumour. In many of the women, on the other hand, there was a prolonged period of difficulty in swallowing, often extending over years, which must have preceded tumour formation, followed by a shorter period of a few months with more aggravated symptoms. The latter period probably covers in them the growth of the tumour. It is this pre-tumour period that requires further investigation in order to throw light on the pre-disposition of women to post-cricoid carcinoma.

(II) A thorough examination is necessary in order to determine what cases of post-cricoid carcinoma are suitable for excision with the best prospects of success. The laryngologist has to advise and assist the surgeon.

(1) Careful examination and palpation of the neck are necessary for: (*a*) The detection of enlarged glands. (*b*) Thickening of tissues suggestive of extrinsic extension into the soft tissues of the neck, the larynx and trachea. (*c*) Secondary involvement of the thyroid gland.

(2) Radiography for: (*a*) The detection of the vertical extent of the growth and for an accurate knowledge of the site of its lower end. (*b*) The exclusion of a possible second intra-thoracic stricture. (*c*) The recognition of possible secondary thoracic glands.

(3) Laryngoscopy: indirect and direct; and œsophagoscopy.

Changes were noticed in the majority of the cases of post-cricoid carcinoma in the series. It is hoped that, in the future, if the patients present themselves at an earlier period, fewer pathological changes will be observed in the laryngoscopic mirror. Impaired mobility or immobility, of one or other vocal cords, implies an extension of the tumour beyond the mucous or muscular coats of the pharynx, and

makes these cases unsuitable for operation. Some interference with the mobility of the vocal cords is observed in forty-three cases in the series, that is in almost one half.

The case essentially suitable for successful excision is one in which the tumour is confined to the laryngeal pharynx and as yet apparently intrinsic, the vocal cords still preserving their normal movements. In spite of careful consideration of all the facts, exposure of the tumour area at the operation may reveal some extension of the disease, not previously suspected.

DISCUSSION.

Mr. E. D. D. DAVIS: Statistics are inconsistent and it is difficult to draw accurate conclusions from them. The Registrar-General's statistics of cancer of the pharynx and œsophagus, published in the Report of the Imperial Cancer Research, show that cancer of the whole pharynx is three times more common in men than in women, and the same is true of cancer of the œsophagus. But it is admitted that cancer in the post-cricoid region is more frequent in women, and it can be explained by the fact that in men carcinoma usually commences lower down, at the orifice of the gullet, or higher in the pyriform fossa and extends from there to the post-cricoid region. Secondary growths in the thyroid occur more frequently when the growth is in the upper end of the œsophagus and extends to the thyroid. In twenty-four cases of true post-cricoid growth the thyroid was not involved, and there were no secondary growths, and it is rare in such cases.

Dr. DAN MCKENZIE: I can recall the history of the development of our knowledge of this type of cancer. We are approaching a more definite knowledge as to its origin. Dr. Logan Turner's facts have brought us to this point: there is probably in women a pre-cancerous stage of the disease. It is difficult to suppose that cancer can exist for years in the post-cricoid region, causing symptoms, and that yet it does not progress. It is more rational to suppose a definite condition exists which ends in cancer: probably irritation leads to malignant degeneration. I draw attention to two cases published in the *Journal of Laryngology* (vol. xxxiii., p. 270), in which I found identical symptoms, a narrowing at this spot, which was permanently overcome by bougieing. These were the first cases of that type I have seen: here there was stenosis. I contended that this was a spasmodic stricture. I have now an open mind on the matter: it may not be spasmodic stricture, but may have been organic, and it is possible that such stenoses may be amongst the factors which predispose to cancer at this particular spot.

Dr. D. R. PATERSON: I will not go into detail, but I would remind members that at last year's Congress I called attention to a particular affection met with in women, which was often followed by the condition of which Dr. Logan Turner speaks. I do not know whether he has made up

his mind on the subject, but I suggest that in many of the cases it is not improbable the changes such as I then described were present. These are conditions in the bucco-pharyngeal region, starting with the tongue and lips and extending through the mouth cavity down into the opening of the gullet. Does Dr. Logan Turner think there is any causal relation with the condition he has described?

Dr. BROWN KELLY: I think my statistics support those of Dr. Logan Turner. Of cases of malignant disease, in the hypopharynx there were five males compared with eighteen females, whereas in the œsophagus the females numbered six and the males forty-one. I also have had female patients who had previously undergone a long course of bougieing—one of them for thirteen years—and who subsequently developed cancer at the entrance to the gullet.

Dr. LOGAN TURNER (in reply): We want to see the tumour in these cases before it invades either the larynx or the soft tissues of the neck or the trachea. The specimen I showed was a bag of mucous membrane, dissected off the back of the larynx and posterior pharyngeal wall and cut across, above and below the tumour. The larynx was left untouched and the patient was fed through an œsophageal tube. The cut end of the œsophagus was brought out at the wound in the neck and stitched there, and the œsophageal tube inserted into it, the patient feeding through this channel. The question of re-establishing the continuity of the gullet is left for later consideration. Statistics may be made to read very much as we wish but I think there can be very little doubt that the majority of cases of tumour in the post-cricoid region have been in women. In Dr. Paterson's paper read at the Congress last year I had observed the condition which he described. I cannot say why women are predisposed to carcinoma. We find in some, a superficial abrasion, which bleeds slightly on the passage of the œsophagoscope. One such had, for two years, difficulty in swallowing, with choking attacks. There was no evidence of carcinoma: she was examined every three months. Her Wassermann reaction was negative.

The Early Diagnosis of Carcinoma of the Oral and Laryngeal Pharynx, including the Diagnosis of Inoperable Cases.[1]

By E. D. D. DAVIS, F.R.C.S.

(ABSTRACT.)

A STUDY of:—

(1) Fourteen cases of carcinoma of the base of the tongue, involving the pharynx.

[1] This paper, with drawings of specimens, will be published, *in extenso*, in the *Journal of Laryngology, Rhinology and Otology*.

(2) Thirty-one cases of carcinoma of the pyriform sinus.

(3) Twenty-four cases of post-cricoid carcinoma.

The site of origin of the growths.

The early symptoms and signs.

The value of examination by the œsophagoscope, and by suspension laryngoscopy.

The results of treatment of five cases by excision of post-cricoid epitheliomata and the signs of inoperable cases.

Impaired Mobility of the Cord in the Diagnosis and Prognosis of Intrinsic Cancer of the Larynx (Observations based on Forty-four Cases Treated by Laryngofissure).[1]

By Sir StClair Thomson, M.D.

(ABSTRACT.)

This communication leaves out of consideration cases which were only seen in consultation and cases which were not operated on. It was thought that more valuable conclusions might be arrived at by limiting the investigation to cases operated on; in which the diagnosis in every case was confirmed by microscopic examination of the removed growth; and in which the after history of each case has been followed up to the present day or until death.

Of these forty-four cases the affected cord was only mobile in twenty-seven. In seventeen its movement was more or less impaired, the degree varying from mere sluggishness to complete fixation. In three of these cases the defect in movement developed while under observation after ten days, six weeks, six months or nine months. In one case an impairment of movement was noted two years before a subglottic growth was diagnosed.

In these forty-four cases it was only possible to remove a satisfactory portion beforehand (for microscopic examination) in ten cases. It is noteworthy that of these ten cases the cord moved freely in eight.

Of the twenty-seven cases with a mobile cord twenty-one are alive and free from local recurrence. Two have had local recurrence; one is dead of cancer in another part of the body; and three have died of other diseases.

Of the seventeen cases with impaired movement, only seven are

[1] This paper is published, *in extenso*, in the *Lancet*, cxcix, p. 183.

alive without recurrence; five have died of local recurrence; one died of recurrence in the glands; one from hæmorrhage soon after the operation; and three died from other causes.

Conclusions.

(1) Impaired mobility of a cord is not a necessary or a frequent symptom of intrinsic cancer of the larynx.

(2) It is, in fact, only met with in a minority of cases presenting themselves for diagnosis.

(3) It is more apt to be met with in an early case when the growth is imbedded in the cord or growing into it than in a distinctly sessile or even pedunculated tumour. It is also more usual with growths situated on the inner surface or the subglottic area than with those seated on the upper surface of the cord. Otherwise, fixation indicates an advanced case.

(4) When present it is a very valuable symptom in distinguishing a malignant from an innocent tumour. It is of little value, and may even be misleading, in diagnosing a malignant growth from a tubercular or syphilitic deposit.

(5) While in some cases it may be necessary to postpone a diagnosis in the absence of this symptom, yet as it is desirable to avoid any delay, we may have to rely on inspection, the age, sex, history and progress of the case, and the exclusion of other possibilities. The help of microscopic examination of a removed portion is only available in a small number of cases (ten out of forty-four).

(6) As regards prognosis it is an unfavourable symptom.

DISCUSSION.

Sir William Milligan: I remember very distinctly being taught that impaired mobility of the vocal cord was almost a certain sign of malignancy, and it took one a considerable number of years to learn to differentiate. I have seen many cases in which the mobility of the cords was normal, and yet in which the growth was malignant. At first, on account of the teaching I had received, I was inclined to think that because the cords moved freely, the case could not be malignant. Some of these cases are very perturbing, because it is difficult to get over the influence of one's original teaching. And now we have a very definite and detailed statement by Sir StClair Thomson which will be of great value to us all. The question as to where the deposit of malignancy is, is one of very great importance. If the malignant growth is on the surface of the cord, there is no reason why there should be any impaired mobility. On the other hand, if there is the least lateralization of the growth, or a tendency

to infiltration, either subglottic or to the side, there is every reason for impairment of movement. This paper teaches us, at all events, that we must discard our previous views on this subject: because a cord moves with perfect freedom we must not think it is necessarily a benign growth. I recall one case which gave me much anxiety: it was in the person of a very prominent Member of Parliament, who had a growth of a suspicious nature on the left vocal cord: it might have been papillomatous, or it might have been malignant. Another opinion was sought, and this opinion was that it could not be malignant, because the cord was so mobile. I insisted, however, that it was malignant, and I did a thyrotomy, removing the cord, which I submitted to microscopical examination. I obtained a very competent opinion to the effect that the condition was malignant throughout. Sir StClair Thomson's paper is an extremely valuable piece of deductive reasoning and clinical experience.

Mr. DOUGLAS HARMER: In cancers of the air-passages we must distinguish two types. First, there is the disease in which the irritation is completely local, and in which the growth is of a superficial character. In such, the growth is spoken of as the "cauliflower" variety. Such cases are often reported as papillomatous, and it may be difficult to decide whether they are malignant. They are very rare on the tongue, but common on the palate and pharynx, and I think they occur on the surface of the vocal cord. I think these are the cases in which there is mobility of the mucous membrane—whether it is the cord or the other part of the upper air passage. On the contrary, all of us know the type of carcinoma which is, microscopically, much more definite, in which the growth, from the start, takes on a deep ulcerative character, and very little tumour formation is present. On palpation of the growth one is uncertain how deep it has extended into the pharynx, the neck or the cartilages. Clinically, this type is quite different from the former. As to prognosis, I believe that cases in the first group do well with operation, whereas those in the second group do badly: it is almost impossible to get a good result with one of these deeply ulcerating carcinomata. There is another point of importance, namely, the effect of radium upon these two types. If filtered radium be applied to one of the cauliflower" epitheliomata by burying it aseptically in the immediate neighbourhood of the tumour, almost without exception, the tumour will shrivel away and become fibrotic. In some cases the shrivelling is so marked that the patient may live two or three years, even though the growth was quite inoperable. In the case of deeply-ulcerating epitheliomata, I have never seen radium do any good: in fact in nearly all of them it irritates the growths, which generally become septic, however carefully you bury the radium. Those are the cases in which Sir William Milligan says the growths soften with radium, and for those it is a bad form of treatment.

Dr. JAMES DONELAN: I do not know whether the first publication of Sir Felix Semon's paper had any relation to the case which is, perhaps, the leading one in regard to this class of cancer, and on which the Chairman (Mr. Mark

Hovell) is more qualified to give an opinion than anyone else now surviving. I refer to the case of the Crown Prince of Germany. What must we do in cases in which we cannot come to a decision? We have not any microscopical results to guide us, and we cannot trust the immobility. While we exercise "the *flair* of the physician" and take into account the patient's age, history and general condition, can we urge the patient to have his vocal cord removed on what has been hitherto regarded as insufficient evidence? Shall we submit such a patient to an impaired voice when it may not have been necessary, rather than run the risk of the case being malignant?

The CHAIRMAN (in answer to the remark of Dr. Donelan, referring to the case of the Crown Prince of Germany): Sir Morell Mackenzie stated that although there was no proof he thought the case would turn out to be malignant.

Sir STCLAIR THOMSON (in reply): I hope to have here to-morrow half a dozen cases, to show the results of laryngo-fissure years after the performance of that operation. One was a case in which the cord was absolutely fixed. I removed the thyroid cartilage, and three years have passed. Still, I am always anxious about these cases, and when the cord is fixed, I think it will be a question whether there should not be at least some hemilaryngectomy. Certainly the thyroid cartilage should be removed and scrutinized, and if it is found to be penetrated, a more extensive operation considered. As to what we are to do as regards diagnosis when the cord is not fixed and there is no specimen available for microscopical examination, I can only say I keep a watch and wait. It is desirable to take these people into your confidence. If the disease is slightly and slowly progressive and the patient is over middle age and we are able to exclude tubercle and syphilis, removal of the cord should be advised. If that were my condition, I would have the cord out, even if it were no more than suspicious. In my forty-four cases there was only one over which pathologists differed. I agree with Mr. Harmer that if these cases present themselves with a projecting growth on the cord and it is removable, they are the best cases one could have: but the deeper infiltrating cases, tending to fix the tissues, are more malignant.

Œsophageal Obstruction due to Hypertrophy of the Cardiac Sphincter and Narrowing of the Epicardia.

By A. BROWN KELLY, M.D. (Glasgow).

THE case upon which this paper is based presented two rare and interesting œsophageal conditions, viz., hypertrophy of the cardiac sphincter and narrowing of the epicardia, one or both of which caused occlusion and death.

John —, aged 21, came to the Victoria Infirmary, Glasgow, on

February 19, 1919, on account of dysphagia. This had set in suddenly six years before without known cause. The obstruction was said to be opposite the lower end of the sternum. Food reached this level without difficulty but a great part was at once regurgitated. At meals, in order to get the food down he had to drink freely and occasionally to stand up, stretch and contort himself. During the previous ten days the stoppage had become almost complete and he therefore sought medical advice.

On inspection, the œsophagus was found to contain a good deal of fluid and food débris. Its diameter was somewhat increased. The hiatus appeared as a long narrow slit which opened and closed rhythmically so long as it was not touched by the tube but closed spasmodically when this was pushed down against it. Firm and steady pressure failed to gain entrance to the stomach. Subsequent attempts to pass bougies through the cardia also failed.

While in hospital the patient had a dazed appearance. His temperature was normal; pulse feeble and usually from 90 to 120; respirations averaged 24; weight 6 st. 7 lb. His food was measured and nearly all was regurgitated. Thus, on March 1 he took 66 oz. and returned 59 oz.; on March 2 he took 55½ oz. and retained only 3 oz. Nutrient enemata were administered and gastrostomy was arranged for but owing to his low state was not performed. He died on March 10.

At the post-mortem examination made by Dr. John Anderson the œsophagus was inspected *in situ* but no band, adhesion, narrowing of the diaphragmatic opening or other condition liable to cause external constriction was found.

On removing the œsophagus it was held up and filled with water but none escaped by the lower end. After a catheter had been passed water ran through but an hour later the cardia was again impermeable to water.

The œsophagus measured 19 cm. from the lower margin of the cricoid to the hiatal level. On slitting it open almost to the hiatus, the upper third was seen to present a dilatation especially to the left; a short middle portion, where crossed by the aorta, was also dilated but relatively less so, while the remaining 11 cm. formed a fusiform sac. The wall was thickened in its whole length but especially towards the lower end; above it measured 5 mm., and in the last 7 or 8 cm., 8 mm. in thickness. The lining membrane of the upper third was rugose and slightly thickened; in the lower two-thirds it was smoother and the mucosa thicker and mammillated; the firm outgrowths varied in size from a pin's head to a small bean.

Microscopic examination showed the thickening of the wall of the œsophagus to be mainly due to hypertrophy of the muscular layer. The circular coat was chiefly affected and in places was three times the normal thickness. The longitudinal coat was involved to a much less degree.

The œsophagus with cardia and epicardia intact, was submitted for examination to Prof. S. G. Shattock, F.R.S., and I am indebted to him for kindly drawing up the following report.

"The œsophagus is dilated for the entire length, the dilatation gradually increasing from above downwards, and ceasing abruptly at the upper level of the diaphragm. In its passage through the diaphragm the œsophagus is of normal size. On either side, on the superior aspect of the diaphragm there is shown a small portion of each pleural cavity.

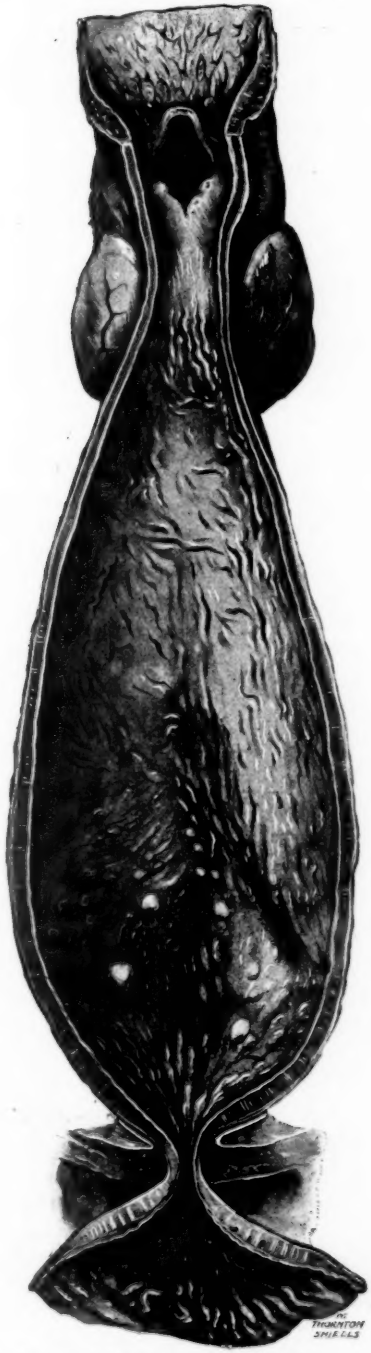
"The muscular wall of the whole of the intrathoracic length of the œsophagus is hypertrophied, the overgrowth being limited, or almost so, to the circular fasciculi.

"The overgrowth is but little marked, if any indeed is present, in the narrow segment which passes through the diaphragm.

"Below the diaphragm, the muscle becomes again thickened and attains a maximum of 4·5 mm. After a distance of 3 cm. (1 inch) the thickened muscle (the arrangement of which shows that it consists of circular fasciculi) shades off into the general muscular wall of the stomach. This subdiaphragmatic thickening corresponds precisely in position with the cardiac sphincter. (*See* 'The Normal Cardiac Orifice and Canal,' figs. 14A, 14B, *Proceedings of the Royal Society of Medicine*, 1919, vol. xii (Section of Laryngology), p. 92.)

"The mucosa of the lower part of the dilatation presents a certain number of smooth white elevations of the nature of epithelial thickenings or callosities.

"*Remarks.*—The striking and unusual feature in regard to this specimen, is the pronounced hypertrophy of the proper cardiac sphincter, a very uncommon accompaniment of œsophagectasia. The interpretation which may be placed upon the anatomical details as a whole is that the cause of the obstruction in this case is a hypertonic contraction of the cardiac sphincter, i.e., a true cardiospasm, which has led to its overgrowth, such as is witnessed in hypertrophic stenosis of the pylorus. This factor distinguishes it from œsophagectasia due to achalasia, where the obstruction is attributable to the tonic contraction of the cardiac sphincter unaccompanied with its hypertrophy.



Œsophagectasia, associated with overgrowth of the cardiac sphincter.
(Dr. Brown Kelly's case).

The overgrowth of the muscular wall of the œsophagus above the diaphragm may be viewed as compensatory in kind, and secondary to the obstruction below. And finally, the absence of dilatation of the œsophageal segment passing through the diaphragm, may be explained by the support afforded by the muscular mass of the latter in this position."

In this case certain ordinary features were present, viz., the symptoms of cardiospasm including dysphagia and regurgitation; the œsophagoscopic appearances of dilatation with retained contents and hypersensitiveness of the suprahiatal region; and the post-mortem findings of dilatation and hypertrophy of the muscular wall of the intrathoracic gullet.

On the other hand, several unusual features were met with, viz., the patient's inability throughout his last illness to get almost any food into the stomach, so that death was due to starvation; the firm closure at the lower end of the gullet preventing the passage of the finest bougies; and the pronounced narrowing or blocking of the diaphragmatic gullet and the great muscular hypertrophy of the cardiac sphincter found at the autopsy.

The obvious cause of the patient's illness and death was therefore the obstruction at and above the cardia. The constant and unyielding anatomical stenosis in this case distinguishes it from the temporary and remediable obstruction due to cardiospasm.

In seeking an explanation of hypertrophy of the œsophageal musculature it is necessary to consider not only the hypertrophy itself but also its relations to obstruction at the cardia and to distension of the gullet. The study of these questions has been facilitated by the valuable series of drawings brought together by Dr. Irwin Moore [1]. In these the thickness of the wall at the different levels has been carefully represented by Mr. Thornton Shiells and commented upon by Professor Shattock. I beg to acknowledge the assistance I have received from these sources.

The normal thickness of the entire œsophageal wall is 3·5 to 4 mm. while that of the muscular layer is from 0·5 to 2·2 mm. (Kraus). Henle gives these measurements as 2 mm. and 1·5 mm. respectively.

A number of cases are on record in which the musculature attained a thickness of 4 to 7 mm. In nearly all of these the main increase was due to hypertrophy of the circular muscular fibres. Occasionally the longitudinal fibres participated in a minor degree, and still more rarely the other components of the wall. Apart from hypertrophy, the

musculature seldom presents any pathological change, e.g., a small-celled infiltration causing separation of the muscular fibres, as in a case reported by Stern, or fatty degeneration of the musculature as noted by Wibrecht. Variations in the thickness of the œsophageal wall may therefore be said, in general terms, to correspond to the degree of development of the circular muscular coat.

The musculature of apparently any part of the œsophageal wall may undergo hypertrophy. In œsophagectasia, as a rule, the cervical part of the gullet is neither dilated nor thickened, while in the remainder of its length to the level of the diaphragm it is altered in both respects. The diaphragmatic section is very seldom affected and the cardiac even more rarely. It is significant that thinning of the wall has not been observed in œsophagectasia. On the contrary, in many specimens, the thickness of the wall, i.e., the degree of muscular hypertrophy, corresponds to the amount of dilatation, instead of the reverse as might be expected. Thus, in spindle-shaped dilatations the wall often attains its maximum thickness where the gullet is widest, and in a lateral bulging the musculature has been found thicker than elsewhere at the same level. This relation between the thickness of the wall and the amount of dilatation, although holding good generally, is not invariable.

In considering hypertrophy of the wall without œsophagectasia, special mention should be made of a case reported by Elliesen [2]. In this the calibre and the thickness of the wall of the upper part of the gullet were normal, but in its lower two-thirds, owing to great hypertrophy of the circular muscular layer, which attained a thickness of 6 mm. to 7 mm., it was converted into a rigid canal somewhat larger than a lead pencil. A uniform lumen of 9 mm. (10 mm. is the narrowest physiological diameter) extended for 18 cm. As Elliesen could find no cause for the hypertrophy he termed it idiopathic. (Fig. 7 of the Irwin Moore series represents an œsophagus of this type.)

Another case of the same kind is described by Ehlers [3], in which the musculature of the lower half of the œsophagus had undergone enormous hypertrophy. About 3 cm. below the upper end the muscular layer alone presented a thickness of 5 mm.: this gradually increased so that at 7 cm. it was 8 mm.; at 10 cm., 12 mm.; and at 1 cm. above the cardia, 9 mm. The lumen was uniform and normal throughout. Hypertrophic stenosis of the pylorus was also present. In the absence of other explanation the author regarded the œsophageal and pyloric conditions as congenital disturbances.

This view receives support from specimens recently shown by Eric

Pritchard and W. T. Hillier [4] at a meeting of the Section for the Study of Disease in Children. They were taken from a baby aged 3 months and presented great hypertrophy of the lower end of the œsophagus, of the pylorus and of the ileo-cæcal sphincter. As there had been no pronounced symptoms of obstruction at any of these orifices it follows that such stenosis as existed was brought about without any great degree of spasm.

This association of cardiac and pyloric hypertrophies should cause those who attribute the latter to spasm to reconsider the question, because œsophagoscopy and post-mortem material have proved that spasm extending over even decades does not lead to hypertrophy at or near the cardia.

In considering narrowing and obstruction of the lower end of the gullet in association with muscular hypertrophy and œsophagectasia one should take into account both the condition of the cardiac sphincter, especially as regards hypertrophy, and the length and patency of that part of the gullet lying between it and the hiatus œsophageus—the epicardia. These two regions and the manner in which they may be affected respectively have not been clearly differentiated in the past.

The œsophagus submitted for inspection demonstrates this need. At the lower end two obstructive conditions are present, namely, great thickening of the cardiac sphincter, and closure of the diaphragmatic section. The former is due to hypertrophy of the circular muscular fibres. In this situation the entire muscular wall, excluding the mucosa, attains a maximum thickness of 4·5 mm. and in consequence the circumference of the cardia is much reduced; its measurement is unobtainable owing to the irregularity of the surface. The diaphragmatic gullet is about 2 cm. in length, and its muscular wall is on an average 1 mm. thick. The mucosa is here gathered into eight prominent folds of almost equal size which run uniformly from gullet to stomach. Owing to these folds it is impossible to measure the internal circumference of this section of the œsophagus, but when the cut edges of the specimen are brought together the lumen is obliterated and the impermeability to water observed at the post-mortem is accounted for. It is noteworthy that the stenosis has not been caused by muscular hypertrophy, for the wall is thinner than in the adjacent parts above and below.

The normal internal circumference of the cardia is 5 to 5·5 cm. and the length of the epicardia is about 5 cm. Reference to the Irwin Moore series of drawings discloses great variations in these

measurements, also in the thickness of the muscular wall of the cardia-epicardia. Thus, in figs. 6, 12 and 13 the circumference of the cardia appears reduced, in fig. 6, it measured 3·2 cm.; in none of these was thickening of the wall present. Fig. 7 shows muscular hypertrophy at and above the cardia for a distance of 5 cm. In fig. 10, a great overgrowth of the circular muscular fibres is seen to correspond with the cardiac canal. The short narrow canal normally representing the diaphragmatic section of the gullet, in œsophagectasia may become merged into the general dilatation and be indicated merely by a constriction. This condition is depicted in figs. 1, 8, 9, and 11, and in these, as Professor Shattock remarks, the dilatation extends to the gastric orifice.

A number of cases of narrowing or stenosis of the "cardia" have been reported but it is doubtful whether the term has always been properly applied. Endoscopists have contributed to the confusion in not a few instances by referring to the hiatus œsophageus as the cardia and by taking no note of the distance between the hiatal level and the upper edge of the gastric mucous membrane. Professor Shattock has recently and opportunely pointed out that the collection of circular fibres forming the cardiac sphincter embraces part of the stomach as well as the end of the œsophagus, that it lies quite below the diaphragm, and that the term cardiac canal is preferable to that of cardiac orifice because the sphincter is not a simple ring but a fusiform thickening of some length.

Stierlin [5] has reported a case of œsophagectasia with stenosis at the cardia which he considered to be of congenital origin. He also refers to a case of Wibrecht's. In both, the internal circumference at the cardia was 2·8 cm. The narrowing was almost as great in the following cases: Zenker 3, Strümpell 3, Giesse 3·5, Leichtenstern 3·6, Strauss 3·8 cm. In none of these is the reduction in the lumen ascribed to muscular hypertrophy. In Strümpell's case [6] the lowest 2 cm. of the gullet as well as the cardia had a circumference of 3 cm., but the wall, especially the muscularis, was thin.

A consideration of the facts adduced leads to the belief that no single cause will account for all the varieties of muscular hypertrophy met with in the œsophageal wall. In cardiospasm it is usual to attribute the thickening of the dilated wall to the extra force required to overcome the obstruction below, and it is significant that hypertrophy of the circular fibres represents the increase. But how are we to explain great thickening of the wall associated with reduction of the lumen of the

œsophagus? In the most marked case of this kind (Ehlers) pyloric stenosis co-existed and the author raised the question whether the more powerful contraction of the stomach necessary to force food into the duodenum caused yielding at the cardia so that food passed up into the œsophagus and there excited opposing contractions. This, however, he dismissed as too far fetched, and he favoured a congenital origin.

As to hypertrophy at or near the cardia, the question naturally arises as to why in Batty Shaw's case (specimen No. 10 shown by Herbert Tilley) the overgrowth is greatest along the diaphragmatic part of the gullet, while in the specimen shown it corresponds with the cardiac sphincter. Are we to regard these localized hypertrophies as congenital disturbances possibly due, as has been suggested, to hyperadrenalism in the mother? or, are we to accept Professor Shattock's view of hypertonic contraction leading to overgrowth? The writer is inclined to favour the congenital origin of the hypertrophic conditions at and near the cardia and of the narrowing of the diaphragmatic gullet, as presented by the specimen under consideration; he recognizes, however, the improbability of the question being settled until the action of the œsophagus in health and disease, particularly in relation to the sympathetic nervous system, is better understood.

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DISCUSSION.

Mr. Kisch: I have been troubled with one or two cases. Occasionally one gets a case in which dilatation is of use, but in others it seems to have no effect. I have a man getting progressively thinner: dilatation does no good, and the question is whether one should do gastrostomy. In the case recorded it seems sad that the man died of inanition when an artificial opening might have been made into the stomach. Does Dr. Brown Kelly think I should be justified in doing gastrostomy in my case?

Mr. Douglas Harmer: I had at patient at St. Bartholomew's Hospital who suffered from this complaint seven years. His condition steadily got worse, so that he arrived at a state in which he was practically starving, and it was impossible to relieve him by using bougies. After due consideration,

gastrostomy was performed, and the peculiar fact was, that as soon as he had returned to the ward and recovered consciousness he said to the sister "I want something to eat, they have cured my obstruction." He was given water, which he swallowed easily, and a little later semi-solids, which were also taken easily, and later still solid food. The gastrostomy wound was maintained, but he continued to feed by the mouth. He was sent out of the hospital, and was watched for some time. Some months later, he had pneumonia, from which he died. We were fortunate in getting a post-mortem examination, when it was found he had little hypertrophy of the muscular coat of the œsophagus anywhere: it was, apparently, a normal œsophagus. Yet for seven years he had this great difficulty in swallowing.

Dr. P. WATSON-WILLIAMS: The author must have given an immense amount of time to the collection of his clinical data. The point raised by Mr. Harmer brings home to us the difficulty and yet the necessity of making a diagnosis between cases which have cardiospasm and those which show hypertrophy. The author reminds us that a cardiospasm extending over a long time does not necessarily, or even usually, lead to hypertrophy. But if you have a case in which there is this obstruction which is not due to malignant disease, you have to make up your mind whether it is hypertrophy or spasm. We must remember that if we have a persistent case of cardiospasm alone, gastrostomy may be the means of getting over the difficulty and enabling the patient to swallow by the mouth.

Dr. SMURTHWAITE: A woman aged 45 came to the clinic because she could not swallow meat, and had to take all her nourishment in a fluid condition. She was fairly stout. She had been taking food in the form of milk and beef tea. Under chloroform, I used the œsophagoscope. It passed to the cardia without encountering any obstruction. She was then given a bismuth meal, and a radiogram showed only slight narrowing at the entrance of the gullet. She was sent back to bed, and two days later she asked for meat, and has continued to eat solids ever since. The only way I can explain that case is, that two years ago she had taken an extra large bolus of meat, and in passing it down she was left with the impression that there was obstruction; and in accordance with that impression she declared her inability to swallow solids.

Sir WILLIAM MILLIGAN: The ætiology of this condition is an interesting matter. What theory can the author advance for this peculiar limited change in the lower end of the œsophagus? Is it possible that this is the outcome of swallowing some caustic fluid at an early period of life? Or is it likely to be a congenital anomaly? It is a very beautiful specimen, and we ought to have a photograph of it in our *Proceedings*. With regard to the question of gastrostomy, I think we must have noticed that after this operation, the patient's power of swallowing, at all events for a time, is considerably better. In Mr. Harmer's case the question of physiological rest could not have come into play, the improvement was so soon after the operation. But in malignant

disease of the œsophagus, for instance, I have constantly found, after gastrostomy, that the patient has been able to swallow in a few days much better than he could before the operation, and this I have attributed to physiological rest of the part.

Dr. IRWIN MOORE: Up to the time of our discussion on œsophagectasia in 1918, the majority of these cases of dilatation or hypertrophy of the œsophagus were regarded as pathological curiosities, and placed in the museums of various hospitals, without very careful clinical histories or notes as to their probable ætiology. Sir William Milligan has referred to the important question of ætiology, and I would like to make a few suggestions for the more efficient study of this condition in the future. I think we should make the Royal Society of Medicine a sort of meeting ground for the research and study of these cases. For instance, if members in the provinces can trace specimens in the museums, or if they have cases and can get a post-mortem examination made on them, and send the specimens up to London, we can not only examine them carefully, but also get a correct drawing done to add to our collection. In this way we may obtain further valuable information by comparison. The question of ætiology is a very important one, and therefore I ask you to note in cases which come under your notice whether any fluid food contents were present in the dilated œsophagus at the post-mortem; in the majority of cases it has been reported that fluid food was present. The second point concerns associated neuroses, and a further one is the relationship of neighbouring parts, which may be of value in studying the factor of mechanical pressure. It is specially important that the post-mortem should be carried out by an expert, and that the portion of the diaphragm surrounding the phrenic area should be removed with the œsophagus in order that the cardiac canal may be more readily and accurately defined. I would remind you of a note I attached to the *Proceedings* at our recent discussion that in view of the extreme rarity of these cases Professor S. G. Shattock would be pleased to make an examination and give his advice on any specimens sent to him at the Royal College of Surgeons. It is impossible to get any idea of causation in these cases unless we get complete clinical histories and also post-mortem examinations. We must study dilatation of the œsophagus as well as hypertrophic cases. The specimen of a dilated œsophagus which I show from the museum of the Royal College of Surgeons[1] was obtained from a man aged 52, who for five years had suffered from regurgitation of food, and the condition was said to have been caused by the pressure of its cardiac end against the central tendon of the diaphragm by an enormously dilated aorta.

Dr. SYME: Will the author state whether, in cardiospasm, the spasm is always at the cardia, or how high up the œsophagus he has found spasm. A case under my care, and still having spasm, was that of a woman aged 26, and the spasm was 3 in. above the cardia. By means of the œsophagoscope one saw the dilated œsophagus, but when we were on the stricture and were

[1] *Trans. Path. Soc. Lond.*, 1888, p. 103.

considering passing a bougie, the spasm suddenly gave way, and the instrument passed into the stomach.

The CHAIRMAN: From the cases which have been mentioned in the discussion it appears that some of these cases have been relieved immediately the stomach was opened, and from the speedy relief given it is evident that sufficient time had not elapsed for the element of rest to come into play. It therefore appears that opening the stomach acted chiefly as a counter-irritant, and if simple severe counter-irritation will thus afford relief, it seems that in these cases of difficulty in swallowing, a large blister on the abdomen, or even a seton, might be tried. It could do no harm.

Dr. BROWN KELLY (in reply): I beg to thank the members for their appreciative remarks. In reply to Mr. Kisch, I recommend gastrostomy to be done before the patient becomes too weak. Even in a case of doubtful diagnosis I think gastrostomy is justifiable, as it often throws light on some gastric condition which has caused the spasm. Mr. Harmer's case illustrates this connexion. Dr. Watson-Williams spoke of the difficulty of differentiating spasm from hypertrophy of the cardia. The latter condition and narrowing of the diaphragmatic portion of the gullet are so rare that they can almost be left out of account. Sir William Milligan asked me for an explanation of the muscular hypertrophy. That is the whole difficulty. We require to know more about the physiology of the œsophagus, of the internal secretions, and of the sympathetic nervous system before we can arrive at the cause. In certain cases in which cardiac hypertrophy is associated with pyloric hypertrophy the conditions are probably congenital. In answer to Dr. Syme, I think there is no doubt that the spasm is occasionally higher than the cardia, certainly at the hiatal level. One can easily obtain confirmation on that point, by observing in a case of cardiospasm the spasmodic closure of the hiatal gullet when the mucous membrane a short distance above is irritated.

Some Practical Points in the Removal of Foreign Bodies impacted in the Food and Air Passages.

By Sir WILLIAM MILLIGAN, M.D.

THE removal of foreign bodies which have become impacted in the food or air passages presents at times serious difficulties and calls for much thought as to how the desired result may best be obtained. In general it is advisable to work in close co-operation with the radiologist, but not necessarily to accept dogmatic statements as to the presence or absence of foreign bodies. Where the radiologist is able to give positive evidence which is in harmony with clinical findings the surgeon's position is very materially strengthened, but

where on the other hand the radiologist's report and clinical indications are at variance my own inclination and practice is to rely upon a careful endoscopic examination. Many foreign bodies are not sufficiently opaque to show in the "negative" and on a few occasions I have found myself seriously embarrassed by relying too implicitly upon radiographic reports.

In certain cases in which the foreign body is lying deeply in the lung, e.g., in a small posterior bronchiole, it is of great advantage to work with the aid of the fluorescent screen. In several cases which have passed through my hands I doubt very much if the foreign body would ever have been successfully extracted had it not been for the great assistance rendered from working under the guidance of the "screen."

Smooth foreign bodies such as glass beads, pebbles, &c., when impacted in a bronchus are frequently very difficult to extract, their smoothness preventing the forceps maintaining a firm grasp and the tendency to a negative atmospheric pressure in the deeper portions of the respiratory tree causing a suction movement which tends to increase the firmness of impaction.

When a suitable case presents itself it is my intention to try Loewenberg's "glue method" as applied to foreign bodies impacted in the external auditory meatus.

The fate of foreign bodies impacted in the respiratory passages and the damage they may cause is a somewhat moot point. My experience is that all hurry and bustle should be avoided. Foreign bodies do remain and have remained quietly at rest in the deeper portions of the respiratory tract for long periods without causing any serious damage. I do not imply by saying this that early attempts should not be made to remove them, but I wish to deprecate any undue haste in undertaking operative procedures.

It is an interesting speculation as to what becomes of certain impacted metallic foreign bodies in the deeper bronchioles. Do they ever become absorbed by the pulmonary juices? In a case recently under my care in which several unsuccessful attempts were made to extract an ordinary pin from a posterior bronchiole in a child's lung (the pin was 1 in. above the level of the diaphragm) the pin was observed by the radiologist from time to time over a period of three months. During that time the little patient was never known to have had a severe fit of coughing and the greatest care was taken to be on the look-out for the pin should it by any chance be suddenly ejected.

Yet the pin disappeared from the small area of consolidated lung originally surrounding it as shown by radiography. Though improbable it is just possible that it has become dissolved. One other case of a very similar nature has occurred in my practice.

In very young children, especially where a prolonged examination is contemplated or has actually taken place, the question of tracheotomy should be carefully considered. I am in favour of an upper bronchoscopic examination, never having found much assistance from a lower bronchoscopy, but at the same time I fully realize the tendency there is to œdema of the tracheal and bronchial mucosa in young children after prolonged instrumentation, however carefully and gently the examination may have been made, and with the smallest tubes possible. In certain difficult cases in children I have performed a tracheotomy *after* the extraction of the foreign body as a safeguard and have reason to think that at times it has saved the situation!

I have placed in the Museum a selection of foreign bodies from my collection many of which were exceedingly difficult to extract and presented interesting problems as to how best to effect successful removal. It is difficult to lay down hard and fast rules. Much depends upon circumstances and upon the armamentarium one happens to possess.

Summary of Fifty-one Foreign Bodies removed from the Food and Air Passages. (Exhibited in the Museum.)

(1) *From the Œsophagus.*—A total of twenty-nine, consisting of twelve coins, one button, six dentures, three safety-pins, one brooch, one ordinary pin, one piece of shrapnel, and one piece of a toy. One coin was impacted and removed from the cardiac orifice of the œsophagus. Amongst the twelve coins the longest sojourn in the œsophagus was six months. One of the safety-pins was removed from an infant aged 3 months.

(2) *From the Air Passages.*—A total of twenty-two: *Larynx*, three: consisting of one denture (*in situ* three weeks), and two fishbones. *Trachea*, three: consisting of one coin (lying on carina), one denture (inhaled by an epileptic), one paper clip (infant). *Bronchi*, sixteen: consisting of two safety-pins, one tie-pin, one ordinary pin, one paper clip, two pieces of bone, one collar stud, two damson stones, one portion of macerated lentil bean, two pieces of toy trumpet, one brass-headed carpet tack, one pebble, one piece of dental instrument.

It is interesting to note that of these foreign bodies six were situated in the right bronchi and ten in the left, which is the reverse of the general opinion that foreign bodies are more apt to be inhaled into the right bronchus. Amongst the number of foreign bodies in the respiratory tube only one, the piece of dental instrument, (located in a deep part of the left bronchus) was spontaneously coughed up.

DISCUSSION.

The CHAIRMAN: These matters brought before us by Sir William Milligan are of extreme importance. As he remarked, his paper raises the question whether time should not be allowed to see what will happen, rather than to operate immediately the foreign body has been swallowed, as has been perhaps too generally done.

Dr. WRIGHT: A case I relate may serve as a warning to others. It has been my custom, after a submucous resection of the septum, to insert plugs consisting of rubber glove fingers filled with gauze, and until recently it was my habit to secure these anteriorly. I have prepared them specially for each case, so that they fit well. I had operated under general anæsthesia, but while the patient was recovering from the anæsthetic he managed to inhale both plugs to the bifurcation of the trachea. Fortunately this accident occurred just as he was leaving the theatre, I noticed the plugs were missing and did a tracheotomy straight away, but no air entered the tracheotomy wound. Securing a pair of Paterson's direct laryngeal forceps I inserted them through the tracheotomy wound, and removed both the plugs, and the man recovered. I still do not know how the accident happened: the plugs were 3 ins. long, and were a tight fit. How they passed end-on through the glottis is a marvel to me.

Mr. HERBERT TILLEY: Sir William Milligan's paper is one of the most thoughtful communications, based on practical experience, which we have had in this section since the direct method came into vogue. I do not wish to criticize, but, rather, to corroborate his experiences. The question of radiography is very important, and I think all patients should be screened in whom we suspect the presence of a foreign body, whether it be in the gullet or in the air passages. Even when there is a foreign body which is not opaque to X-rays it may set up an inflammation which may project a shadow and arouse suspicion. In the air passages it is necessary that the exposure be instantaneous. I had an experience which was the reverse of Sir William Milligan's. The radiographer in this case insisted that a foreign body was in the lung, and I equally doubted this view. The mother stated that the child had "swallowed a glass bead, and was coughing." I saw the child, who was happy and comfortable: there was no stridor nor distress in breathing. I asked for a radiogram. The report stated that there was a *button* in the

right upper bronchus. I pointed out that the child had swallowed a bead, not a button, but the radiographer adhered to his report. We put the child under an anæsthetic, and I searched carefully in the bronchi but found nothing. A further radiogram was taken, and the radiogram showed a button in the lower part the upper lobe of the right lung. I pointed out that the child must have stood on its head and given an inhalation for this to have happened. Further investigation elicited the fact that the child in wriggling under the anæsthetic had worked its shirt up from the waist so that the button was opposite the right lung, and in that position the radiograph was taken. We must always give weight to the history of the accident especially if it is confirmed by a person who was present at the time, but our diagnosis should not rest on any one finding. The second point raised is as to the solubility of pins and other metallic substances by the pulmonary juices. I doubt whether lung tissue will dissolve these bodies, and we know how many years some of them have been *in situ* without, apparently, doing harm, until some conbination of disturbances lead to their exciting active symptoms. I think it more likely that the pin Sir William referred to was momentarily freed from its position and was coughed up, or that it became lodged in a position near the root of the lung, where it was not revealed by the skiagram. I remember dividing in the bronchus a pin which I could not remove whole. I removed half of it but lost the remainder, and three days afterwards, when I went into the ward, the sister brought me the other half, which had been coughed up. With regard to the patient needing tracheotomy from the point of view of the subglottic œdema, as Chevalier Jackson pointed out in his book, the patient is suffocating in his own secretion, and the indication is to suck out the secretion with a tube. But, as Sir William would say, if you rely on having to do this afterwards, the patient may in the meantime suffocate for want of tracheotomy. I do not see that we can decide the point. It will be of a certainty less frequently necessary if men will get into the way of using the small calibre tubes for little children, rather than one which causes stretching of the soft tissues when it is being passed through the glottis; it is this undue dilatation which causes œdema and it should be carefully avoided.

Dr. D. R. PATERSON: I would not only lay stress on the first point made, but I would go further: I consider that in future a properly equipped throat department will have attached to it an X-ray equipment. That is so already in a number of continental clinics where the attached radiologist knows intimately the anatomy of this region and the necessary sinuses as well as the varied appearances which may present themselves in the relations of foreign bodies. With regard to the disappearance of foreign bodies in children, I have seen an illustration of this. It was that of a child admitted to the hospital with pneumonia and the history of a foreign body having been swallowed. We saw, by the X-rays, a tack impacted in a bronchus. There was a temperature of 103° F. I explained to the parents that it ought to be taken out, but nothing would persuade them to allow the child to take an anæsthetic. The child

went out, and sometime afterwards when it came up again, no trace of the foreign body could be seen. As Mr. Tilley suggests there is a possibility that it may have shifted suddenly. One knows how suddenly foreign bodies do come out. The only other point I would make is that concerning subsequent tracheotomy. Sir William limited this procedure to cases in which there was much difficulty about the extraction of the foreign body. I would put it the other way. My experience is that where there is a likelihood of difficulty in extraction, an initial tracheotomy is a procedure to be recommended because it renders the case easy to deal with. It gives the advantage of being able to use a shorter tube of larger calibre without adding materially to the risk of the the whole procedure. I agree there is harm in passing tubes which stretch the parts.

Sir StClair Thomson: The most valuable part of Sir William Milligan's delightfully clear and terse paper is that in which he recorded his two failures, and I think this debate would be of greater value if we were all to record our own. All clinics, and some consulting rooms, are decorated with votive offerings of successfully removed bodies. Most of them are metallic or other solid bodies, partly because of the X-ray help which can then be given, and partly because with them sepsis does not so readily arise. One of Sir William's failures was the case of the pea which had shed its husk. In America practitioners have more experience of similar cases than we have, because of the habit there of chewing what are called "pea-nuts," and hence their experience is worth listening to. I willingly put on record my one and only failure, because doubtless I may later have more. It had to do with a foreign body different from any represented in the specimens which have been handed round. A young Indian Prince, training in this country, went to the theatre, and was chewing a "betel-nut" when something made him laugh and the nut disappeared from his mouth and entered his lung. Within twenty-four hours he had a temperature of 102° F. and a violent cough. I did not appear on the scene until five days later. Little was seen in the skiagram, but very little air was entering the lung in the right lower lobe. We proceeded to endoscopy, with the help of Dr. Irwin Moore, who will confirm my statement that it was a very difficult case owing to the cough. Though we had an expert anæsthetist and I used as much cocaine as I dared, we never abolished the cough. We found the nut impacted in a bronchiole of the right lower lobe, and after many vain efforts to get round it and loosen it, I had to make a frontal attack. It was soft and macerated, and came away in fragments. In spite of the cough we worked at it one and a quarter hours, and we thought we had got it all out. The physician reported that air was entering everywhere except in a small lobule of the lung, and for two days we thought that all would be well. But this improvement was deceptive, because he again ran a temperature, and began to spit up pus. In spite of my request, his medical adviser and family declined to allow a second endoscopy for the search for any remaining portion. Finally a portion of rib was resected and pus in a bronchiole was found. When he came round from the anæsthetic he coughed, and there, on his lip,

was a fœtid mass of necrosed cartilage and some vegetable fibre. It was now thought that all would be well, and I was shocked at receiving a letter saying he was dead. It seems that when the period of relief passed, his temperature again oscillated, he had pain in his head, and he died of septic meningitis. If I had this case over again, I would ask for an endoscopy within twenty-four hours, I would make more careful efforts to get round the foreign body and loosen it and get it out *en bloc*, and I would urge a second endoscopy. It was a privilege to see Chevalier Jackson at work last year. We have all read his book, but we have not all had the opportunity of seeing him at work. He is in no hurry to remove a foreign body unless circumstances call for haste. I had the idea that he had a special train and travelled all over the United States removing foreign bodies, and I told him I thought his must be a jolly life! But it is exceptional for him to go to a case of foreign body: he insists that the case shall come to him, and any real hurry is unusual. When he gets the case into his clinic he takes time, and has a complete overhaul before he even "tubes" the patient, as it is called there. There is examination by X-rays, by a physician, and if there is expectoration, it is examined by a bacteriologist, before he proceeds to endoscopy. Mr. Rose and I saw seven cases endoscoped in one afternoon, and they were all done without a general anæsthetic and without cocaine; I do not think they even had an injection of morphia. It is done largely by suggestion, and by Jackson's wonderful influence over his patients. Children are rolled in what is called a "mummy wrapper." He lets nobody but himself look down. He passes his tube, and as soon as he has located the foreign body and taken stock of it, he mentions the instrument he wants, and when it is handed to him he takes out the foreign body generally at the first attempt. I am sure many will lay this lesson to heart, because it is when we proceed to "show off" the foreign body, and indicate how quickly we are going to take it out, that we miss it, or it shifts. Next, there is the strict trouble Jackson takes over everything being sterile; for every case he changes his linen and gloves and outfit, and does not touch the next patient with anything he had on at the preceding operation. In the large cities there they do not leave the extraction of foreign bodies to any laryngologist, but to men who have special experience in this work. In America, in fact, there is a society of per-oral endoscopists.

Dr. LE MAITRE (in French, translated by Sir StClair Thomson): I would first urge the great importance of collaborating with the radiologist, the same one each time, not going from one to another. During the war I was in charge of a large facial repair hospital at Vichy, where a large amount of original work was done, and I removed forty-three foreign bodies, chiefly from the sinuses and pterygo-maxillary fossæ. I not only use X-rays to locate them, but I believe in intermittent examination by means of the screen, the screen being placed beneath the patient. In my early days, sixteen years ago, a child had a screw in its lung. I did not get it out, and an abscess formed. The screw was then evacuated from the abscess, and the child made a good recovery.

Dr. WILLIAM HILL: I readily acknowledge I have had failures. The last one occurred when I was about to leave St. Mary's. On the X-ray table I tried to get away a pin which I had been unable to remove on two previous occasions, and I failed. The pin lay obliquely in the right lung. I went down in every direction, and the X-rays showed me when I was right over it apparently, but when the patient was turned over I found I was an inch or two to one side. At last I got the end of the tube very near to the point of the pin. There was a temptation to put a knife down and tunnel the tissue, but I resisted it and left the pin alone. I have learned that the patient has not since had a symptom, though the pin is still there. It is not unlikely that an abscess may form, and from that abscess the pin may be evacuated. In the meantime the patient is none the worse for my severe manipulations. I also failed to extract a paper-clip from the lung of a child at Southampton, and I subsequently heard of that child's death. I never found it; it was in the upper right lobe; it had been inhaled upwards, and I could not get round the corner. Chevalier Jackson has an instrument for this manipulation. I suppose the death of the child took place from bronchiectasis.

Dr. IRWIN MOORE: I would remind members that this is the third time Sir William Milligan has brought his specimens up from Manchester and on two occasions has been prevented from exhibiting them. I would like to make some comments on his remarks. With regard to co-operation of the endoscopist with the radiologist, this is useless if the radiologist is going to leave in the middle of a case and let an assistant carry on, or perhaps a nurse who is said to have some experience in this kind of work. There should be an expert radiologist always in attendance whilst a foreign body is being removed with the assistance of the fluorescent screen, who can screen the case and interpret what is seen, and guide the instruments of the endoscopist. The next point is that in London we have not got, in my opinion, satisfactory tables for the combined removal with radiography of foreign bodies. On one occasion whilst removing a foreign body I was receiving violent shocks from loose wires, which incapacitated me for some minutes. I agree that a negative or positive statement ought not to be accepted without question. Recently I had a case sent to me unaccompanied by a skiagram, and with the bare statement that the patient had swallowed a plum-stone shown by X-rays to be impacted in the œsophagus. On endoscopic examination no plum-stone was present and I could not find anything wrong. The appearance in the X-ray picture was probably due to a shadow, or simple blurring of the negative. It is very important that skiagrams should be taken in the antero-oblique position, for in this position the heart and vessels are not so much in the way. The importance of working with the fluorescent screen in the removal of foreign bodies was shown by Mr. Rose at one of our recent meetings[1] in the case of a tooth impacted in the lung. This is a difficult foreign body to remove and Mr. Rose attributed his success to the satisfactory co-operation of his

[1] *Proceedings*, 1920, xiii (Sect. Laryng.), p. 163.

radiographer colleague, who screened for him. Sir William Milligan tells us of his failures, but I think I am right in saying that his failures have not been due to any deficiency in technique nor in skill, for he has shown us the most remarkable one-man collection of foreign bodies in this country. In the two cases which he classes as failures, the position of the foreign bodies in a posterior lower lobe bronchus accounts for the failure. Chevalier Jackson reports several failures with pins in a similar position and this authority concludes that bronchoscopy has its limitations. The cases reported by Sir William Milligan support this view. I do not think we use tubes long enough and of small enough calibre for success ; we want tubes more the length and diameter of those originally introduced by Killian, so that we can get further down into the lower and smaller bronchioles. Sir William Milligan says he has not found in the literature a case of a pin impacted in a lower lobe bronchus which has been coughed up, but I would remind him of a very interesting case occurring at the London Hospital, reported by Mr. Hunter Tod [1] to this Section in 1917 and 1919, in which all the difficulties referred to were experienced in connexion with a pin in a lower posterior lobe bronchus. The pin was never actually seen by direct vision but was observed by X-rays close to the endoscopic tube, advancing and receding apparently within grasp of the forceps blades. After several failures of bronchoscopy, Mr. Tod asked the opinion of this section as to further treatment, since a surgical colleague was anxious to remove the pin by pneumonotomy. Though no definite opinion was expressed, members favoured a waiting policy. Mr. Tod decided against operation and later events proved that he was right. Ten months later the child had an attack of pneumonia, which ran a typical course, without leading to a pulmonary abscess, and apparently had no relationship to the foreign body still *in situ* in the lung. Recovery followed, whilst the pin was coughed up four months later after sixteen months' sojourn in the lung. It was corroded. Another case has also been recorded by Colquhoun[2] (New Zealand) in 1886. Mr. Kellock[3] has recorded an interesting case in which he performed thoracotomy successfully for the removal of a pin in a lower lobe bronchus after several attempts at bronchoscopy had failed. Cases should not, however, be given up and handed over to the general surgeon for thoracotomy because of failure by endoscopy after the first, second, or even third attempt. Success has come after several failures. If the few foreign bodies we fail to retrieve are left alone a local abscess may form and there is a better chance of the foreign body being coughed up. In fact records show a large number of foreign bodies so coughed up during pre-bronchoscopic days. I do not think it is likely that a metallic body such as a pin can be dissolved by the secretions of the lungs in so short a time. I think it is more likely to become encysted and being sterile it will not cause trouble, or it may travel to a part where it

[1] *Proc. Roy. Soc. Med.*, 1917, x (Sect. Laryngol.), p. 90. Also *ibid*, 1919, xii, p. 60.

[2] *Austral. Med. Journ.*, N.S., viii, p. 489.

[3] "A Case of Pneumonotomy for Foreign Body," *Proc. Roy. Soc. Med.*, 1912, vi (Clin. Sect.), p. 64.

may not be shown by X-rays. I would advise members not to worry when confronted with a case of foreign body. If the first attempt at removal fails, wait a few days whilst keeping watch on the patient, then make a fresh attempt. Twenty-six years is not a record stay for a foreign body in the lungs. Gross[1] reported a case in which a piece of roughened bone measuring $\frac{3}{4}$ in. in length and $1\frac{1}{4}$ in. in breadth, was inhaled into the air passages of a child aged 3, and expelled sixty years later during a fit of coughing. Chevalier Jackson[2] has recorded a case in which he removed a collar-stud which had been impacted in the lungs for twenty-five years. When a foreign body in the lung gives rise to irritation and cough, it is a sign of great importance, and indicates the necessity for early removal. You cannot allow the patient in such a case to go away and be watched by somebody else. You must keep in touch with him and make fresh attempts at early removal. With regard to the slipping of forceps, the most difficult and dangerous class of foreign body with which we have to deal are those rounded and liable to absorb moisture readily, such as peas or seeds. The forceps now shown I designed to securely grasp such bodies. They do not disintegrate or break them up as in the case of other forceps, and thus the risk of fatal pneumonia caused by small pieces being left behind is avoided.

Mr. WORTHINGTON (Exeter): In connexion with metallic bodies in the lung, there are a large number of people who are carrying such bodies in their lung, most of them as relics of the war, and attempts at removal have not been thought necessary. These fragments are supposed to be encysted, and in most cases have caused no trouble. In cases coming to hospital in the next ten or fifteen years with a history of pneumonia, the possibility of a foreign body should be gone into, and a skiagram taken. I think radiographs of soldiers taken during the war are being carefully kept by the War Office, and they may enable us to decide whether metallic bodies can be dissolved on a long stay in the tissues. Mr. Tilley says he would X-ray every case with a foreign body in the œsophagus or the lung. I cannot quite agree with that. In the vast majority of cases of foreign body in the œsophagus there is no question about the case, and the sooner such body is removed the better. In Exeter I make it a rule that if a case of foreign body in the œsophagus is admitted, an attempt at removal is made on the same day. If it is not then at the point to which it was first referred by the patient, we know it has moved, and that search must be made lower down; there is as a rule no need for X-rays. I think the cases in which a skiagram is necessary before endoscopy are those of foreign body in the lung, for there you have to determine the plane in which the foreign body lies. Sir StClair Thomson has urged us to confess our failures. One failure of mine raises a point of interest in that it presented a difficulty I had not experienced before. A patient came

[1] "Foreign Bodies in the Air Passages," Philadelphia, 1884, p. 172.

[2] *Trans. Seventeenth Internat. Congress Med.*, Lond., 1913 (Sect. xv, Rhino-Laryngology), p. 60

with the history of having swallowed a tooth-plate. I did not have a skiagram taken, the sensation of the presence of the plate being very marked at about the sternal notch. But I could not find it. I then had it X-rayed, and there it was. I removed it at the second attempt. The reason I could not find it the first time was, that it was impacted just beyond the entrance of the œsophagus, in the anterior wall. Two incisor teeth of the plate were embedded in the wall, and the colour was such a perfect match of the colour of the œsophageal wall, that if I had not caught sight of the edge I should not have distinguished the plate. Such a possibility should be borne in mind.

Sir WILLIAM MILLIGAN (in reply): Some interesting points have been raised. I have had a precisely similar case to Dr. Wright's, in which the glove fingers filled with gauze escaped and one entered the trachea. But I have, for years past, made it a rule to use a salmon-gut suture to join the two plugs together. I expected Mr. Tilley, with his analytical mind, would have another theory at hand. I cannot disprove it: it is merely a speculation of mine with regard to the dissolving of metallic foreign bodies in the lung. With regard to small-calibre tubes, I have taken his remarks to heart: he has frequently advocated them, and I agree as to their value, seeing the necessity for exercising great gentleness in manipulation. But when the child is under the anæsthetic for a considerable time—and I have no hesitation in keeping children under for an hour or longer—there is some element of irritation, even if there is not actual trauma. It is in connexion with this class of case I raised the question of tracheotomy. In answer to Dr. Paterson, it is a question of the advisability of doing tracheotomy in a case in which the patient has to be under the anæsthetic for a considerable time, and in which the possibility of traumatism may arise. Sir StClair Thomson made a remark with which I entirely agree, namely, as to allowing other people to look down the tube. There are one or two of my old house surgeons here, and they will bear me out when I say I do not mind students or nurses looking down the œsophagus, but I forbid it in the case of the lung: the condition of affairs in the two cases is entirely different. The case Sir StClair Thomson mentioned is very interesting, and if I have such a case I shall try the Loewenberg's glue method, as employed for removing foreign bodies from the ear. For the removal of foreign bodies I use Dr. Irwin Moore's excellent instruments. With regard to Dr. Le Maitre's remarks, and the possibility of an abscess forming, that is just what I try to avoid. That is the danger when a case is left long. Even if we fail at first, we should still continue to try to remove the foreign body. Dr. Irwin Moore does not think the pin will dissolve. He is quite entitled to hold that opinion: I cannot disprove it. I shall go on working on the matter until I reach some decision. I have experienced the greatest comfort in using his instruments, and I appreciate them as much as does anyone in this room. Mr. Worthington remarked about foreign bodies being encysted, but it is a different proposition in the case of a foreign body in the lung as compared with one in a bronchiole. The discussion has been valuable in eliciting cases of failure, because it is only by them that we learn how to act in the future.

Latent Sinusitis in Children.

By P. Watson-Williams, M.D.

(ABSTRACT.)

While infective sinusitis in adults, associated, as is usually the case, with symptoms suggestive of a sinus infection, is very commonly suspected and diagnosed, similar conditions arise in children of all ages, but are more likely to remain undiagnosed unless attended with acute or manifest symptoms.

Acute sinusitis in infants is rarely localized, but involves the nasal mucosa and the relatively undeveloped sinuses as a whole, usually with symptoms of purulent rhinitis, which may become chronic or subside into a recurring non-purulent nasal catarrh or eventuate in cure; or, on the other hand, result in producing a large area of external suppuration as described by Skillern.

In children between the ages of 5 or 6 and 15 years a nasal sinus infection is more liable to be localized, and occasionally with symptoms such as lead us to suspect nasal sinusitis in adults.

A chronic or recurring nasal catarrh in a child is usually attributed correctly to adenoids or to infected tonsils, and if the symptoms persist after an operation for the removal of tonsils and adenoids, the child is sometimes supposed to have a recurrence of enlarged tonsils and adenoids, and a second and sometimes a third operation is performed.

The symptoms of chronic latent sinusitis are essentially similar to those of infected tonsils and adenoids, viz., a recurrent nasal catarrh, buccal respiration, catarrhal deafness, aprosexia, mental backwardness, and chronic sepsis. In the sphenoidal sinus cases especially other manifestations may arise, such as restriction of visual and colour fields and so forth, but if they are not suspected and sought for these symptoms pass unnoticed.

More serious results of chronic sinus infection are the results of infecting organisms passing continually on the one hand into the gastro-intestinal tract and sometimes infecting the appendix; while, on the other hand, the bronchi and lungs may become involved—bronchitis, pneumonia, and possibly a tuberculous infection being thereby determined.

Diagnosis: A persistent anterior unilateral nasal discharge is highly suggestive of a sinus infection, provided the presence of a foreign body

is eliminated. A posterior rhinoscopic view showing pus or muco-pus in one choana is similarly strong evidence of a nasal sinusitis. It is necessary to make examinations on more than one occasion to eliminate the possibility of an adventitious collection being mistaken for a persistent secretion.

The most useful methods of examination in the writer's experience are: (*a*) endo-rhinoscopy, and (*b*) exploration of the sinuses by the suction syringe.

(*a*) Endo-rhinoscopy in children, from the age of 7 onwards, is often possible with local anæsthesia, but, on the other hand, is often impossible without a general anæsthetic. When a definite streak of pus is observed to be coming from the spheno-ethmoidal region or from the middle meatus of one or both sides, the evidence of the corresponding sphenoidal or antral cavities being the source of the purulent discharge is almost conclusive.

(*b*) Exploration of the maxillary antra or sphenoidal sinuses by means of the suction syringe is most valuable, but necessitates a general anæsthetic in children. If definite pus is sucked into the syringe from a maxillary antrum or a sphenoidal sinus, one has clear proof of the cavity being infected. Unfortunately, the exploration of the sphenoidal sinuses in young children is far less easy than the exploration of the maxillary antra and is not so free from risk. Nevertheless, the exploration of the sphenoidal sinuses in children is usually a fairly easy and safe procedure provided one is accustomed to such investigations in the adult.

Example: Miss R. H., aged 11. July, 1917: Was brought on account of recurring colds which persisted, "inability to apply her mind," anæmia, buccal respiration, &c. The child was referred to me as a case of adenoids. Anterior rhinoscopy: No pus, the nasal passages narrow. Posterior rhinoscopy: A small pad of adenoids seen; no pus. Endo-rhinoscopy: Right side clear; left, a definite stream of bubbly muco-pus apparently from the left middle meatus, with some pus in the left middle turbinal. Fauces: A slight enlargement of the left tonsil; some post-palatal muco-pus. After several examinations she was operated on in September, 1917. The sinuses explored: Exploration of the maxillary antra, clear on both sides. Exploration of the sphenoidal sinuses: Right, $3\frac{1}{8}$ in. from nose tip to the back wall, washing clear; left sphenoidal sinus at least a drachm of thick yellowish-white pus was sucked out. The sinus measured 3 in. from nose tip to the back wall, which felt boggy. Cultures: From the antra, no pyogenic organism;

from the right sphenoidal sinus, occasional staphylococcus; film examination, no polynuclears; from the left sphenoidal sinus, culture *Staphylococcus albus* and streptococci. Film examination: Polynuclears and phagocytosis of cocci.

Other examples of latent sinusitis in children have been recorded by the writer in the *Journal of Laryngology*, 1920, xxxv, No. 4, and the records of several other illustrative cases were read.

DISCUSSION.

Dr. SYME: I do not agree with the description of these cases as latent: indeed, it would be better if the term were dropped altogether. In the great majority of cases of sinusitis the condition is only apparently latent because sufficient care is not taken in the methods of diagnosis. Not only in children, but also in adults the sinuses should be explored, the antra, for instance, by proof-puncture or by Dr. Watson-Williams's method, on the symptoms alone. If only those cases are explored in which on rhinoscopic examination evident signs of sinus disease are found, a large proportion—probably the majority—of cases of sinusitis will be overlooked. I agree with the reader of the paper that sinusitis is much more common in children than is usually supposed. I have notes of about thirty cases of antral disease in children under 12 or 13 years of age on whom operation was carried out, and on looking over the notes of all my cases of antral disease, I have been struck with the frequency with which it has been found, especially in females, about the age of 16. In those cases the lining has been found at the operation extensively diseased, pointing to long duration of the affection. Sinus disease in children should be taken more seriously, and submitted to operation with less delay, than in adults, because in children it tends to produce atrophic changes—whether true atrophic rhinitis or not, I cannot say—in the nasal mucosa. I am somewhat sceptical about purulent rhinitis as an entity: at any rate I should only arrive at it as a diagnosis after careful examination had excluded sinusitis or some other cause for the discharge. Neither in children nor in adults do I make use of the intranasal procedure on the antrum: I always perform the complete antral operation, and I thoroughly curette the diseased lining membrane.

Mr. H. LAWSON WHALE: I have often wondered why the terms "latent" and "manifest" have so long survived in the text-books. The only possible clinical classification of sinusitis would be "closed"—which soon becomes a fulminating empyema—and "open."

Mr. H. J. BANKS-DAVIS: I look upon Dr. Watson-Williams's observations as of great importance. Sinus disease is often undetected, as the symptoms complained of often point to nasal catarrh only. A continued post-nasal discharge should always suggest the possibility of sinus disease. In my own

clinic all these cases are examined by transillumination as a routine, and it is surprising how frequently a shadow may be observed and the origin of the rhinitis thereby traced to its foundation. I think the operative treatment of sinusitis and its results are not very satisfactory, for even after a well-done operation patients are not free of discharge, and sometimes crusts: patients expect an operation to "cure" them, and they become disappointed at having to continue lavage for a long time afterwards, as many do. I am in the habit of telling them that for mechanical reasons antral cases, unless acute, cannot get well without a suitable operation, which will drain the abscess cavity, but that I can give them no promise as to arrest of secretion unless treatment is continued afterwards by their own selves often for many weeks and even months.

Mr. F. H. WESTMACOTT: After many years' experience at the Manchester Children's Hospital, I have only observed two cases of suppuration of accessory cavities up to 14 years of age. Those were maxillary antral abscesses, and due in each case to a carious non-erupted canine tooth, which was lying horizontally in the abscess sac. Both were boys aged about 12. The accessory cavities in children are small, and the ostia large. If suppuration takes place I believe they are not closed empyemata, and that they go on to spontaneous cure, or are remedied by the nasal douche. In the case of open empyema, it is rarely, if ever, necessary to do more than wash it out with a sterile solution. Transillumination I regard as fallacious in children, on account of the crowding together of the teeth in the maxilla.

Mr. HERBERT TILLEY: I would emphasize the necessity for remembering that the polyarthritis of children may be caused by the absorption of infective material from the nasal accessory sinuses. In children a slight degree of evening pyrexia often accompanied joint inflammation, while in adults the latter are often unaccompanied by pyrexia. This may be explained, perhaps, by the more ready absorption which occurs in the softer tissues of childhood. Mr. Banks-Davis's remarks rather favour the view that opacity of an antrum indicates the presence of an empyema, but my experience is that many healthy antra are dark when transilluminated, and the test is only of value in so far as it bears out other objective symptoms. I do not share Mr. Banks-Davis's pessimism as to the incurability of antral empyemata. In many cases in which there is a continuation of purulent discharge or crust-formation after the operation, it is because the surgeon removed all, or too much, of the lining mucous membrane of the sinus, so that it is replaced by granulation tissue, and this will secrete some pus, which then dries into crusts. An extensively diseased antral mucous membrane will return to its normal condition if free, spontaneous and permanent drainage be established.

Mr. J. F. O'MALLEY: I quite agree with the author of the paper that sinusitis is more common in children than one has been accustomed to believe. If he found a case with sepsis in the nose and nasopharynx, such as is present in so many cases of tonsils and adenoids, with ill-developed nasal

passages, would he explore the sinus, or would he be content to remove the tonsils and adenoids, and await events? I can quite understand that where one finds nasal and post-nasal sepsis, and very little adenoids and ocular disturbances—such as contraction of visual field—it is our obvious duty to explore the sinuses.

Mr. NORMAN PIKE: I am struck with the great depth that Dr. Watson-Williams has gone in some of his sphenoidal sinus cases, in one instance over 4 in. from the anterior nares and that in a young subject, that is over 10 cm. I have always found the distance to vary between 7 and 8 cm. from the anterior nares, this depending on whether the patient is a male or female. I have always thought that this measurement is a constant one, but such variations as those in Dr. Watson-Williams's cases have upset my ideas on the subject.

Dr. IRWIN MOORE: This paper is a most important contribution to our present knowledge on the subject. Suppuration of the nasal accessory sinuses in children, though not so common as in adults, has not received the attention in this country which it deserves. It would appear to be more frequent than is generally supposed, and accounts for many cases of systemic infection which remain undiagnosed, because a sinus infection in children is supposed to be so rare, that it is not commonly suspected or looked for. The importance of this subject has been shown by many valuable contributions from our confrères in America. With regard to the question asked by a previous speaker as to the size of the sphenoidal sinus in children at varying periods from birth upwards, there appears to be a considerable diversity of opinion.[1] I have recorded at a meeting of this section on February 2, 1917,[2] a sphenoidal sinus the size of a hazel nut in a child aged six years—from which the pedicle of a large sphenochoanal polypus had originated.

Mr. MARK HOVELL (Chairman): Mr. Tilley has done good by calling attention to arthritic symptoms in connexion with suppuration of the nasal

[1] *Postscript.*—G. B. Wood (Philadelphia), *Trans. Amer. Laryngol. Assoc.*, 1914, p. 115, states that, from sections of the sphenoid he had made, he had never found the slightest signs of a sphenoidal cavity in children at six months of age; that the sphenoidal sinus can only be recognized as a small indenture at the first year; and that in the majority of cases it is not of sufficient size to be of any importance until the sixth year. Laurent (Brussels), *Journ. of Laryngol., Rhinol. and Otol.*, 1900, xv, p. 207, expresses the opinion that the sphenoidal sinus is the last to develop, following the frontal sinus, which he says develops probably about the second year. Ingersoll (Cleveland, Ohio), *Laryngoscope*, 1909, xix, p. 902, remarks that it is so rudimentary until the seventh year that the cavity is not apparent. On the other hand Mosher (Boston), *Trans. Amer. Laryngol. Assoc.*, 1914, p. 129, confirms the generally held view that at birth the sphenoidal sinus has a well-formed ostium and a small cavity—the height, length, and width being 4 mm. by 2 mm. by 2 mm.; at 3 years of age there is a sizeable cavity 6 mm. by 6 mm. by 9 mm., and at 7½ years it measures 12 mm. by 13 mm. by 9 mm.—i.e., ½ in. in its chief diameter. Coakley (New York), *op. cit.*, has proved by autopsy that the size of a sphenoidal sinus in a child of 1½ years was that of a hazel nut. Again, Mosher (*op. cit.*) has found a sphenoidal sinus of average adult diameter in a boy, aged 12.

[2] *Proc. Roy. Soc. Med.*, 1917, x (Sect. Laryngol.), p. 67.

accessory cavities, for auto-sepsis resulting from this condition is frequently met with, and not sufficiently recognized as a cause to insure a search being made for its origin. Last January a gentleman who brought his daughter for deafness said he had pain in the shoulder and asked whom he should consult. When he brought his daughter again two months later he asked me to look at his left nostril which was uncomfortable, and I found there was suppuration in the left maxillary antrum. The pain in the shoulder disappeared on removal of the purulent discharge. In a paper which I read ten years ago when speaking of the connexion which exists between granular pharyngitis and nasopharyngeal catarrh with vaginal discharge, I mentioned a case which occurred in the practice of a man who was aware of this relationship. He saw a patient suffering from nasopharyngeal catarrh, granular pharyngitis, and arthritic knee pains, which had been diagnosed as gouty. On investigating the cause of the vaginal discharge a forgotten pessary was discovered. After the removal of the pessary and vaginal discharge the arthritic pain disappeared, and the throat resumed the normal condition. With regard to Mr. Banks-Davis's remarks about transillumination, I think he places undue importance on this method of examination. I ceased to place reliance on it many years ago, for I found that often a translucent cavity contained pus and a dark cavity was free from discharge.

Dr. WATSON-WILLIAMS (in reply): I am very glad to hear that my thesis and the practical importance of the due recognition of these cases have been supported so generally. I regret that the limited time makes it impossible for me to reply to all the questions that have been asked. The series of examples of latent sinus infection in children is intended to show that these infections are sometimes the cause of recurrent adenoids and tonsils, in other symptoms indistinguishable from glandular fever, or polyarticular arthritis, headaches, aprosexia—while in others it seemed highly probable that the infection gaining access through the gastro-intestinal tract may cause appendicitis. The term "latent" sinusitis has been criticized but I consider that it is the term most applicable to the cases to which I have referred. "Latent" means hidden, and patients in whom a sinusitis exists but does not declare itself by symptoms accepted as indicating sinusitis may be said to have a hidden or "latent" sinus infection. I differ from Mr. Whale in his use of the terms "open" as applicable to these cases. Many "open" "sinus" cases are very obviously suffering from sinus suppuration. Moreover, such cases I have described in children are sometimes open and at other times closed. Furthermore, I have particularly excluded from my paper those cases which are exhibiting the usually accepted signs and symptoms of sinusitis, and my intention has been to emphasize the importance of seeking for a sinus infection in young patients who, though infected, afford only symptoms such as we usually attribute to adenoid growths. Such a latent sphenoidal sinus infection may be diagnosed as nasal catarrh and adenoids, and yet when carefully examined may be found to have a canalicular neuritis, and grave visual defects due to a latent sphenoidal sinus infection.

Per-nasal Dacryocystostomy.

By H. Lawson Whale, F.R.C.S.

(ABSTRACT.)

In considering the historical evolution of per-nasal dacryocystostomy, many of the past attempts may be disregarded, in that they neglected what now appears to be a cardinal principle—that the sac itself must be attacked, not the nasal duct. West's method is the prototype of such operations, and is simply a refinement of the techniques of Choronshitzky, Wiener, and Sauer, who obtained drainage by merely forcing a probe through a canaliculus into the nose, without any set manœuvre to remove discrete pieces of bone.

Indications: The rhinologist is limited in his choice of cases, since the selection is made for him by the ophthalmologist. Any case suitable for external excision is, at some period or other, suitable for per-nasal attacks.

Contra-indications: Acute empyema.

Temporary and surmountable contra-indications: Deviated septum; enlarged middle turbinal; sinusitis, especially ethmoidal or frontal.

Technique: It is of paramount importance to remove sufficient bone. The gouge cuts the bone almost parallel to its surface, and so tends to slip. Care is necessary to avoid formation of false passages in passing canaliculus probe. Support the nose from outside while chiselling is necessary, so as to avoid fracture-dislocations.

Complications and disappointments: Failure of operation to cure. Subcutaneous or subconjunctival ecchymosis. Abrasion of septum with subsequent adhesions. No need to fear that the patient will "blow his nose into his eye" subsequently.

Results: "Is this a good operation?" Since we only receive cases selected by ophthalmologists, this question is for them to decide. In the author's series of twenty-five, 63 per cent. are cures, 12 per cent. partly cured, and 25 per cent. failures. Longest duration to date of a cure is six years.

DISCUSSION.

Mr. J. F. O'Malley: I do not regard the operation as an easy one, and I speak from experience of some twenty cases. The indications for operation which guide me are: (1) Obstruction in the duct below the sac; (2) mucocele

or empyema of sac. It is important to make sure that the canaliculi are patent. A deflected septum is not, in my opinion, a contra-indication, as it can be quickly resected at the time. In fact it is a useful procedure in the case of a narrow nose, before trying to expose the lacrymal sac. A large opening in the bone is important. To find the sac, I make a small external skin incision, and insert a blunt probe to push the sac into the intra-nasal bony window. I tried injecting methylene blue into the sac, but it did not help.

Dr. D. R. PATERSON: I have had considerable experience of West's operation, and am pleased with the results. I am in the habit of resecting part of the septum when necessary. One contra-indication to the operation is a positive Wassermann reaction, because in those cases there is a tendency for the opening to close.

Dr. JAMES DONELAN: I am glad Mr. Whale has laid stress on the importance of getting into the lower end of the sac, rather than into any part of the duct. Mr. Howarth has further elucidated that point. I have done several cases, with varying fortune. I will only refer to one, which I showed here about three years ago, the case of a boy aged 11. Partly through a miscalculation, owing to the small space, I got into the sac instead of the duct and had a good result, while on the other there was failure, although the duct was opened. It is important, in choosing these cases, to know how much has been already attempted by the ophthalmologist.

Mr. W. M. MOLLISON: The operation I have found difficult, in consequence I modify the proceeding somewhat: the mucous membrane is incised in front of the anterior end of the middle turbinal and a sharp-pointed Watson-Williams's frontal sinus curette is driven through the bone and the point manœuvred through to the lower canaliculus—previously slit. The opening thus made is enlarged by the curette and a fine rubber tube drawn through to the nose from the canaliculus and the ends fastened together on the face; the tube is left in position for five or six days. Results are satisfactory. The youngest patient operated on was 4 years old and his epiphora was cured. In one case the operation was performed on an old woman with cataract and suppurative dacryocystitis. The ophthalmic surgeon wished the operation to be done in order to have a clean field for the cataract operation. After a preliminary increase in sepsis the condition cleared up.

Angeiomata of the Larynx.[1]

By Irwin Moore, M.B.

(ABSTRACT.)

A study of all the cases—seventy-one in number—recorded in literature, with special reference to the question of hæmorrhage, and the treatment of these tumours.

Illustrated on the epidiascope by drawings of the larynx in twelve of the recorded cases, showing the varying positions and appearance of these tumours.

Statistics of Morell Mackenzie, Fauvel, Heinz, Norris Wolfenden, Chiari, Jurasz, Schmidt, Schroetter, Lennox Browne, Wyatt Wingrave, Phillips and Ruh, Emil Mayer, from 1862 to 1916, together with extra cases (some overlooked by these authors) recorded up to June, 1920.

Classification.

(1) Hæmangeioma simplex, or local telangiectasis.

(2) Hæmangeioma cavernosum—the most common variety—with a roughened surface, and frequently pedunculated.

(3) Hæmangeioma diffusum or telangiectasis diffusa.

(4) Lymphangeioma.

(5) Mixed or atypical cases—e.g., angeiomatous papillomata, angeiomyxomata, angeiofibroma.

The *ætiology* is obscure. Some authorities consider that these growths are due to previous inflammatory conditions of the mucous membrane, and the history of catarrh of the larynx in nearly every case would appear to support this view. They are said to occur more frequently in those who abuse their voice, and some observers consider that there is a relationship between nasal obstruction and their development. On the other hand there appears to be evidence that they are congenital in origin and at a later period suddenly develop and assume rapid growth. Lymphangeiomata are undoubtedly congenital.

[1] This paper, *in extenso*, together with twelve illustrations, will be published in the *Journal of Laryngology, Rhinology, and Otology*.

Appearance.—They may be single or multiple, but are generally unilateral and single and vary in size from a lentil seed upwards to that of a walnut. They are seldom pedunculated and generally appear as raised plaques. They are generally of soft consistence; the hæmangeiomata being of a dark bluish-grey or purplish colour, of a racemose or blackberry-like character.

Age and Sex.—They may occur at any age but more frequently during the middle period of life.

Site.—These neoplasms may be found in any part of the larynx. The opinion expressed, from the earlier cases recorded, that they are generally situated near to the anterior commissure and usually on the right vocal cord is not confirmed by later records, which show that they are much more generally distributed, for amongst sixty-four cases in which the site of the growth was definitely recorded, thirty-three were situated on the vocal cords, thirteen on the left, eleven on the right; the position of nine was not stated, and only ten were close to the anterior commissure. Of the remaining thirty-one cases, four were on the ventricular bands; four originated from the ventricle of Morgagni; two were on the epiglottis; four on the aryepiglottic folds; two in the pyriform fossa; six on the arytænoid or in the inter-arytænoid region; and three were subglottic; six were of a diffuse character (in five cases partly originating from the vocal cord).

Diagnosis.

They may be recognized by a history of blood-stained sputum, or unaccountable hæmorrhage which relieves for a time any hoarseness present; by their colour and soft consistency, but a considerable number have not been diagnosed until after removal and microscopical examination. Their clinical recognition is important since these growths have been frequently confused with malignant disease, tuberculosis, syphilis and cysts.

Conclusions.

Question of Hæmorrhage.

(*a*) *In Unoperated Cases.*—Amongst the total seventy-one cases recorded, epistaxis is only referred to as having occurred spontaneously in twelve cases, and varied from a small amount to profuse severe hæmorrhage, and from a single bleeding to frequent repeated hæmorrhages.

(*b*) *In Operated Cases.*—Amongst the total seventy-one cases, operative treatment was only definitely stated to have been carried out in thirty-nine cases; so we may conclude that the remainder were not interfered with. Removal by forceps was employed in nineteen cases, the cold wire snare in five cases, the galvano-cautery in eight cases, thyrofissure in six. Suspension laryngoscopy and excision with scissors in one case, and radium in one case. Amongst these thirty-nine cases operated on, considerable or severe hæmorrhage occurred in twenty-five cases, and no hæmorrhage or only slight bleeding occurred in fourteen cases. In the nineteen cases in which the tumour was removed by forceps, severe bleeding occurred in seven cases, slight bleeding in four, and none in eight cases. In the five cases in which the tumour was removed by the cold snare, severe bleeding occurred in two cases, slight bleeding in two, and none in one case. In the seven cases in which the tumour was removed by the galvano-cautery snare, severe bleeding occurred in three cases, slight bleeding in two cases, and none in two cases. In the six cases of thyrofissure there was no bleeding, and also in the single case treated by radium.

In the entire series of seventy-one cases no death has been recorded from spontaneous hæmorrhage. Death has occurred in only three cases from hæmorrhage—e.g., in a case recorded by Ferrari,[1] following the removal of a piece for microscopical examination; in a case of Shurley's,[2] during a tracheotomy, through cutting into the growth; and in a case of Navratil's,[3] the cause of which is not stated. Chiari[4] in referring to this case says the growth was a myxo-angeioma, rich in vessels, and not a true angeioma. In a case reported by Norman Patterson[5], the patient was only saved from death following post-operative hæmorrhage, by an urgent tracheotomy and removal of a large blood-clot which had blocked the bronchi and lower portion of the trachea.

Treatment.

Non-Operative.

(1) *Sulpho-ricinate of Phenol.*—The employment of this preparation has proved successful in a case reported by T. K. Hamilton[6] and may be of service in some cases.

[1] *Lo Sperimentale*, Firenze, 1888, lxii, pp. 593-601.
[2] "Diseases of Nose and Throat," 1900, p. 570.
[3] *Internat. Centralbl. f. Laryngol.*, 1910, xxvi, p. 151.
[4] *Archiv f. Laryngol. u. Rhinol.*, 1896, v, p. 100.
[5] *Proc. Roy. Soc. Med.*, 1920 (Sect. Laryng.), xiii, p. 180.
[6] *Austral. Med. Gaz.*, 1899, April 20.

(2) *Application of Radium.*—The satisfactory results in a case recorded by Ryerson [1] should encourage further trial in similar cases.

Operative.

If the tumour is not causing any serious trouble it is best left alone, and in view of the opinion that the nasal obstruction may encourage the growth of the tumour, it is recommended that any obstruction or catarrh should be remedied. If slight hæmorrhage occurs it may be treated by the application of the galvano-cautery to the bleeding point.

ENDOLARYNGEAL REMOVAL.

The Indirect Method: By Forceps, Cold Snare, Galvano-cautery Puncture, Galvano-cautery Snare.

If the tumour causes serious symptoms, such as respiratory obstruction, or if repeated hæmorrhages occur, removal of the mass is indicated, and this may be accomplished by means of endolaryngeal forceps, the snare or galvano-cautery. Hirsch [2] remarks that if the tumour is very small, and has no pedicle, it cannot be removed by the snare or forceps, so the galvano-cautery affords the best means for destroying it. In view of the hæmorrhage which may occur Vitto-Massei [3] recommends that tracheotomy should precede removal, even by the galvano-cautery. On the other hand, Chiari advises that tracheotomy should only be performed if it is impossible to arrest any accompanying hæmorrhage by other methods.

As shown by the records, hæmorrhage is not usually troublesome in the case of smaller growths, and the excessive hæmorrhage which has occurred in a few cases after endolaryngeal removal has been easily arrested by endolaryngeal methods. In a case of severe post-operative bleeding reported by Biaggi [4] it was controlled by gelatine enemata.

Rille [5] (Innsbrück) recommends the following gelatine injections:—

Gelatine alba	3·0
Dissolved in distilled water	300·0
Sodium chloride	1·50

Sterilize for a quarter of an hour.

[1] *Journ. Canadian Med. Assoc.*, 1912, n.s., ii, p. 111.
[2] *Wien. klin. Wochenschr.*, 1908, xxi, pp. 592-594.
[3] *Bollett. d. melattie dell' Orecchio,*" &c., 1906, xxiv, pp. 109-114.
[4] *Internat. Centralbl. f. Laryngol.*, 1905, xxi, p. 464.
[5] *Wien. klin. Wochenschr.*, 1901, No. 47.

Still the fact remains that the danger of endolaryngeal removal by forceps, snare or galvano-cautery must be considered from the point of view of the risk of exposing the patient to hæmorrhage, difficult to arrest (in some cases) endolaryngeally, the possibility of severe laryngeal spasm, and of the entrance of blood into the bronchi. The risk is well shown by the cases of Ferrari and Norman Patterson previously mentioned.

The Direct Per-oral Method.

(*a*) *By Means of Laryngeal Tube Spatulæ.*—This method is to be recommended, since, if bleeding occurs, the endoscopic tube may be employed as an intubation tube, and an intratracheal suction tube attached to Kelly's apparatus passed down the endoscopic tube. By this means any blood which may enter the respiratory passages can be immediately removed and the necessity of an urgent tracheotomy avoided.

(*b*) *By Means of the Suspension Apparatus.*—Experience has shown the ease with which growths of the epiglottis and aryepiglottic folds may be removed by this procedure, and this method is admirably adapted for the removal of angeiomatous tumours, especially those situated in the aryepiglottic region. A preliminary laryngotomy or tracheotomy should always first be performed, in order to avoid the risk of an urgent operation when a hæmorrhage has occurred, and the life of the patient is in danger.

Robert Clyde Lynch[1] (New Orleans), during the discussion on a case reported by Emil Mayer,[1] suggested the use of the suspension apparatus in those cases in which it was possible to get underneath and around the place of attachment of these tumours, and especially if the surface is not too broad.

The case recorded by Norman Patterson and Pike in which the growth was removed by suspension laryngoscopy occurred after completion of this monograph, and it is interesting to note that consequent on their experience in this case they recommend preliminary tracheotomy. This opinion coincides with the conclusion arrived at by the writer after a careful study of all the recorded cases. It is remarkable, considering the great rarity of these tumours, that following an interval of fourteen years two cases should be brought to the notice of the Section of Laryngology, Royal Society of Medicine, within a year of each other.

[1] *Trans. Amer. Laryngol. Assoc.*, 1916, p. 190.

Though the results of endolaryngeal treatment have been satisfactory in many cases, it is as a rule much safer, if operative interference is indicated, to perform a thyrofissure.

Extralaryngeal Route.

Thyrofissure.

Since the occurrence of hæmorrhage in the removal of the growth is due to opening into the cavernous spaces, this can be avoided in the non-pedunculated forms affecting the ventricular bands or vocal cords by the performance of thyrofissure, with the advantage that a wide sweep can be made through normal tissue round the growth, as in the case of intrinsic malignant disease. In this way bleeding is practically *nil*, and any hæmorrhage which does occur can be easily controlled. Improvements during recent years in the technique of the operation have done away with the risk of opening the larynx.

Toubert,[1] Edmund Mayer,[2] Emil Mayer[3] and others advocate removal by thyrofissure on the grounds of the risk of hæmorrhage. Emil Mayer[3] has performed this operation under local anæsthesia in one case, in which a thorough excision of the tumour was carried out, with only slight bleeding, the cut surface of the mucous membrane being afterwards re-united by its edges with sutures, thus restoring the continuity of the mucous membrane.

Hamilton White[4] remarks that the satisfactory results of thyrofissure lead one to believe that this operation should be undertaken, and the more so since the risks of endolaryngeal operations have been shown to be so uncertain.

Richardson[5] has pointed out the importance of angeioma cavernosum being differentiated from lympho-angeioma, since the latter may be safely dealt with endolaryngeally, and does not require in any case thyrofissure.

[1] *Bull. et mém. Soc. Franç. d'Oto-Rhino-Laryngol.*, 1912, xxviii, p. 144.
[2] *Internat. Centralbl. f. Laryngol.*, 1904, xx, p. 416.
[3] *Trans. Amer. Laryngol. Assoc.*, 1916, p. 190.
[4] *Wien. klin. Wochenschr.*, 1908, xxi, p. 571.
[5] *Trans. Amer. Laryngol. Assoc.*, 1917, p. 194.

Tonsillectomy for Rheumatoid Arthritis.

By W. M. Mollison, M.Ch.

I present these observations to you with diffidence, because I feel that a subject like rheumatoid arthritis should be dealt with by a physician rather than a laryngologist. It is a disease of many symptoms and of uncertain pathology, or one may say of many pathologies. Few deny that infection plays a large part in the causation, but as to the origin of the infection there is diversity of opinion. Not long since it was a fortunate patient suffering from arthritis who did not lose every tooth in his head, however innocent they seemed to any save the skilled dentist; indeed it is quite unusual for the arthritic patient to be referred to the laryngologist before his teeth have been extracted. The colon has been cited as the cause *par excellence* and the heroic operation of colectomy has been advised (and I must add carried out successfully). Now it is the tonsil, and laryngologists must see to it that their judgment is not warped to the extent of advising removal of tonsils in the case of all arthritic patients.

The tonsil is recognized as the infecting source in many cases of rheumatism, and in conditions often associated with it—myocarditis, pericarditis, persistently rapid pulse, continued pyrexia—and removal of the tonsil is in a large percentage of cases successful in effecting a cure. It is natural perhaps to hope that rheumatoid arthritis or chronic infective arthritis should respond to removal of tonsils as does acute infective arthritis (rheumatism), and this hope is justified in a certain percentage of the cases. Recently a physician told me he considered that results obtained as a result of enucleation of tonsils were distinctly more encouraging than those following the removal of teeth.

Rheumatoid arthritis has several different clinical manifestations, but for the purposes of these notes I propose simplifying them to: Pains in the joints, creaking in the joints, and deformity of the joints; one must add also pains referred to different parts of the body called "neuritis" by the patient, really fibrositis and myositis. There is often combination of these symptoms; one joint or many may be involved.

The number of cases collected is small; they are private cases operated upon during the last four or five years and are consecutive; but I think they represent quite fairly the results obtained from a much larger number. The number of patients, thirty-six: those "cured,"

eleven (30·5 per cent.) ; improved or cured, twenty-five (69·4 per cent.) no improvement, five (13·9 per cent.).

In a recent American paper the percentage of cures given was eighty; it is however possible that the paper was founded on a series of cases in which a hopeful result was to be looked for.

The improvement in some cases was extraordinary and perhaps the best way of demonstrating the value of removal of the tonsils will be by quoting in detail some of the cases. It was noticed that, generally speaking, the sooner operation was performed after the beginning of symptoms the better the result. But on the other hand three or four cases which had persisted for seven or eight years were improved enormously. The ages of the patients varied from 22 to 64. Fourteen were over the age of 44. The eldest patient was 64 and curiously enough she was one of the most successful cases in the series. The cases that showed no improvement were those of old standing, namely, 13 and 16 years respectively (Cases Nos. 8 and 22), and those in which the X-rays showed marked changes in the bones about the joints. In these cases the tonsils were not obviously septic, and indeed I did not urge operation, but on previous experience told the patient that removal might lessen the pain. The tonsils were in some cases enlarged and the patients had frequently complained of sore throat and from the tonsil obvious septic material could be expressed. In others the tonsils were small and buried, and only with considerable difficulty could one exude any material at all. In all the tonsils examined a streptococcus was found.

Conclusions.

All cases should be examined by a physician for general investigation in order to exclude causes other than throat.

The tonsil is often the focus of infection in rheumatoid arthritis. Its removal is in some cases directly indicated.[1] In other cases the results of removal are problematical, but often in advanced cases removal may be followed by unexpected improvement. Removal may be followed by great diminution in the pain, and sometimes by disappearance of the peri-articular swellings.

The details of the cases are as follows:—

[1] When pressure on the tonsil causes septic material to exude from the crypts, or there is complaint of sore throats, accompanied by exacerbations of joint symptoms.

	Sex	Age	Main symptoms	Date of operation	Result
1	F.	23	Sore throats, pains in fingers, ankles, knees, and wrists	Oct., 1919	Six months later did not complain of joints, but local condition no better?
2	F.	37	Pains in various joints for two years, and antral suppuration	Nov., 1917 (and antrum)	Much better, though still some pain in the earliest affected joints; antrum still pustular
3	F.	50	Rheumatism in feet and right shoulder	July, 1918	1919, still pain in a toe; ? no better
4	F.	58	Right hip shortening; pain	May, 1920	Still pain; no improvement
5	F.	27	Pain eighteen months and evening rise of temperature	Sept., 1918	Recovered in three months; well
6	M.	23	Pain in interphalangeal joints for some months	1917	Well three days after operation
7	M.	34	Myalgia in lumbar region and abdomen	1917	Better after three or four weeks, but not cured
8	F.	44	Pains in hands, shoulders, knees; deformity of wrists; very old standing; worse since pain and empyema, 1913	1920	No better
9	M.	44	Pains in feet, elbows, loins for one to two years	June, 1918	Better soon after operation; well
10	M.	56	Pains in most joints for seven to eight years; deformity of wrists; albuminuria	1918	Better soon after operation; albumin gone; still no pain, but deformity remains
11	F.	44	Pains in hands and feet for six to seven years	June, 1919	(?)
12	F.	64	Pain in left wrist for six months, slight deformity	Dec., 1919	Well very soon, felt ten years younger; deformity disappeared
13	F.	50	Pain and some swelling in various joints for two years	1913	No better (?)
14	M.	38	Joint pains in left knee	April, 1920	Much better two or three weeks after operation; has relapsed somewhat
15	M.	37	Creaking of many joints (had been advised colectomy)	April, 1920	Distinctly better
16	F.	46	Rheumatism for years	Oct., 1919	Was improving three months after operation
17	F.	50	Pain began in shoulders, now in all joints, one year	Jan., 1920	Definitely better, though still pains at times
18	F.	38	For about a year stiff shoulders and knee; stiff neck; right elbow, ? tennis elbow	Feb. 1920	Free from rheumatism, no stiff neck; after playing in tennis tournament has pain in arm
19	M.	41	Rheumatism for years	1913	(?)
20	F.	27	Rheumatism in various joints; is a massage student, but unable to continue	March, 1920	Rapid improvement; returned to her work
21	F.	24	Stiffness in arms, legs, and feet for one year	1918	1920, better
22	F.	60	Sixteen years rheumatism in various joints; a good deal of pain	April, 1920	No improvement
23	F.	34	Pains in various joints	May, 1918	Definitely improved
24	M.	36	Pain and stiffness in spine	March, 1920, and plaster jacket applied	Much improved, but (?) due to jacket

	Sex	Age	Main symptoms	Date of operation	Result
25	M.	42	Pains in limbs eighteen months, had to give up work as a dentist; ? fibrositis	April, 1920	Pains all gone; back at work
26	F.	52	Arthritis three years; improved, but remains in her hands; deaf	Aug., 1918	September, 1918, hands "woke up" after operation, but more comfortable; later, completely cured, also deformity cured
27	F.	35	Pain in interphalangeal joints; periarticular swelling; pain in feet also; unable to continue duties as a nurse	July, 1917	Rapid improvement; went back to work
28	F.	36	Pain in all joints about a year	1918	Did not improve
29	F.	16	Pain in all joints about a year	1915	Only slight improvement
30	F.	26	Pain, swelling and stiffness in many joints for a few months, followed tonsillitis	Feb., 1920	For a week after operation more pain in knees, but now perfectly free from all symptoms; feels better than she has for a year
31	M.	37	Five years' history of rheumatism	April, 1919	Letter from physician, May, 1920, certainly better than in 1917; put on 5 lb. and general health better, but pain in right wrist again for three to four weeks; "joint infection certainly more quiescent," though not gone
32	M.	36	Two years' history of arthritis	Feb., 1920	Very much better; pain in joints less and less frequent; very glad he had tonsils removed
33	M.	28	For seven months attacks of rise of temperature and aches all over	Feb., 1920	Soon after general symptoms gone, locally still discomfort; June, no feverish attacks; heart less jumpy; pains, no improvement
34	F.	25	Fibrositis, shoulders and neck	Dec., 1917	Improved; died in February, 1919, influenza and pneumonia
35	M.	40	Rheumatoid arthritis for two years or more; stiffness and getting worse; had had tonsils enucleated, but a very small nodule remained in one bed; unable to get about with any comfort; previous vaccine treatment	March, 1915, to remove nodule	July, 1915, no rheumatism since; working ten hours a day
36	F.	49	Six years' history; various joints	May, 1920	(?)

DISCUSSION.

Mr. H. LAWSON WHALE: I have lately seen a case of unsuspected sepsis in the supratonsillar fossa, resulting in death two days after I had seen the patient, and only one week after the onset of the illness. A swab which was taken in life yielded a pure culture of a long-chained streptococcus.

Mr. E. M. WOODMAN: A chronic septic absorption from the tonsils is, in my experience, far more common than is generally supposed. In many cases

the tonsils lie entirely hidden from view, but if a curved probe be used to lift aside the anterior pillar of the fauces, and pressure be made on the upper pole of the tonsil in a downward direction, a free flow of pus can often be obtained. In one case I have frequently examined the throat of a lady suffering from chronic bacilluria at varying intervals during the last four years in order to discover the source of absorption. By this method of examination free pus was found to flow from the atrophic scarred right tonsil. An operation was performed and the patient is better in health than she has been for several years.

Dr. IRWIN MOORE: Mr. Mollison's paper is one of the most valuable contributions to the Congress, for it is a record of personal experience and constitutes a careful statistical study of the relationship between the unhealthy tonsil and rheumatoid arthritis. Insufficient attention has been given to the relationship in this country, and we are now only just beginning to realize its importance and the necessity of getting rid of septic foci of infection in the tonsils by complete enucleation. Great advances have been made in America in the study of systemic diseases and their association with the tonsils. French, of Brooklyn, has contributed valuable research work in connexion with the exploration of the tonsil *in situ*, by devising a method of transillumination by means of an illuminating tube or tonsilloscope. He has shown that septic foci in the tonsillar crypts may be localized, so that the healthy and unhealthy tonsil may be differentiated.

The following Demonstrations were given during the Congress:—

(1) Dr. LLEWELYN POWELL, Dr. FELIX ROOD, Dr. F. E. SHIPWAY, and Dr. H. E. G. BOYLE: General Anæsthesia in Nose and Throat Operations. Demonstrations of Technique.

(2) Dr. E. P. CUMBERBATCH: Diathermic Cauterization in the Treatment of Inoperable Growths of the Mouth, Tongue, Pharynx, &c. Demonstration of Technique.

(3) Mr. HAYWARD PINCH (Radium Institute): Radium Treatment of Inoperable Growths of the Mouth, Pharynx, and Œsophagus. Demonstration of Technique.

(4) Mr. LAYTON: The Technique of Tonsillectomy under Local Anæsthesia.

(5) Mr. NORMAN PATTERSON: A Modified Suspension Laryngoscopy Apparatus.

(6) Dr. IRWIN MOORE: A New Illuminating Apparatus and Endoscopic Tubes for the Direct Examination of the Œsophagus and Respiratory Passages. Designed for the use of either Proximal or Distal Lighting.

A representative exhibition of specimens, drawings and photographs, in connexion with Rhino-Laryngology, collected from the museums of London hospitals and private exhibitors, was held in the West Hall.

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Section of Medicine.

President—Dr. A. F. Voelcker.

Anæmias of the Hæmolytic Jaundice Group.[1]

By Gordon Ward, M.D.

The object of this paper is to propound the thesis that there is a group of anæmias or of morbid states without anæmia characterized by:—

(1) A tendency to familial, hereditary and/or congenital incidence

(2) But occurring also in the form of sporadic, sometimes frankly infective, cases

(3) With an intermittent or paroxysmal course

(4) And changes in the red cells which are, in most cases, peculiar to these diseases: but which

(5) Themselves depend on prior changes in the plasma

(6) And are of a nature to render the red cells more prone to destruction, either by the normal mechanism of portal hæmolysis or by phagocytosis or by both.

With the exception of hæmolytic jaundice these diseases are of extremely rare occurrence, and are almost unknown in British literature. Their importance is therefore largely theoretical, but nevertheless discussion of them tends to throw considerable light on the pathogenesis of these and other forms of hæmolytic anæmia, and even on the associated questions of therapeutics. The diseases in question are:—

(I) Hæmolytic jaundice or acholuric jaundice or cholæmia.

(II) Dresbach's syndrome with elliptical red cells.

(III) Herrick's anæmia with sickle-shaped red cells.

[1] At a meeting of the Section, held October 28, 1919.

(IV) Malins' syndrome or phagocytic anæmia or auto-erythrophagocytosis.

(V) Clough and Richter's syndrome with auto-agglutination of red cells.

It is proposed to describe these diseases with particular reference to their fulfilment of the above postulates, and to follow this with some comments on the group as a whole in its relation to other groups of hæmolytic anæmia — e.g., the so-called pernicious anæmia and paroxysmal hæmoglobinuria.

(I) Hæmolytic Jaundice.

This disease is now very well known to the clinician, but there remain several undecided points in connexion with its pathology. On the one hand several authors scarcely distinguish it, at least in its more severe forms, from Addisonian anæmia, while on the other we have those who seek its affinities among the various forms of hepatic cirrhosis with splenomegaly. Without discussing these different theories the writer would state his own idea of its pathology, for which he claims no originality. It is as follows:—

The primary defect appears to be, to use Garrod's apt phrase, an "inborn error of metabolism." From this there results a morbid condition of the blood-plasma. Some prefer to think of this as a physical and molecular change, others regard it as chemical and partaking of the nature of toxin production. The writer inclines to the former view, but at present it will be better to speak as if an actual toxin, some new and injurious body, were produced in the plasma.

This toxin is in its action strictly comparable to many well known chemical blood poisons—e.g., phenylhydrazine, toluylenediamine, &c., very rarely it so injures the red cells that they actually lose their hæmoglobin in the peripheral circulation. There is then hæmoglobinæmia [1], and if this reaches a sufficient concentration the kidney threshold for hæmoglobin is passed and hæmoglobinuria results [1].

In the great majority of cases the injury to the red cells is less than this, and without actually destroying them the toxin renders them more easily destroyed by normal portal (splenic) hæmolysis. Normal hæmolysis depends on the activity of the spleen which separates the hæmoglobin from the red cells and retains the stroma. The hæmoglobin is passed by the circulation to the liver and there elaborated into bile.

There is not in acholuric jaundice any increase in splenic function but, in virtue of the already injured red cells, the normal function suffices to produce much greater blood destruction. There is then a strain on the bile-producing functions of the liver, the bile becomes thickened and viscid, and is re-absorbed into the circulation, and so jaundice results—i.e., bile pigments are found in the blood. Very often they are absent from the urine, but this, again, is a secondary matter, and depends on the kidney threshold for bile pigments. If this be passed bile will appear in the urine [3]. It is probable that the threshold is usually raised in this disease, just as the sugar threshold may be raised in diabetes and there may be hyperglycæmia without glycuresis.

Although that function of the spleen which is concerned with hæmolysis of red cells (the "normal hæmolytic function of the spleen") is not exalted, it has also certain other functions which need notice. There is first function of dealing with the stroma of dead cells. This is naturally increased, there is a true work hypertrophy. Hence the spleen enlarges. It is not quite correct to regard this as purely spodogenous; there is more entailed than mere storage of waste products—viz., the increased production of the phagocytic cells which have the duty of disposing of these products and the increased vascularity associated with it.

Moreover, an injured red cell is, *ipso facto*, likely to be a ready prey of phagocytes, and this before it has a chance to be hæmolysed—i.e., before it reaches the portal circulation. It is devoured whole. This constitutes the phagocytic function of the spleen; it is not peculiar to the spleen but is apt to be more marked in this organ because of its relatively large content of endothelial cells, which are the cells employed as phagocytes. This function is hardly in evidence in health, but it is probable that any red cell that manages to live out its life without meeting with the baneful activity of the spleen is finally disposed of in this way [4]. In one of the anæmias of this group the blood destruction is almost entirely phagocytic—viz., in Malins' syndrome.

But the phagocytic factor has no great influence in the production of anæmia in acholuric jaundice, the anæmia is predominantly hæmolytic.

Were it not for the fact that the anæmia is predominantly hæmolytic—(i.e., splenic), removal of the spleen would hardly have the good results which are usual. On removal of the spleen its hæmolytic functions would appear to be exercised vicariously by the gastro-

intestinal lymphatic system [4]. The mechanism by which this substitution is brought about cannot at present be explained, either in this or in parallel instances. One supposes, however, that it depends in some way on the necessity for a normal rate and amount of bile production. If this is so, we should not expect this vicarious hæmolytic function to reach even the normal for a healthy person, because in acholuric jaundice a normal amount of hæmolysis is more easily produced than in other diseases or in a state of health, owing to the defective red cells being little able to resist hæmolysis.

We do not, in fact, find after splenectomy any more than the normal rate of bile production.

There is another difficulty to face. Why does the fragility of the red cells sometimes remain [5] and sometimes disappear [6] when the spleen is removed? Here also an explanation can be offered which is purely theoretical and may or may not correspond to facts. It is known that the immersion of red cells in various fluids [38] and changes in the blood plasma which normally carries them may produce differences in their resistance to saline hæmolysis. It is at least possible that this difference may be more readily produced on the young and immature cell—such a cell as exhibits polychromasia. In acholuric jaundice these polychromasic cells are very numerous, and it is probable that in some cases these and these alone are sufficiently affected to render them liable to saline hæmolysis in higher concentrations than would affect a normal cell. In such cases splenectomy would abolish the altered fragility simply by doing away with the output of young and polychromasic cells — an entirely indirect action brought about by cessation of morbid hæmolysis, and so of excessive regeneration. In other cases in which even the normal red cells received that impression from the toxic plasma which we estimate by saline hæmolysis, the removal of the spleen could not be expected to have any effect of this kind.

The foregoing sketch of the pathology of acholuric jaundice is very largely hypothetical and, in the present state of our knowledge, nothing else is possible.

The disease is sufficiently well known to make it unnecessary to detail clinical features other than those which must be mentioned in estimating the extent to which this disease fulfils the postulates required for inclusion in the group of which, in the writer's opinion, it is the most frequently encountered example. These can now be considered in order.

Familial, hereditary and congenital cases have been very frequently recorded and require no further reference.

Sporadic cases occur both congenitally and as the result of acquired infections—e.g., syphilis [7], tuberculosis [8], &c. The latter are sometimes regarded as falling in rather a distinct class, and are more pernicious in their clinical manifestations. It is not disputed however that the congenital and acquired forms are far more nearly related to each other than either of them is to any other disease.

An intermittent or paroxysmal course is often seen. Paroxysms commonly take the form of a sudden increase of jaundice accompanied with gastric or other secondary symptoms. In Wilson's cases these could be excited by cold baths [9], and exposure has been noted as a cause of exacerbation by other writers [10].

The chief change in the red cells capable of detection is an increased fragility, a liability to hæmolysis in concentrations of salt that prevent lysis in normal cells. This change is apparently peculiar to the disease. Other changes—e.g., the large proportion of reticulated cells and of cells with basophil granulation—occur most commonly perhaps in this disease, but are found also in many other conditions.

That these changes depend on a plasma change is less easy to demonstrate. It has been supposed that there is an inherent defect in the bone marrow, but the results of splenectomy have gone far to refute this view. Moreover it is impossible to explain the acquired cases on these grounds. Nevertheless few authors have claimed to have actually demonstrated any change in the plasma. Dudgeon [11] has demonstrated auto-agglutinins, but his results have not been verified by others. It is possible that he was dealing with a family with the double defect, for familial agglutination (which will be referred to later) has been described as the sole ascertained blood abnormality in families. Indeed it falls within the group of diseases now under discussion. Dudgeon's observation is of great interest. More information is needed on this point, and at present we can only say that we arrive at a primary plasma defect by a process of exclusion—no other explanation will fit the known facts.

That the red cells are rendered more liable to hæmolysis is attested by the increased hæmolysis characteristic of the disease. The evidence of increased phagocytosis is contained in the spleen, where are found masses of pigment [12] such as we know to be produced by the destruction of whole red cells. On this point the researches of Hunter [4] are of the greatest value, and have never been disputed, although they have had too little attention from later workers.

We have now shown that acholuric jaundice fulfils all the postulates for this group except that the existence of a primary plasma defect is not directly demonstrated. The indirect evidence amounts almost to certainty. In certain other diseases of this group a plasma defect can be unequivocally proved, and it is largely because of the importance of this and other analogies that the group is here presented as a whole.

(II) Dresbach's Syndrome.

In 1904 Dresbach described a peculiar case in which the red cells instead of being oval were elliptical in shape. "In many of them the biconcavity was scarcely perceptible," but they were otherwise normal. The subject was a healthy mulatto student who discovered the changes during the routine examination of his blood in a physiology class. The case excited some comment, and this is summarized in a second paper by Dresbach. By the kindness of Dr. Dresbach I have copies of both these papers [14], [13], and am also allowed to reproduce a photograph of the cells. The average length was 10·3 μ and the average width 4·1 μ. The patient's brother had normal cells. No other member of the family was examined. Ewald, of Strassburg, informed Dresbach that he believed a similar observation was made at Königsberg twenty or thirty years before. There would appear to be no record of this case.

Dresbach's patient was in excellent health. He died not long after of acute rheumatism with heart failure—which can hardly have had any connexion with his red cells.

Bishop in 1914 [15] recorded a similar case which came to light during a routine blood examination in a case of appendicitis. In this case a sister was found to be similarly affected, but her father and her two nieces were normal. Both patients had excellent health. The fragility of the red cells was normal.

There are thus only three recorded cases of persons in good health but with elliptical red cells. No experiments seem to have been made on these bloods except such as were necessary to exclude artefact. This was done most thoroughly. The disease, if so it may be called, thus fulfils only a few of our postulates.

There is evidence of familial incidence in Bishop's two cases, while Dresbach's case was sporadic as far as is known, but only one other member of the family could be examined.

There is no suggestion of anæmia or of a paroxysmal course.

The red cell changes are certainly peculiar to the disease.

A primary plasma defect is certainly not in evidence, but we may note two interesting analogies. First, oval cells are very frequent in most severe anæmias. Cabot [16] published a plate from a case of "pernicious

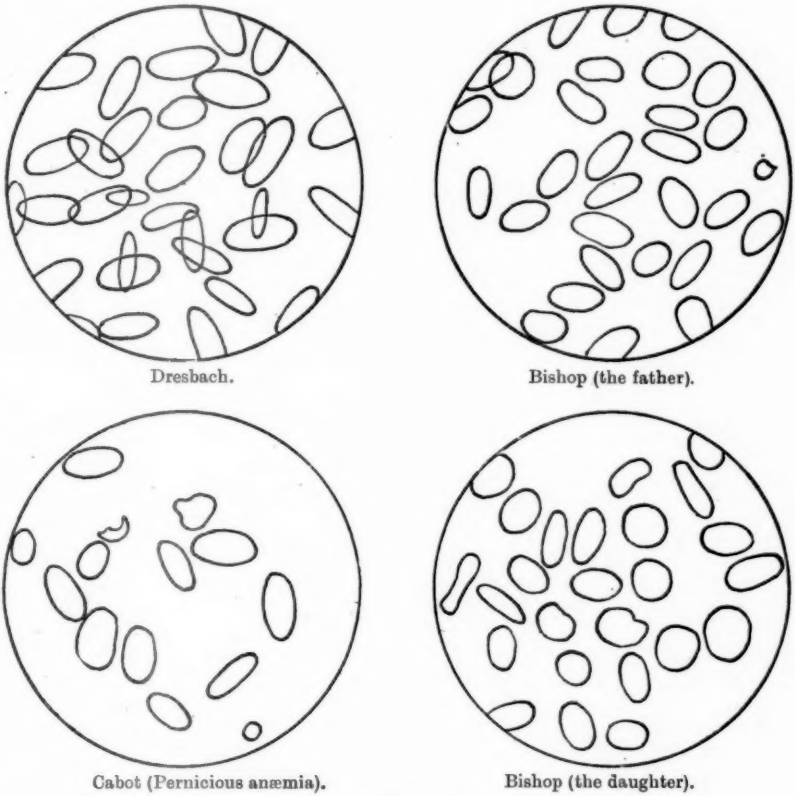


FIG. 1.

Camera lucida drawings from published photographs in the cases mentioned. Dr. Melvin Dresbach has very kindly sent the writer a photograph from which the drawing in his case is made. The elliptical shape of the cells is well shown, and also the variations in size.

anæmia," and this is a good example of what may almost be described as a sporadic and frankly infective case. Secondly, in Herrick's anæmia the peculiar sickle-shaped cells are accompanied by a proportion of oval cells, and these changes are definitely proved by Emmel to depend on

plasma defect [17]. There are thus grounds for supposing that there is a plasma defect. It is difficult to understand how otherwise one could explain the condition, for the marrow could hardly turn out normal and abnormal cells at the same time. We have already seen how a plasma defect might affect some cells and not others, according to their age, &c.

There is no evidence that these oval red cells are especially liable to hæmolysis. The patients were in good health, but so also are some of the victims of acholuric jaundice, especially during intervals when there is perhaps little or no jaundice.

Dresbach's syndrome is recognized by Cooke and Meyer as being allied to Herrick's anæmia, and should more cases be forthcoming it is to be hoped that we shall have more extensive evidence as to the effect of the plasma on normal and pathological cells, and on the presence or absence of hæmolysis or phagocytosis.

(III) Herrick's Anæmia.

In 1910 Herrick [18] described a case of "peculiar elongated and sickle-shaped red blood cells in a case of severe anæmia." Similar cases were reported by Washburn [19] in 1911, and Cooke and Meyer [21] in 1915. Emmel [17] [20] has made further studies of the last case. Only in Herrick's case is there any adequate clinical description.

The postulates for this group are fulfilled to the following extent :—

In Herrick's case no family history of illness was present, the patient was one of four children and the other three had good health. No family history is given in Washburn's case (access to the original is not possible, but Cooke and Meyer give a good résumé). In Cooke's case two brothers and a sister had died of anæmia at an early age, and the father's cells showed the sickle-shaped cells when kept for a few hours in culture [17]. There is thus definite evidence of familial incidence and a great probability of congenital development. Herrick's case appears to have been sporadic, and was possibly connected with an antecedent attack of yaws.

Postulate three requires a paroxysmal course. This was very evident in Herrick's case, and the paroxysms were like those of acholuric jaundice—i.e., accompanied by jaundice. In Cooke's case the abnormal red cells entirely disappeared for a time but returned later. The changes in the red cells are evidently peculiar to the disease. Camera lucida drawings from the published illustrations are given herewith.

Although considerably elongated cells are not uncommon in many severe anæmias, it will probably be agreed that these sickle-shaped forms do not occur except in Herrick's anæmia.

The question of prior plasma defect is settled by Emmel's researches. He showed that the patient's plasma could induce similar changes in a

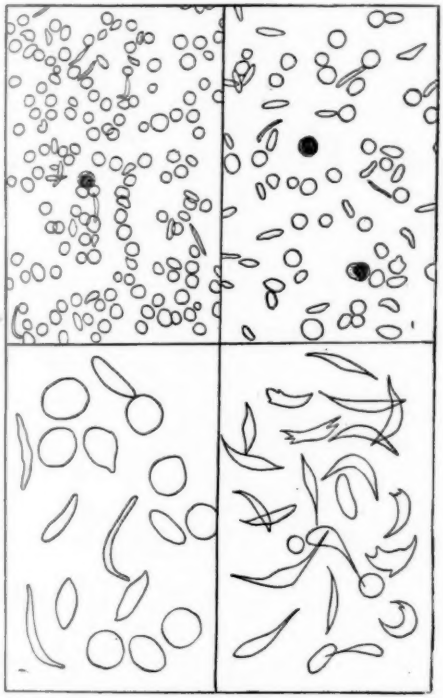


FIG. 2.

Camera lucida drawings from published illustrations. On the left, actual field from Herrick's case, and below, some individual cells on a larger scale. On the right, Cooke and Meyer's case, and below, some of Emmel's culture forms from the same case. Note variations in size as well as in shape.

pathological blood, that the changes were more pronounced in her own blood when cultured[1] and could be produced in the father's blood by the same means. The father had a slight anæmia.

Abnormal hæmolysis is shown by the recurring attacks of jaundice

[1] Cultures consisted in hanging drop preparations kept at body heat for varying periods of time, evaporation being guarded against.

in Herrick's case and by the presence of nucleated red cells and a high colour index. It is noteworthy that all cases had some degree of eosinophilia. Careful and repeated searches revealed no parasites or other cause for this.

Abnormal phagocytosis was shown in Cooke's case by the occasional finding in the blood of a phagocytic mononuclear containing red cells. In no case was the spleen enlarged, and this suggests that phagocytosis can hardly have a prominent place in the disease.

The disease thus fulfils all the postulates for this group and is seen to bear a close resemblance to acholuric jaundice. An additional point of resemblance is provided by the fact that all these patients (except the father of Cooke's case) had more or less intractable leg ulcers. This has been noted in acholuric jaundice by more than one author [22], and by the writer in an unpublished case. Herrick alone mentions actual jaundice, but Cooke and Washburn mention a greenish tinge of the conjunctiva and Washburn's case had been operated upon for gall-stones. All the patients were negroes or mulattos, so that a degree of yellowness which would at once attract attention in a white man, would here readily escape notice. No case was fatal, although in one case other members of the family had died of anæmia, a statement which is vouched for by the physician who attended the patient.

(IV) Malins' Syndrome.

Of this there are three cases on record. The first is that of Malins, published in 1894 [23], the second that of Bartlett [24], and Mary Rowley [25], and the third that of Van Nuys [26].

The characteristic feature is anæmia accompanied by splenomegaly and the presence in the blood of large number of active phagocytes, both mononuclear and polynuclear.

Malins' case was possibly one of the syndrome *per se*. In the other cases it is possible that the syndrome was merely superadded to pre-existing disease—viz., heart failure, trichocephalus and tænia infection and possibly leukæmia in Van Nuys' case, and aortic disease with heart failure in that of Bartlett and Rowley.

Malins' case was that of a woman whose first complaint was of an increasing fullness of her left side. After some months she was suddenly attacked by acute pain, and a spleen with rotated pedicle was removed. Only once, and that some months after splenectomy, did she show all

the characteristic blood changes. They are recorded as follows: "The most remarkable point was the presence in the blood of fairly considerable numbers of large white nucleated corpuscles, varying in diameter from 30 μ to 50 μ, in which were contained several red cells, in some twenty or more. In individual ones the red corpuscles, appeared to be undergoing a process of disintegration, while small masses of blood pigment could be observed in those where the change was most marked."

The examination of the spleen is recorded as follows, and is the only evidence we have of the sort in this disease: "The spleen, which weighed 2 lb. 5 oz., was much enlarged, firm, and of a dark colour. The increase in its size was general, so that there was not much alteration in shape, but the notches were very indistinct. The capsule was much thickened. There was a well marked twist of the vessels, &c., forming the pedicle; it was found that complete thrombosis had occurred so as to make them impervious. Microscopic examination of the spleen showed great engorgement with red corpuscles, the sinuses and meshes of the reticulum being everywhere crowded with them. The fibrous framework and capsule were much thickened. No abnormal elements could be detected."

Five months after operation the white cells were still increasing in number and had reached 50,000. Differential counts are not given. The patient said that she "felt much better without her spleen than with it." The red cells had fallen to 3,300,000 from a normal figure three months after operation, so that what disease she had was evidently progressive.

Bartlett's case was peculiar in that the phagocytes were only found in blood from the lobes of the ears, which were cyanosed. The number of white cells was as high as 262,000. The liver was enlarged but not the spleen. This same case came later under the care of Rowley, who has described it at length. There was then great ascites; neither liver nor spleen could for this reason be palpated. There was a dragging sensation and pain in the splenic area. The patient lived only one month after coming under observation, and died of heart failure. No autopsy was obtained. There was progressive anæmia while the white cells increased from 43,000 to 700,000. The phagocytosis was not confined to mononuclear endothelial cells but affected all varieties of white cells. Although cell counts varied very much in different parts of the body, phagocytes were not confined to the lobes of the ears as when the case was under Bartlett's care. The blood of this patient mixed with normal blood imparted its

phagocytic powers[1] to the normal white cells, and when introduced into a guinea-pig caused two-thirds of its white cells to behave similarly. However, the pig recovered.

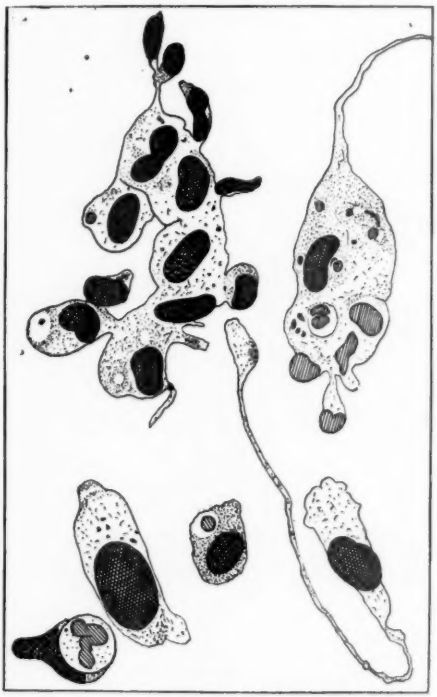


FIG. 3.

Camera lucida drawings from illustrations to Van Nuys' paper. Partly diagrammatic. Deeply-shaded nuclei are distinguished thus from ingested nuclei. Note the large plaque of endothelial cells and the long "streamer" on one cell.

Van Nuys' patient had massive splenomegaly, hepatomegaly, chronic bronchitis and aortic regurgitation, besides other ills. His phagocytes were similar to those seen in the other cases, but seemed to have a preference for ingesting leucocytes rather than red cells. The

[1] This is Rowley's description of the phenomenon observed. The writer would express it rather differently—namely, that the patient's serum so altered the red cells of a healthy person that they were devoured by his leucocytes—i.e., they were opsonized. There is no evidence in the experiment that the healthy leucocytes had any higher phagocytic power than is possessed by normal leucocytes.

leucocytes reached as high as 100,000, and Van Nuys remarks that the case might be considered one of leukæmia. During an attack of erysipelas the abnormal cells almost disappeared, but began to recur before death. The case is not fully reported.

We have in these cases [1] a very remarkable syndrome characterized by splenomegaly, a progressive increase in leucocytes and fall in red cells, and well marked intravascular phagocytosis of red and white cells.

With regard to our postulates we cannot say that it is ever hereditary or familial. We can say that it may occur in infective conditions on a lesser scale, for auto-erythro-phagocytosis is so well known a feature of various parasitic diseases [27] [28] that it has even been regarded as a pathognomonic [29]. The changes in the red cells can only be inferred from the fact that they are so readily seized on by phagocytes, as will occasionally happen when non-homologous blood, or blood which has been kept too long outside the body, is used in transfusion. The classical example of this is that recorded by Hopkins [30], and the writer has met with a similar example but not so marked.

That the defect is in the plasma and not the red cell primarily is shown by Rowley's experiment on the guinea-pig.

The existence of increased portal hæmolysis is not evidenced in these reports, and, indeed, was not to be expected, for there is every reason to suppose that the hæmolysis was purely phagocytic.

We may sum up our little knowledge of the disease as follows:—

Of the early stages little is known. Malins alone mentions that the spleen had been enlarged some months before it was removed. His microscopical examination suggests a considerable duration. At this time the blood changes were practically *nil*. It was only some time after the removal of the spleen that increasing leucocytosis and phagocytosis appeared, and the patient seems to have been lost to view after her white cells reached 50,000 and her red had fallen to 3,000,000. It is to this stage of the disease that the other two examples seem to belong, and both were fatal soon after they came under observation. The leucocytes mounted rapidly, so that the appearance of leukæmia was produced. This may have been purely reactionary, for very large numbers of white cells (especially in Van Nuys' case) were being destroyed. In the absence of autopsy evidence the point cannot be settled.

[1] The cases of Buchan and Comrie [39] suggest a transition between acholuric jaundice and Malins' syndrome, but are perhaps better grouped with the former. Circulating phagocytes were present in two cases, but all five cases were anomalous and difficult to classify precisely.

One might suppose that the phagocytosis should be interpreted as a sort of acquired viciousness on the part of the phagocytes. There is a tendency observable in the papers quoted to take this view, although Rowley expressly says, in summing up, that the whole phenomena might be explained by supposing that there was present an auto-hæmopsonin. This is my own view, although I am little enamoured of the particular term employed, convenient though it is.

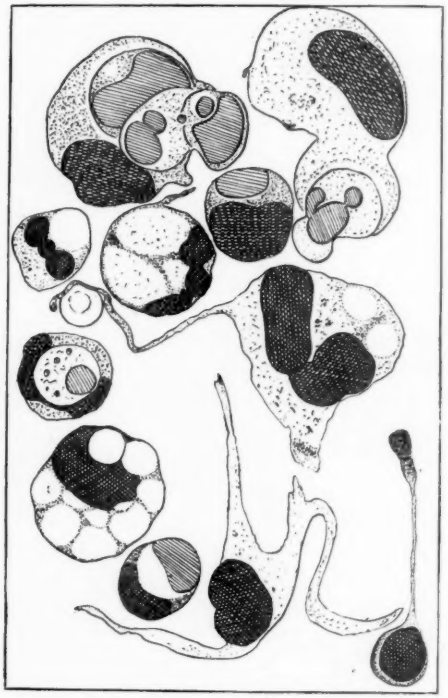


FIG. 4.

Camera lucida drawings from illustrations of phagocytes in Mary Rowley's paper. Partly diagrammatic.

The plasma change producing this effect was not constantly nor everywhere in evidence. At one time there was no phagocytosis except in the lobes of the ear; this was in Rowley's case while under Bartlett's observation. In Van Nuys' case phagocytosis was reduced to a minimum during an attack of erysipelas. In Malins' case phagocytosis was only

once noticed in the peripheral blood—it may well have been present elsewhere. There is no evidence as to what may have produced the plasma change. In Van Nuys' case the red cells were very little devoured, so that the plasma defect was probably slightly different. Rowley says that she has occasionally noticed a few such phagocytic cells in ordinary cases of leukæmia. This could hardly be assumed to be due to extra phagocytic activity of the leukæmic cells—e.g,, myeloblasts, &c., for these are known to be functionally defective. I do not suppose that any of these three cases were allied to primary leukæmia, a view which is rather borne out by the guinea-pig experiment.

(V) Familial Auto-Agglutination.

This has been described by Clough and Richter [31] and is included in this group because it presents close analogies with those conditions already described.

In a mother and daughter these authors found that the blood contained auto-agglutinins, active only at comparatively low temperatures and agglutinating also the red cells of various animals and of other persons of the same blood group, in this case Group III of Moss. The serum contained also a great variety of other bodies, some of which could be separated by absorption. No anæmia nor illness resulted from this abnormality. The condition was first discovered by the observation that the red cells of the mother clumped in a blood counting pipette, she being under treatment for pneumonia.

This inconvenient occurrence will not be strange to anyone who is called upon to do many blood examinations, especially if protozoal diseases come much under his notice. It is well known in trypanosomiasis [32] and in this disease is apt to be associated with phagocytosis.

Hektoen [40], writing in 1906, remarks that "phagocytosis without agglutination may occur, but that is rare; agglutination without phagocytosis is more common; agglutination and phagocytosis most common if the serum has any action whatever upon the red corpuscles used."[1]

[1] Meek (*Lancet*, 1910, ii, p. 1267) is more concise and definite on the relationship between agglutination and phagocytosis. He says: "From the examination of a considerable number of sera it may be stated briefly that where any given serum caused agglutination of any given sample of washed red corpuscles, when this unheated serum was mixed with these corpuscles in the presence of a suspension of endothelial cells and incubated, the red cells were invariably ingested by the phagocytes. Conversely, in no case where a serum failed to agglutinate certain red corpuscles was it found capable of causing them to undergo phagocytosis."

That the remarks of Hektoen are accurate seems to have been the experience of all who have devoted attention to the subject. Herein is one reason for allying auto-agglutination to the other members of a group which already includes varying degrees of auto-phagocytosis. Another is to be found in the records of Dudgeon [11] and Le Gendre [37], both of whom have recorded auto-agglutination in cases of acholuric jaundice.

Of the postulates for this group, auto-agglutination fulfils the requirements that it shall be familial and also sporadic and associated with definite infections. That it is intermittent or paroxysmal remains to be proved but it may be noted that in Clough and Richter's case it was less marked when the pneumonia had long subsided. The changes in the red cells are specific and are certainly due to serum changes. They probably render the cells more liable to phagocytosis and possibly also to hæmolysis, but this also is not definitely ascertained.

One may hope that at no distant date the phenomenon of auto-agglutination will be co-ordinated with whatever pathological and clinical conditions it may produce.

The Value of Grouping these Diseases together.

Any one of these diseases taken alone may well seem peculiar and difficult to elucidate, but if it be compared with other diseases in the group, certain points that were previously obscure are much illuminated.

Thus an inherent and primary plasma defect has not been proved for hæmolytic jaundice or Dresbach's syndrome, but the demonstration by Emmel that even more striking changes in the red cells may be produced by defective plasma goes far to suggest that the same sort of thing occurs in the former diseases.

Now many, perhaps all, congenital and hereditary diseases may be said to depend on some plasma defect. The merit of this grouping is that it brings together for the first time those in which the defect has a selective action on the red cells. Starting from this point we at once find that the diseases have other points in common. This again further justifies our grouping. Thus three of them are associated with a severe or fatal anæmia. Moreover these three form a natural scale, at one end an almost purely hæmolytic anæmia, at the other Malins' syndrome which is an almost purely phagocytic anæmia; between them we have Herrick's anæmia in which both factors participate. Further,

all these diseases are seen to exist in a less developed form in various infections and relative common conditions. In other words they exhibit both primary and secondary varieties.

It is not impossible that other members of this group may be discovered. One might find globular red cells or red cells showing any of the effects which may be produced by immersing them in different liquids.

There is also something to be said from the therapeutic side. So long as one regarded the abnormalities as due to congenital defect of the bone marrow, therapy was bound to seem hopeless. But when primary plasma defects are envisaged more hope appears. We are learning to modify the plasma in desired directions. Paroxysmal hæmoglobinuria is due to a plasma defect and this can be remedied by anti-anaphylactic treatment [34]. We might be able to immunize the body against whatever plasma defect is present in hæmolytic jaundice or to prevent the action of the "opsonins" in such cases as that of Rowley. The guinea-pig injected was able to do so. It is true that we are hardly at the commencement of this sort of therapy, but no harm is done if we begin to group together the diseases in which it will be most appropriate.

Splenectomy has also to be considered. We have attempted an explanation of the rationale of splenectomy in hæmolytic jaundice. The theory put forward that the enlargement of the spleen in that disease is spodogenous (i.e., due to accumulation of waste products) in part, but in still greater part phagocytic, may be applied to other members of the group. In Dresbach's syndrome there is no hæmolysis of any sort beyond the normal, and splenomegaly is accordingly absent. In Malins' disease splenomegaly may be massive and spleen puncture has shown the presence of phagocytes and also giant multi-nucleated cells which were possibly precursors of the former. We may assume that the enlargement is here also to be attributed to phagocytic activity. In Rowley's case the spleen was little if at all enlarged but it is interesting to note that here the peripheral endothelial cells were evidently taking a great part in the disease, for at one time only the lobes of the ears showed phagocytes in the blood. In Herrick's disease the spleen was not found enlarged but there was considerable portal hæmolysis. It might therefore be well to remove the spleen in such cases, as it would be in cases of hæmolytic jaundice without splenomegaly, in order to diminish portal hæmolysis. But since one case, like various cases of hæmolytic jaundice, showed circulating phagocytes,

we should do well to bear in mind that a cure would not necessarily result, although we might be certain to produce some alleviation.

The Relation of this to other Groups of Anæmia.

By bringing these diseases together we are also better able to understand the relation of any one of them, or of the group as a whole, to other hæmolytic anæmias.

Addisonian ("pernicious") anæmia must first be considered. It would seem that in this disease we are dealing with an actual vicious over-activity of the spleen.[1] This greatly increases portal hæmolysis, although the red cells are normal in their resistance to destruction. This is the only theory which fits all the facts and also explains the difference between hæmolytic jaundice and Addisonian anæmia. The over-activity of the spleen is produced by an exaggeration of the normal stimulus to splenic activity, of which we know little except that it seems to depend on digestion. When, therefore, the spleen is removed the disease is not cured, because the stimulus remains and the other tissues which are able to perform the work of the spleen vicariously proceed to do so. There may, indeed, be a period of improvement while these tissues are assuming their new duties but nothing more is to be expected. In many cases the stimulus will have been sufficiently strong to mobilize the vicariously acting tissues before splenectomy is done and in these still less improvement can be hoped for. In hæmolytic jaundice, on the other hand, the hæmolytic function of the spleen is never more than normal although, owing to the defect of the red cells, a normal function destroys many more cells. Hence

[1] It will not be generally accepted as proved that Addisonian anæmia is due to increased portal hæmolysis dependent on exaggerated splenic function but, nevertheless, this is the tendency of modern opinion in spite of the failure to demonstrate hæmolytic powers in splenic extracts in this disease. This failure is not really surprising since there is no a priori reason why this should be possible. Our tests for hæmolysins are of the most arbitrary nature and almost always include certain ceremonial washings of the red cells which cannot be without influence. The work of Lindeman [33] on citrate and other methods of transfusion is eloquent of the molecular changes which may be induced in red cells by subjecting them to apparently harmless agents, but these changes are not necessarily demonstrable by in vitro experiments, although Banti [36] and Robertson [35] find the cells of the splenic vein less resistant to hæmolysis than those of the peripheral areas. Hæmolysis was tested with saline solutions. This is suggestive of "some toxic substance or enzyme" in Robertson's opinion. One may be permitted to think that it is more probably an enzyme; in any case, the work of these authors is highly significant. Even with the methods at our command it would be most interesting to know whether the red cells leaving the splenic vein had perchance adsorbed some hæmolysin, for this is what one might expect to occur.

we see improvement after splenectomy and relatively little vicarious assumption of splenic functions.

The resemblance to paroxysmal hæmoglobinuria is very close, for the obvious reason that this is also a disease in which the red cells are injured by defect in the plasma. The whole reaction, however, is anaphylactoid and very acute in incidence and most of the hæmolysis is produced by actual destruction of circulating red cells. This is very rarely the case in hæmolytic jaundice, but in one case in which it did occur it must have been extremely hard to decide which disease was present. It is further of interest that some crises of hæmolytic jaundice have been precipitated by cold, just as is paroxysmal hæmoglobinuria and that both syndromes may depend on antecedent syphilis or malaria. It would almost have been pardonable to bring paroxysmal hæmoglobinuria into the group under discussion. This might have been done if there was a true congenital, familial form of the disease, but the writer has not been able to satisfy himself that this is the case. Moreover, the evolution of the two groups of diseases is somewhat different.

Similarly the anæmia of malaria may occur as a secondary and symptomatic form of acholuric jaundice, but there is lacking a primary and essential form in any way comparable.

Of the other hæmolytic anæmias such as those of pregnancy, and rarely of bothriocephalus or other infection, we can only say that these seem to resemble the Addisonian group.

The Group Pathology.

The pathology of the whole group may be summarized as follows: There is a primary disturbance in the composition of the plasma. This may be merely a molecular and physical change or may involve the presence of bodies new to the plasma in any form.

The plasma in turn affects the red cells and may (1) alter their reaction to saline hæmolysis, (2) change their shape, or (3) alter their humoral reactions in some unexplained way.

Of such cells a large number are rendered more liable to the ordinary processes of hæmolysis. So far as is known this is not true of oval cells or in auto-agglutination, but it is true of other changes. They are rendered more liable to both sorts of hæmolysis, i.e., splenic or phagocytic. One of these may predominate to the virtual exclusion of the other.

Where the hæmolytic function predominates there is jaundice, and this may be expected to disappear after splenectomy. The phagocytic function is not altered thereby.

The changes in the numbers of the red cells are dependent on the amount of destruction and the amount of marrow regeneration. Signs of the latter are seen in the presence of nucleated red cells, &c.

Except from the anæmia the patient's health is not disturbed in the above diseases, with the exception of Malins' syndrome, in their primary and essential forms. The plasma changes do not seem to have any toxic effects except those manifested in blood destruction and its results, such as splenomegaly, jaundice, &c.

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[10] Paroxysms of Cholæmia brought on by Exposure: WILSON, loc. cit.; BETTMANN, loc. cit.; TILESTON and GRIFFIN, *Amer. Journ. Med. Sci.*, 1910, cxxxix, p. 847, incriminate fatigue especially, and so do some other authors.

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[14] *Idem. Ibid.*, xxi, 1905, p. 473.

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Section of Medicine.

President—Sir ARCHIBALD GARROD, K.C.M.G.

Arthritis in Dysentery: Its Causation, Prognosis and Treatment.[1]

By GEORGE GRAHAM, M.D.

THE association of joint affections with dysentery was first noticed by Sydenham in 1672 and was also referred to by various authors in the eighteenth century. The subject does not seem to have attracted much notice although Hertwig [5] in 1863 and Dewevre [4] in 1887, who was responsible for finding the reference to Sydenham, have called attention to it. Salle [6] gave an account of it in 1903, and Beveridge [1] described five cases in which this complication occurred out of 147 cases of dysentery in South Africa. Manson states that swelling of the joints occurs as a rare complication of bacillary dysentery and a short summary of the subject is given by A. E. Garrod [2] in Clifford Allbutt's "System of Medicine." Joint affections have been reported as occurring only in cases of bacillary dysentery and no mention is made of them by the workers in India where amœbic dysentery is the usual type of disease.

However, since dysentery became prevalent under war conditions, Moorhead [3] has described eight cases in which swelling of the joints occurred in the course of a dysentery in which *Amœba histolytica* had been found. Three Frenchmen, Rist [7], Crouzon [8], Moreau [9], all working independently, observed swelling of the joints and conjunctivitis occurring in cases of bacillary dysentery. Colonel Cowan and Captain Miller [10] have described seven cases of arthritis which occurred in cases of bacillary dysentery in Egypt. Captain Waller [11] has recorded six cases of arthritis which occurred in Mesopotamia,

[1] At a meeting of the Section, held November 25, 1919.

and Captain Klein [12] reported five cases which occurred during an outbreak of dysentery at the Rouen base in 1917.

During the autumn of 1915, when many cases of dysentery, both amœbic and bacillary, came to Malta, Colonel Sir A. E. Garrod, A.M.S., saw a few cases in which the joints were affected. During the summer and autumn of 1916 when the dysentery cases which came from Salonica were almost entirely of the bacillary type a number of cases occurred. At Colonel Garrod's request, I took notes of some of these cases, nearly all of which I saw with him at the different hospitals. Besides these thirty-three cases there were some others of which I have no notes. After August, 1916, the cases were only taken from half the hospitals of the island and it is probable that over fifty cases occurred.

However, as it is an unfamiliar complication it has been thought worth while to describe the signs and symptoms.

(I) Clinical Symptoms.

With one exception all the patients contracted the dysentery elsewhere and the joints were usually already affected before the patients arrived in Malta, consequently we did not see the early stages and are dependent on the notes made either at the Salonica hospitals or on the hospital ship (where the symptoms often started) and on the patients' statements about the date of the onset, &c. Although we did not see the earliest joints affected we had the opportunity of observing the spread of the disease from joint to joint and of watching the progress of the trouble. The symptoms did not appear, as a rule, until the extreme severity of the dysentery was over, although in one case the pains in the joints started on the same day as the dysentery began. The usual day of onset (Table I) lay between the sixth and the twenty-third day after the first symptoms of dysentery. In one case the symptoms began on the ninetieth day but in this case the attack had been severe with one or more relapses, and blood and mucus had been passed only thirteen days before the onset of the joint symptoms.

The first symptom was severe pain, which preceded the swelling of the joint by some hours. The effusion into the joint took place very quickly and was usually large. The severe pain went away long before the swelling of the joint subsided. There was no reddening of the skin and the joint was not hot even in the most acute cases that we saw. In some cases many joints were attacked on the same day, but in most of the cases the joints were attacked at intervals. In one case four

joints were affected in the course of ten days. The commonest joint to be affected was the knee-joint; both knee-joints were involved in sixteen cases and one knee-joint in fifteen cases (Table II). The ankles and elbows came next in order of frequency. The small joints of the wrists and hands were symmetrically involved in three cases; in five cases one hand only was attacked. The sterno-clavicular joint was not affected in any of the cases of this series, but Major Hessel saw two cases in which it was involved; the temporo-maxillary joint was attacked twice.

TABLE I.

Day of onset	Character of dysentery	Character of joint affection	Date of eye infection
0	None	Severe	2
1	Mild	Very severe	—
6	Severe	Severe	—
8	Mild	Severe	—
8	Mild	Severe	—
9	Mild	Mild	34
10	Mild	Severe	25
12	Mild	Severe	12
12	Severe	Mild	—
12	Severe	Severe	—
14	Mild	Mild	15
14	Mild	Mild	—
14	Severe	Mild	14
14-21	Mild	Severe	—
15	Mild	Severe	15
16	Mild	Severe	12
16	Mild	Mild	—
17	Mild	Mild	—
17	Severe	Mild	—
18	Severe	Severe	20
18	Severe	Severe	—
18	Severe	Severe	—
19	Mild	Mild	13
19	Mild	Mild	—
21	Mild	Severe	18
23	Mild	Mild	—
23	Mild	Mild	—
26	Mild	Severe	—
29	Severe	Mild	21
90	Severe	Mild	88

The onset of the symptoms is said by some observers to occur between the fourteenth and twenty-first days, though Waller mentions that one case began on the eleventh day. In our series (Table I)

six men started their symptoms before the twelfth day and the onset in the remaining cases lay between the twelfth and twenty-ninth days with the exception of one case. This man did not show any symptom until the ninetieth day of the disease but he had had a relapse of the symptoms of dysentery only thirteen days before the joint affection began. An analysis of the table does not show any relation between the severity of the dysentery and that of the joint affection. A mild dysentery may be followed by a severe joint affection or vice versa.

TABLE II.

Knees	Right	24
,, ...	Left	23
Ankles	Right	5
,, ...	Left	10
Shoulders	Right	4
,, ...	Left	5
Elbows	Right	7
,, ...	Left	9
Wrists	Right	2
,, ...	Left	3
Hands	Right	6
,, ...	Left	5
Great toe	...	1
Temporo-maxillary	...	2

The analysis of the figures suggests that the patients who show symptoms early are more liable to have severe attacks. Thus, of the fourteen cases which were described as arising before the sixteenth day nine were severe and five mild; of the fifteen cases which arose after that day six were severe and nine mild. However, one of the most severe cases did not begin until the twenty-sixth day of the disease. In the severe cases there was a considerable rise in the temperature. This rise was sometimes synchronous with the onset of the joint lesion; it more often preceded it and sometimes succeeded it. The temperature oscillated between 99° and 100° or 102° F. at night as in Charts I, II, III, IV. In the very severe cases the temperature continued to oscillate for many weeks. No other cause for the pyrexia could be discovered and except for the joint lesion there was nothing abnormal about the patients. There is often a considerable rise of temperature in the early stages of a severe dysentery, but in these stages the acute symptoms of dysentery had usually subsided before the onset of the joint symptoms. In the mild cases the temperature was little if at all raised. In twenty of the thirty-three cases there was long continued pyrexia, while in the other thirteen cases pyrexia was slight and of short duration.

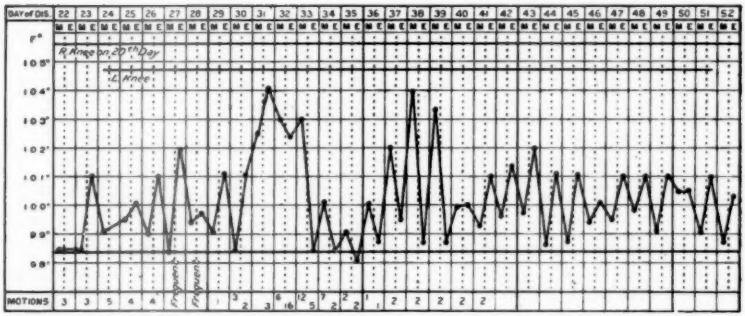


CHART I (Case II).

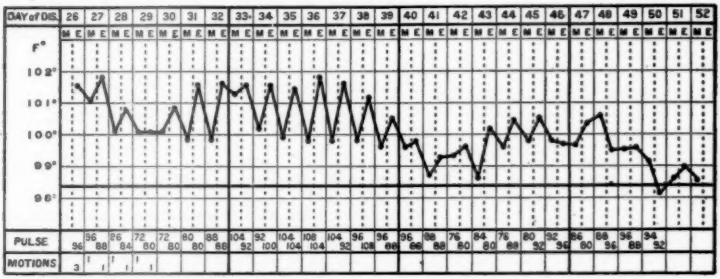


CHART II (Case VII).

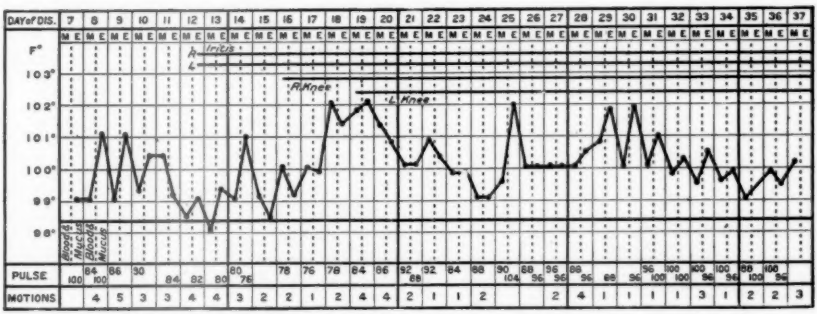


CHART III (Case XII).

The progress of these cases varied greatly. Some patients made quick recoveries: seven of them were quite well within four weeks. Twenty were ill for a much longer period and three, who were sent home to England, showed little improvement after two months. One of these was allowed to walk after about six months and was then just able to use his hands to write a letter. When the joint symptoms

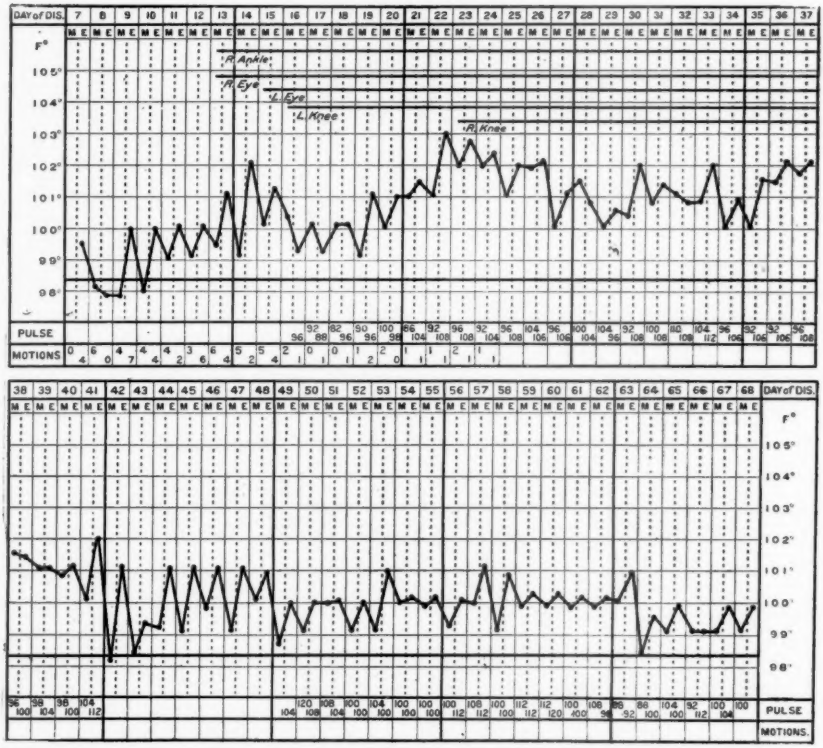


CHART IV (Case XXX).

subsided quickly there was seldom any after-trouble, but when this process was a slow one with relapses there was a great tendency to stiffness. It is not possible to say what the ultimate fate of these cases will be. The cases seen by Beveridge in South Africa were all of short duration and the joints improved as soon as the temperature fell, but Dewevre states that in some cases the process of complete recovery is a

slow one, and records one in which it lasted seven months. Garrod has seen some chronic cases in London at different times and believes that the joints ultimately recover completely.

Some of the patients developed eye symptoms and this point also has been noted previously. These symptoms were of two kinds: (1) a marked redness and injection of the vessels of the conjunctiva. This was not accompanied, as a rule, by any purulent discharge, though it was attended by considerable pain and lachrymation. In a few cases photophobia was present. (2) A cyclitis or an irido-cyclitis. This occurred in seven cases, five of which were seen by Captain W. H. Kiep, R.A.M.C., and have been described in detail elsewhere—Kiep and Maxwell [13]. Captain Kiep has also seen irido-cyclitis in two other cases of Shiga dysentery in which there was no involvement of the joints. It is possible that some of the patients with conjunctivitis also had a cyclitis which was not diagnosed but the iris was certainly not involved.

The eye symptoms appeared between the fourteenth and thirty-fourth day of the disease and in one case on the eighty-eighth day; sometimes a few days before and sometimes just after the joints were attacked (Table I, p. 25). The conjunctivitis always cleared up quite quickly with washes such as argyrol 2 per cent. When the irido-cyclitis developed under observation it yielded readily to treatment with atropine. In three cases in which the irido-cyclitis was well developed before the patients arrived in Malta, recovery was very slow. One man was left with ring synechiæ and an iridectomy was eventually performed. Conjunctivitis was also noticed by Rist and Crouzon but Moreau saw two cases of conjunctivitis and iritis.

As arthritis is a fairly constant feature of serum disease and many dysentery patients were treated with serum it was necessary to exclude the possibility that the arthritis was caused by the serum. Eight of these cases were treated with serum but in one case it was given for a relapse of dysentery after the joint symptom had started (Table III, p. 30).

Serum arthritis is a very painful disease, but it is extremely rare for any swelling of the joints to occur. I have only seen it on two occasions, and in each case it was suspected that the patient was developing dysenteric arthritis. But the presence of the urticaria and the transient nature of the swelling (twenty-four to forty-eight hours) constitute evidence that the joint swelling was part of an unusually severe attack of serum disease.

In the cases of dysenteric arthritis which had been treated with

serum the joint symptoms were absolutely different from those of serum arthritis; one patient was well in three weeks and all the others were ill more than four weeks.

TABLE III.

Serum			Joints		
Number of doses	Amount of dose, in cubic centimetres	Day of disease	Day of disease	Duration of disease
1	?	10	18	Over 5 weeks
2	?	4	9	Well in 3 weeks
8	20 c.c.	4 before 8th day, 4 between 8th and 14th days	18	Over 4 weeks
1	40 c.c.	6	12	Over 7 weeks
1	40 c.c.	Early ? day	90	Well in 4 weeks
1	?	Early ? day	6	Over 7 weeks
1	?	Early ? day	19	Over 4 weeks

The question whether the administration of serum prevents the involvement of the joints is more difficult to answer. Two of the seven men were given serum fairly early. One man had two doses of serum for a slight attack of dysentery after the fourth day and he developed a mild arthritis on the ninth day, after going for a short walk. The other man had four doses before the eighth day and another four doses of 20 c.c. between the eighth and fourteenth days; he developed a very severe arthritis on the eighteenth day. The other four men had one dose of serum, and as we could not ascertain either the amount of serum given or the day of disease on which it was given, it is impossible to say much about the effects of the serum in preventing the onset of dysenteric arthritis. Major W. T. Hessel, R.A.M.C., has analysed the dysentery cases which developed locally in this command, and none of the cases which were treated with serum early and in adequate doses (at least 100 c.c. in two doses) developed any joint trouble. This evidence is suggestive, but the number of cases which developed locally was not large. As the joint lesion is an infrequent complication, it needs a careful analysis of a large number of cases, treated with and without serum, before a definite answer can be given.

The usual provisional diagnosis made in these cases was rheumatism or acute rheumatism. The mode of onset and the way in which one joint is involved after another, does suggest rheumatic fever. But the absence of the flushing of the skin and of great heat in the joint is against that diagnosis. The behaviour of the joints when treated with sodium salicylate in adequate doses (20 gr. every four or two hours)

is quite different from that of rheumatic fever. Aspirin seems to be more efficacious than sodium salicylate in relieving the severity of the pain, but the joint swelling does not subside under its use. In its extremely chronic nature also it is quite unlike rheumatic fever. Moreover in no case was there any sign of endocarditis.

The general appearance of the disease resembles that of gonococcal arthritis, and this resemblance is enhanced by the frequency with which conjunctivitis and irido-cyclitis accompany the joint lesions (the joint-eye syndrome). The cases, however, differ in that the fasciæ do not seem to be affected as they so often are after gonorrhœa. This possibility was carefully excluded and in no case was there any evidence of any recent gonorrhœa. One man had been infected with gonorrhœa seven years previously and one case had been diagnosed as gonorrhœal arthritis. In this case a Gram-negative diplococcus was found in the eye discharge on one occasion, but no gonococci were obtained on subsequent occasions, either from the eye or from the urethra. However, it is not safe to make the diagnosis on the presence of a Gram-negative diplococcus as several varieties of these may be present in eye discharge. The man denied that he had had gonorrhœa and there was a definite history that blood and mucus had been passed for about ten days, though this had been ascribed to an attack of malaria. However, the fact that the patient's blood agglutinated Shiga's bacillus in a dilution of 1 in 320 on the forty-fifth day of disease is fair evidence that the previous attack had been dysentery.

The possibility of Mediterranean fever was considered in three severe cases, but the blood culture was sterile and the blood did not agglutinate *Micrococcus melitensis*. The subsequent course of the cases did not resemble that of Mediterranean fever.

The only other joint-condition which bears any resemblance to these cases is "rheumatoid arthritis." In the acute stages, however, there is little resemblance, and the presence of the eye symptoms would prevent that diagnosis being made. In the late stages, however, especially when the hands are affected, there is considerable resemblance, and if the cases were first seen after many weeks, the diagnosis of "rheumatoid arthritis" might be made, if no history of the onset of the disease were available.

The treatment of these cases is most unsatisfactory and the disease seems to run its course. Sodium salicylate, even in large doses of 20 gr. every four or two hours, has little apparent effect either on the joint swellings or on the height of the temperature. Aspirin seems to be

effective in relieving pain. Quinine was tried in some cases. Locally methyl salicylate has been used but with little effect; Scott's dressing seemed to be rather more efficacious. When there is severe pain or any tendency for the limb to assume a bad position the application of a splint is advisable. Puncture of the joints was tried in a few cases without effect. Massage is of great use in the later stages to overcome any stiffness, and radiant heat was tried with some success in one severe case.

Serum was given in two cases for a relapse of the dysentery, but the joints were not relieved to any marked extent. Rist has used large doses of serum, 50 to 60 c.c. for four days, and states that the joints recover very quickly. In the light of his experience, serum deserves a trial, but I have had no chance of observing its effect.

(II) Analysis of the Cases.

The cases which showed joint symptoms may be divided into five groups for the purpose of investigating the cause of the joint lesion.

(I) Cases which have been diagnosed as dysentery on clinical grounds, in which either Shiga's bacillus was isolated from the stool or in which the blood contained a specific agglutinin for Shiga's bacillus.

The cases in this group do not need any special comment, as the clinical diagnosis of dysentery was confirmed by the pathological findings and the clinical picture of the joint-eye syndrome was the one described. In five patients Shiga's bacillus was isolated from the stool, and in seven the blood contained a specific agglutinin for Shiga's bacillus.

(II) Cases which have been diagnosed as dysentery on clinical grounds, in which either Flexner's bacillus was isolated from the stool or in which the blood contained the specific agglutinin for Flexner's bacillus. In two patients Flexner's bacillus was isolated from the stool, and in two the blood agglutinated Flexner's bacillus. The two cases in which Flexner's bacillus was isolated need some comment.

In Case XIII the joint symptoms developed some fourteen to twenty-one days after an attack of diarrhœa (nine to ten stools a day for nine days), which was not severe enough to prevent the man doing his work. While the man was in hospital here the diagnosis of arthritis following a mild dysentery was suggested. Shortly after this

he had another attack of diarrhœa in which mucus, but no blood, was passed. In this attack, which was no worse than the previous attack, Flexner's bacillus was isolated from the stool. This observation made the previous diagnosis of dysenteric arthritis much more certain.

Case XIV was that of a doctor in the R.A.M.C., aged 47, *circa*, who stated that he had had a bad attack of gout twenty years previously, and had then been ill for some time. The dysentery, which was of local origin, was very mild, and only lasted three days, but Flexner's bacillus was isolated from the stool, and the blood agglutinated Flexner's bacillus strongly. The joint lesion developed on the fourteenth day and was very severe. He had great pain and the joint condition cleared up very slowly. Colchicum gave no relief. In view of the previous history of joint troubles considerable doubt existed whether this was a recurrence of the previous disease or was to be ascribed to the mild attack of dysentery. Against the latter view is the history of a previous attack of an unknown joint affection twenty years previously, and the fact that the joint symptoms were more severe than is usually the case in dysenteric arthritis. In favour of the view are the facts that (1) the joints were attacked fourteen days after the onset of the dysentery; (2) that the clinical picture was similar to that of the other cases. It is true that the symptoms were more severe than those of most of the cases, but they were not more severe than those of Case XV, in which the patient also had a Flexner dysentery.

(III) Cases which were diagnosed as dysentery on clinical grounds, but in which no pathogenic bacilli were isolated from the stool, and in which the blood did not agglutinate any of the dysentery bacilli.

The group contains twelve cases in which the diagnosis rests only on clinical evidence. In two cases the attack of dysentery was very severe, but in the other ten cases it was fairly mild.

During the summer and autumn of 1916 the diagnosis of dysentery was often made on clinical grounds when no positive pathological findings could be obtained. Several explanations may be put forward to explain this state of affairs :—

(1) In the cases of this group the stools had not been examined at the onset of the disease, which is the most favourable time for isolating the dysentery bacilli. Many of the cases were mild ones, and the men were practically convalescent from their dysentery when they arrived here, and it was unlikely that the dysentery bacilli would be isolated under these conditions.

(2) The blood of these patients was tested once shortly after their arrival here, and no agglutinins for the dysentery bacilli were found. The time of appearance of the agglutinins in dysentery is a subject on which much work is being done at present. Captain J. Speares and Dr. P. P. Debono have found that in cases of Shiga dysentery the agglutinins appear regularly by the second week. In cases of Flexner dysentery the development of the agglutinin is capricious, and in some cases in which Flexner's bacillus was isolated no agglutinins developed at all.

(3) It is still a moot point whether all the bacilli capable of causing dysentery have been identified. In a certain number of cases of dysentery which occurred during the summer and autumn of 1916, bacilli were isolated which gave the sugar reactions of Shiga's bacillus, but which did not agglutinate with the specific Shiga serum. The blood of these patients did not agglutinate Shiga's or Flexner's bacillus, and the pathological findings were therefore negative.

As there are these difficulties in the way of getting a positive pathological result in a certain number of cases of dysentery, I have felt justified in including the twelve cases in Group III under the heading of clinical dysentery. The joint affections in these cases were very similar to those of the proved Flexner and Shiga cases, but were of a rather milder type.

(IV) Cases in which the diagnosis of dysentery was not made at the onset of the disease. This group contains three cases which are of great interest.

Case XXIX was that of a man who passed blood and mucus for four days, but a diagnosis of dysentery was not made. The joint symptoms developed on the eighth day, together with a very severe constitutional disturbance. The joint affection was one of the most severe of the series, and it was six months before he was able to walk or write letters.

Case XXX stands in rather a different category, and has already been described. In this case the history of diarrhœa with blood and mucus was ascribed to malaria, but the joints and conjunctivitis to a gonococcal infection. The high agglutination, 1 in 320, to Shiga's bacillus is good evidence that the original infection was really dysentery.

In Case XXXI the patient, an officer, had not had any illness resembling dysentery, and had only had the mildest of diarrhœa. The joint affection came on suddenly, and was severe, but the joints were

quite well by the seventy-sixth day. An irido-cyclitis developed on the second day, and was still bad on his arrival at Malta on the seventy-sixth day. Captain Kiep saw the patient on account of his eyes; he elicited the history of the joint affection, and had the blood and stools examined. The stool culture was negative, but the blood agglutinated Shiga's bacillus in a dilution of 1 in 800. A positive agglutination in this dilution is evidence that the patient has been infected with Shiga's bacillus, and yet no history of any illness resembling dysentery could be obtained. It is recognized that very mild cases occur in all epidemics, but such a high agglutination in a man who could only give a vague history of diarrhœa was most unexpected.

(V) Cases in which there was no evidence of dysentery.

Case XXXII was that of a man whose joint symptoms and constitutional disturbance were almost as severe as those of Case XXIX. In this case the only history of previous illness which could be obtained was that of a mild diarrhœa of three days' duration. The joints became affected twenty-one days after the onset of the diarrhœa. The stool and blood examinations gave negative results, and a blood culture was sterile. The clinical picture was almost identical with that of Case XXIX, and it is difficult to believe that the cause of the joint trouble was not the same in the two cases.

Case XXXIII was that of a man who gave a history of a mild diarrhœa which was followed by a joint affection on the fifteenth day. The joint affection was fairly severe and the temperature oscillated for some days.

These two cases are placed in a group by themselves as there was no evidence of any dysentery.

Joint symptoms occurred in thirty-three cases, and in eighteen cases the nature of the dysentery was ascertained; in twelve cases the diagnosis of dysentery was made on clinical grounds; in three cases the diagnosis of dysentery was originally overlooked, and in two cases there was no evidence of any attack of dysentery.

In the light of this evidence it seems clear that all cases of joint swelling which arise when bacillary dysentery is prevalent must be carefully investigated. Even a mild dysentery, which may pass almost unnoticed when there is a great deal of disease about, may cause severe joint symptoms, and the history of the dysentery may be only elicited by cross-questioning. Even if a history of diarrhœa can be obtained, it is worth while examining the stools and testing the agglutinating power of the blood. Whilst it is not suggested that every case of joint

swelling which occurs under these conditions is secondary to a dysenteric infection, yet it may be said that these cases should always be regarded with suspicion.

The evidence that arthritis occurs as a complication of amœbic dysentery is very weak. Moorhead saw eight cases in Egypt after the Gallipoli expedition, in which the *Amœba histolytica* had been seen in the stool. However, a certain number of cases of dysentery arising on the Peninsula were undoubtedly mixed infections, and it is quite possible that these cases were of that kind.

Two hypotheses must be considered as to the cause of the joint lesion :—

(1) An infection of the joint with dysentery bacilli.

(2) A chemical effect set up by the toxins produced by the dysentery bacilli.

There is, unfortunately, no direct evidence in favour of either hypothesis. The dysentery bacilli have not hitherto been isolated from the joint fluid, but the joints have not often been punctured. Unfortunately we did not see the early stages of these joints, and under these circumstances we did not feel justified in puncturing them as the most favourable time for isolation of the bacilli had passed. One joint was punctured within twenty-four hours of the onset, but the culture remained sterile. Rist punctured the joints on five occasions but no bacteria were found. Waller punctured six joints but the cultures remained sterile. Klein had the same results in five cases. Klein tested the joint fluid for agglutinins four times and found a positive result to Shiga's bacillus in three cases and to Flexner's bacillus in one case. In one case the titre of the joint fluid was above that of the blood 50 to 18 units, and in one it was half that of the blood, 24 to 48 units; in the other two cases it was about the same. Dr. Debono examined the joint fluid of one case in Malta; the fluid was sterile but the agglutinating titre was less than that of the blood. If the joint affection is caused by a direct invasion of dysentery bacilli, these must have reached the joint through the blood stream. In dysentery the bacilli have only been found in the blood stream on a few occasions.

The absence of any positive pathological finding is therefore in favour of the view that the toxins are responsible for the joint lesion, but too much stress should not be laid on the purely negative result. There is a certain amount of indirect evidence in favour of the first hypothesis. The pyrexia, which occurs in the most severe cases and which may

continue for some weeks after all the intestinal symptoms have subsided, is much more suggestive of a direct infection of the joint than of an inflammation set up by a toxin. However, the best indirect evidence is the striking resemblance between the clinical picture of dysenteric and gonococcal arthritis. The association of joint swellings, irido-cyclitis and long continued pyrexia in the two diseases has already been referred to. In the case of the gonococcal arthritis the gonococcus has been recovered from the joint fluid in a certain number of cases, and it has also been recovered from the blood in at least two cases of endocarditis. It seems quite certain that the joint lesion in gonococcal arthritis is caused by a direct infection with bacteria, and as the resemblance between the clinical picture of the two diseases is so great it is suggestive that the joint lesion in dysentery is caused by the dysentery bacilli. The case in which both arthritis and iritis were present and the serum agglutinated Shiga's bacillus in a dilution of 1 in 800, although dysenteric symptoms had never been present, is strongly in favour of this view.

(III) Conclusions.

(1) An affection of one or more joints takes place as a rare complication in bacillary dysentery.

(2) The joint affection is often accompanied by a severe constitutional disturbance.

(3) Conjunctivitis and irido-cyclitis are secondary complications in about half the cases.

(4) The disease runs a slow and chronic course but apparently ends in a complete recovery.

I have to thank Colonel Sir A. E. Garrod, A.M.S., F.R.S., for drawing my attention to this subject and for his constant help and advice. I am indebted to the medical officers who have furnished me with the notes and charts of their cases, and especially to Major W. T. Hessel, R.A.M.C., Captain W. S. Finch, R.A.M.C., and Captain W. H. Kiep, R.A.M.C. The bacteriological work was done in the laboratories of Tigne, St. Andrew's and Imtarfa hospitals under the charge of Colonel A. C. O'Sullivan, Major J. A. Arkwright, and Major Broughton Alcock respectively, and I have to thank them for their assistance. I wish to thank Captain J. Speares, R.A.M.C., and Dr. P. P. Debono for permission to quote their unpublished results.

GROUP I.

Case	Day of onset of dysentery	Day of disease on arrival at Malta	Character of disease	Serum		Bacteriology		Joints		Temperature	Progress	Eye Symptoms	
				Day of disease	Dose	Stool culture	Agglutination	Day of disease	Joints affected			Day of disease	Progress
I	July	20 circa	Severe, but short	—	—	Shiga's bacillus at Salonica	+ to Shiga's bacillus	17	Right elbow and knee	High at onset	Well in 10 days	—	—
II	Aug. 6	20	Moderately severe	10	One dose	Shiga's bacillus	Weak + to Shiga's bacillus	18	Right and left knees	High and long-continued (Chart I)	Joints still bad on 55th day	—	—
	Aug. 28	42	Relapse	45	40 c.c.								
III	Oct. 21	? day	Moderately severe	—	—	Shiga's bacillus at Salonica	Shiga's bacillus $\frac{1}{100}$ ++, $\frac{1}{200}$ +	21	Left knee and ankle	?	Had a relapse in Malta; joints painful	18 circa	Mild conjunctivitis
IV	Oct. 27	26	Mild	4	Two doses	Shiga's bacillus at Salonica	?	9 21	Right knee Left knee and right ankle	Slight	Joints well on 34th day	34	Cyclitis, right eye; rapid improvement with serum
V	Sept. 23	10	Mild	—	—	Shiga's bacillus	Shiga's bacillus $\frac{1}{300}$ ++ on fortieth day; later, $\frac{1}{500}$ ++	9	Right and left knee, right elbow, left shoulder	High, oscillating, long-continued	Very slow progress, quite well after six months	34	5th of relapse, conjunctivitis
		30	Severe	32	60 c.c.							35	Irido-cyclitis right and left, complete recovery
VI	July 20	16	Severe	6	40 c.c.	—	Weak + to Shiga's bacillus	12	Left knee	?	Joints still swollen on 40th day	—	—
VII	July 8	26	Severe	—	—	—	Strong + to Shiga's bacillus	23 24 25 33	Right and left elbow Left knee Left wrist Right knee	101·5°—99° F. (Chart II)	Very slow progress	—	—
VIII	June 24	38	Severe	—	—	—	Strong + to Shiga's bacillus	18	Right knee and hand	No details	Joints still bad on 38th day; improving on 75th day	20 38	Conjunctivitis right and left eyes Irido-cyclitis right and left; got well quickly
IX	Aug. 19	18	Severe	—	—	—	Weak + to Shiga's bacillus	14	Left knee and right ankle	100°—102° F. at onset	Walking about on 38th day	14	Mild conjunctivitis
X	Aug. 16	20	Severe	—	—	—	Strong + to Shiga's bacillus	29	Right elbow, shoulder and forefinger	Oscillating, 18th to 54th day	Walking about on 64th day	21	Mild conjunctivitis
XI	Oct. 4	7 circa	Mild	—	—	—	$\frac{1}{100}$ + to $\frac{1}{320}$ + Shiga's bacillus	1	Right and left ankles, knees, wrists, hands	Oscillating for some time	Very slow progress	—	—
XII	Sept. 29	47	Mild	—	—	—	60 $\frac{1}{200}$ ++ 110 $\frac{1}{400}$ ++	16	Right and left knee	Oscillating to 100°-101° F. (Chart III)	Walked on 80th day	92 110	Severe irido-cyclitis right and left; ciliary hyperæmia gone Iridectomy for ring synechiæ

GROUP II.

Case	Day of onset of dysentery	Day of disease on arrival at Malta	Character of disease	Serum		Bacteriology		Joints		Temperature	Progress	Eye Symptoms	
				Day of disease	Dose	Stool culture	Agglutination	Day of disease	Joints affected			Day of disease	Progress
XIII	July 1	30	Very mild; Stool nine to ten a day for nine days	—	—	Not done	Not done	14-21	Right and left knee; temporo-maxillary; big toe	—	Right knee still swollen on 55th day; joints very stiff	—	—
		55	Mucus, no blood	—	—	Flexner's bacillus isolated	—	—	—	—	—	—	—
XIV	Sept. 20	Local case	Very mild, ill three days	—	—	Flexner's bacillus isolated	Strong + to Flexner's bacillus	14	Right knee and left wrist	101°--99° F. from 17th to 23rd	Severe attack; very slow progress; improving on 82nd day	—	—
								22	Left knee and ankle				
								27	Right wrist; very great pain	—	—	—	—
XV	May 18	30 circa	Severe	Between 1st and 8th	Four doses	—	Strong + to Flexner's bacillus	18	Right and left knee, right hand	102°—99° F., for three weeks	Joints nearly well on 67th day; right hand still stiff	—	—
				Between 8th and 14th	Four doses	—	—	—	—	—			
XVI	Jan. 27	26	Not severe	—	—	—	Strong + to Flexner's bacillus	26	Right ankle	Oscillating for a while	Joints normal on 70th day; two relapses between 46th and 60th days	—	—
								32	Right knee				
								34	Left knee; left knee punctured, clear yellow fluid; culture, no growth				

GROUP III.

Case	Day of onset of dysentery	Day of disease on arrival at Malta	Character of disease	Serum		Bacteriology		Joints		Temperature	Progress	Eye Symptoms	
				Day of disease	Dose	Stool culture	Agglutination	Day of disease	Joints affected			Day of disease	Progress
XVII	July 30	4	Mild	—	—	—	—	14	Left knee	Oscillating before onset of joints	Good recovery	15	Mild conjunctivitis right and left
XVIII	July 12	?	Mild	—	—	—	—	19	Right and left knee	99° — 101° F. for a few days	Walking on 40th day	13	Mild conjunctivitis right and left
XIX	May 21	96	Severe, with relapses	In first few days	40 c.c.	—	—	90 92	Right knee Left knee, right and left elbow	102° F.; rise continued until 118th day	Swelling gone on 118th day	88	Mild conjunctivitis right and left
XX	July 1	?	Mild	—	—	—	—	17 25	Left ankle Left knee, elbow and shoulder	?	Fairly quick recovery	—	—
XXI	June 2	?	Severe	?	One dose	—	—	6	Right and left knees, elbows, shoulders, and hands	98° — 101° F. for some time	Joints recovered quickly; relapse on 40th day, and hands still bad on 56th day	—	—
XXII	June 2	?	Mild	—	—	—	—	8	Left knee and elbow	?	Left elbow still bad on leaving for England on 30th day	—	—
XXIII	Aug. 18	22	Mild	—	—	—	—	15 23	Right elbow Right knee; hand in a relapse	—	Joints still bad on 28th day	—	Conjunctivitis and irido-cyclitis, right and left
XXIV	Aug. 13	?	Mild	—	—	—	—	16	Several joints	?	Got well quickly	16	Conjunctivitis, right and left, got well quickly
XXV	Aug. 16	?	Mild	?	One dose	—	—	19	Right ankle; a metacarpal joint	None	Joints still bad on 49th day	—	—
XXVI	Sept. 9	?	Mild	—	—	—	—	14	Right knee, ankle, and elbow; painless swelling	?	—	—	—
XXVII	Sept. 4	?	Very mild	—	—	Not examined	Not examined	23	Right knee	?	Mild case	—	—
XXVIII	Aug. 29	?	Very mild	—	—	Not examined	Not examined	23	Right knee	?	Nearly well	—	—

GROUP IV.

Case	Day of onset of dysentery	Day of disease on arrival at Malta	Character of disease	Serum		Bacteriology		Joints		Temperature	Progress	Eye Symptoms	
				Day of disease	Dose	Stool culture	Agglutination	Day of disease	Joints affected			Day of disease	Progress
XXIX	May 30	21	Mild; blood and mucus four days	—	—	—	—	8	Right and left knees, ankles, elbows, hands and wrists	Oscillating 100°—102° F. for over 70 days; blood culture negative	To England on 70th day, still very ill; able to walk and write letters on 180th day	—	—
XXX	Sept. 20	45	Blood and mucus for ten days; no malaria parasites	—	—	—	++ $\frac{1}{500}$ to Shiga's bacillus	12 13 22	Right ankle Left ankle Left knee and right ankle; diagnosed as gonococcal arthritis	Oscillating for seventy days (Chart IV)	Joints well on 76th day	12	Conjunctivitis, right and left
XXXI	—	76	No history of dysentery	—	—	—	++ $\frac{1}{500}$ to Shiga's bacillus	1 2 5	Right ankle Right knee Right shoulder		Joints quite well on 76th day	2	Irido-cyclitis; iritis still bad on 76th day

GROUP V.

XXXII	May 8	36	Mild diarrhœa, three to four a day for three days	—	—	—	—	26	Left knee and ankle; later, right knee and ankle	101°—102° F. from 36th to 66th day	To England on 66th day; joints still swollen and painful	—	—
XXXIII	Sept. 1	26	Diarrhœa for five days; very mild	—	—	—	—	26	Temporo-maxillary; left ankle and knee	Oscillating	—	—	—

Note.—At the laboratories of Tigne and St. Andrew's Hospitals, under the charge of Colonel A. C. O'Sullivan, R.A.M.C., and Major J. A. Arkwright, R.A.M.C., the agglutinations are done by the macroscopic method, and the dilution is stated. At the laboratory of Imtarfa Hospital, under the charge of Major R. B. Broughton Alcock, R.A.M.C., S.R., the agglutinations are done by the slide method, and the results are stated as strong or weak +.

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Section of Medicine.

President—Dr. A. F. Voelcker.

The Early Manifestations of Scurvy.[1]

By Ransom Pickard, Colonel, C.B., C.M.G., A.M.S.(T.F.),
and G. W. Lloyd, M.D., B.S.

Two groups of cases are described in this paper, having the same symptoms, but investigated from a somewhat different standpoint; for this reason they are kept apart. Another group, bringing the total cases to thirty-two, was examined, presenting the same symptoms, but is not referred to in detail.

The manner in which the first group was discovered is of interest. In August, 1915, in a Divisional Rest Station in France, there were a large number of cases in which the symptoms complained of were pains in the joints and muscles. In an investigation of these a proportion were found to be anæmic and to have an affection of the gums, either pyorrhœa, bleeding, or loose teeth. It was at first assumed that the pains and anæmia were of septic origin, the cause being the diseased gums. One of us (G. W. L.) discovered two patients who each had a subcutaneous hæmorrhage.

Upon this, scurvy was thought to be a possible cause of the symptoms. But if this were so, it was argued (by R. P.) that some of the patients should have a degree of night-blindness. Tests were devised, necessarily of a rough nature but accurate enough for the purpose, and by these it was obvious that nine of the twelve cases in the first group had a moderate degree of night-blindness, though none of them had complained of it nor were aware of it.

[1] At a meeting of the Section, held February 24, 1920.

The second group was chosen in a different manner, anæmia being the symptom which was taken as the basis of selection. One day, when the walking cases were parading for dinner, we went along the line and picked out ten men who appeared to be the most anæmic. These had their hæmoglobin estimated. One had a percentage of 94, so he was removed from the group; he was suffering from whitlow. A second case, with 70 per cent. of hæmoglobin, was also excluded because he was suffering from tertiary syphilis. Neither of these two cases had night-blindness. The remaining eight were then examined as to pain, affections of the gums, and night-blindness: they were found to have some or all of them.

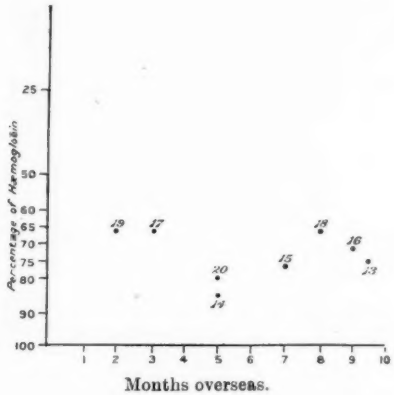


CHART I.

The numbers within the lines are the serial numbers of cases.

Tables I and II show the incidence of the symptoms in the cases comprising the two groups.

Rashes.—Three cases in Table I had rashes. These consisted of a few purpuric spots each around a hairy follicle on the front of the thighs and occasionally on the legs. They would fade and one or two fresh spots appear. They were about 2 mm. in diameter.

Indurations (subcutaneous hæmorrhages).—These consisted of indurations, with the skin over them discoloured as in a bruise, and occurred on the buttocks, outer part of thighs and upper arms. In two cases after becoming absorbed they reappeared at the same sites. Their position no doubt was due to the pressure of lying on the ground.

TABLE I.—FIRST SERIES OF CASES.

Serial number of case	Age	Anæmia	Gums		Teeth		Muscular pains	Ecchymosis	Rash	Night blindness
			Pyorrhœa	Bleeding	Number loose	Number lost overseas				
1	20	+	+				+			+
2	19	+						+	+	
3	38	+	+					+		
4	36	+					+			+
5	18	+							+	+
6	32	+		+	+	1	+		+	+
7	38	+	+	+	2	1	+			+
8	43		+			2	+			+
9	28	+	+	+		5	+			+
10	31	+	+				+			
11	32		+				+			+
12	25	+					+			+
Total cases with each symptom		10	7	3	2	4	9	2	3	9

TABLE II.—SECOND SERIES OF CASES.

Serial number of case	Age	Percentage of hæmoglobin	Gums		Teeth		Muscular pains	Ecchymosis	Rash	Night blindness	Months overseas
			Pyorrhœa	Bleeding	Number loose	Number lost overseas					
13	26	75		+ s			+			+	9½
14	29	85		+ s		3	+			+	5
15	46	76		+ s			+			+	7
16	42	72	+		3		+				9
17	23	66		+ s						+	3
18	41	66	+		1		+			+	8
19	19	65	+	+						+	2*
20	40	80	+	+ s			+			+	5
Total cases with each symptom		8	4	6	2	1	6			7	

s = slight. * ? tubercular.

Pyorrhœa.—Many had the grey necrosis of epithelium of mucous membrane at dental edge of gums characteristic of ulcerative stomatitis. It was about this time that ulcerative stomatitis (ulcero-membranous gingivitis) appeared in the Army.

It is maintained by us that the symptoms anæmia, pains in the joints and muscles, ecchymosis, swollen gums with suppuration, bleeding, or loose teeth, and night-blindness form together symptoms of scurvy in its slighter degrees: they are in miniature the same symptoms that exist in the severe cases that are usually included in scurvy. It will be seen from Tables I and II that most of the cases did not suffer from all the above symptoms, but this is exactly what happens in mild cases of any disease; it is usual to find an incomplete picture.

Night-blindness.—Special attention must be drawn to this, as it was the symptom that finally decided the diagnosis. The methods of examination were as follows:—

Tests for Night Blindness.—(*a*) The vision was taken in daylight, without and with correction, if the latter were necessary.

(*b*) Then the field of vision was taken in the four principal meridia, at one metre distance, by a circular white spot 10 mm. in diameter, on a black surface 3 cm. by 5 cm., the patient fixing the examiner's nose. This method gives rough estimations only, but the differences between the fields of normal men and the patients were quite large enough to be observed. The losses, when large enough to be noted, were put down as—e.g., $\frac{1}{2}$, this meaning half the angle subtended by the periphery of the normal field with a line drawn from the eye investigated to the investigator's nose; the investigator depending upon his experience with screen work to estimate this with sufficient accuracy for this rough test.

(*c*) The media and the fundi were then examined.

(*d*) Candlelight tests in a barn at about 8 p.m. (1) The patient and the candle were placed at 10 metres from the types, 5 metres from each other. Under the conditions the patient should have just read the $\frac{6}{60}$ type. This was put down as a fraction $\frac{10}{10}$. If he could not do this, the light was slowly brought towards the types. The result was then expressed as a fraction:—

$$\frac{\text{Distance of light from types in metres}}{10}$$

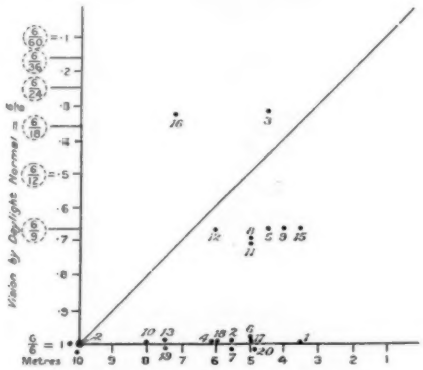
The light was then brought back to 10 metres and the patient moved slowly towards the types, the result being shown in a similar way:—

$$\frac{\text{Distance from types in metres}}{10}$$

(2) The field of vision was then taken, it being checked by a simultaneous observation with a normal eye, that of an officer or orderly.

(*e*) Open-air test, about 9 p.m., without illumination. The distance at which the $\frac{6}{60}$ type could be read by a normal man was first ascertained; this was shown as a fraction, e.g., $\frac{3}{60}$; the patient was then examined, and the result similarly shown.

As this symptom is very important, the details of the results are shown in Table III for the two groups, in Table IV for the controls. A group of controls was taken from among the other patients; they were chosen at haphazard except that they had no pains, gum affections or anæmia. The results are shown in Table IV. It will be seen that none of them had night-blindness. That sixteen out of twenty cases should have this, while none in the group of controls had it, raises a great probability that the affection from which the two groups comprising the twenty were suffering is a distinct clinical entity.



Reading by candle-light (60 M. type).

CHART II.

Comparison of vision by daylight and candle-light.

Charts were constructed to see if there were any correlation between the symptoms which could be mathematically expressed, such as percentage of hæmoglobin, the vision by night or candle-light, the field of vision, or between these and the months of service oversea, but there was no approach to a constant relationship. So that, not only was there irregularity in the number of symptoms shown by the patients, but the intensity of the symptom when present was irregular. It was obvious that the symptoms were of slight degree. Their varying relationship to the months of service abroad emphasizes the varying susceptibility of individuals to the absence of vitamines (*see* Charts I and II).

Diagnosis.—A reference to Tables I and II brings out the important point that anæmia, muscular or joint pains, bleeding gums, pyorrhœa and loose teeth are in themselves enough to raise a strong suspicion that scurvy is present, at least under conditions where vitamines are scanty, even if only for periods. This is important from a military point of view. It being found that the vision tests were exciting the curiosity of the patients, who were discussing these among themselves, it was thought advisable to discontinue them lest there should be complaints of night blindness arising. Although this symptom was invaluable in the early cases to establish the diagnosis, this could be made with fair accuracy later without its aid.

In August, 1915, the troops of the Division from which the cases were drawn were in the trenches for four to six days' spells, during which times they had no fresh meat nor vegetables. Out of the trenches they had an ample supply of these. Thus they were intermittently short of vitamines, hence the cases were of slight degree, few in number, and of erratic incidence, only very susceptible individuals being affected. No case occurred in those above the rank of Corporal, presumably because they had more variety in food. All the thirty-two cases but one investigated occurred in Infantry battalions, i.e., in units which had spells of trench duty. In only two cases had the men had less than five months' service in France; these two had no rheumatic pains, which were present in nearly all the other cases.

In the autumn of 1915 these cases practically ceased. On inquiry from officers of the Field Ambulance who did turns of duty in the trenches, it was found that the men were able to acquire fruit and vegetables locally; doubtless this accounted for the practical cessation of these mild cases of scurvy.

Shortly after this time strict orders were issued that fresh meat and vegetables were to be provided for men in the trenches. In the first half of 1916 a very few isolated cases were seen, after that there were none.

The practical bearing of these cases upon civilian work is obvious. Where there is a real lack of food in big cities there is the possibility of finding these mild cases of scurvy; it is claimed that the group of symptoms given will lead to the correct diagnosis. These cases are particularly likely to occur in women whose work is sedentary and done at home, and whose food is often scanty. To save time they omit to cook vegetables, and their supply of fresh meat being scanty, they fall back on bread and butter or jam, a diet very defective in antiscorbutic

TABLE III.—DETAILS OF EXAMINATION OF VISION.

	Serial number or letter of case	Daylight: Vision R.	Daylight: Vision L.	Daylight: Fields R. 1	Daylight: Fields L. 1	Candlelight, 8 P.M.: Patient moving 10 metres R.	Candlelight, 8 P.M.: Patient moving 10 metres L.	Candlelight, 8 P.M.: Light moving 10 metres R.	Candlelight, 8 P.M.: Light moving 10 metres L.	Fields R. 1	Fields L. 1	Open Air, 9 P.M. R.	Open Air, 9 P.M. L.	Open Air, 9 P.M. Normal	Disease or Injury
First Series	1					3½	3½	4	4			1½	1½	3 metres	
	2			1	1	10	10	10	10			2	2	2 ,,	
	3	<		—		—	4½	—	4½	—		—	1	2 ,,	Right eye : diffuse retino-choroidal atrophy, therefore not included
	4			1	1	5½	6	5	5	1	1	2	2	3 ,,	
	5			1	1	4½	4½	4½	4½	½	½	1	1	2 ,,	
	6			1	1	5	5	4½	5	< 1	< 1	1½	1½	3 ,,	
	7			1	1	5½	5½	4	3½		< 1	½	½	1 ,,	
	8			< 1	< 1	5	4½	3½	3½			½	½	1 ,,	
	9			½	½	4	3½	4	4			½	½	1 ,,	
	10			1	1	8	8	9	9	1	1	1	1½	1½ ,,	
	11			1	1	5	5	3	6	< 1	< 1	½	1	1½ ,,	
	12			1	1	5	7	4	6	< 1	< 1	1	1½	1½ ,,	
Second Series	13			1	1	8	7	5	4			3	3	4 ,,	
	14			< 1		6	5	5	3	1	1	2	2	4 ,,	
	15	(1)			—	4	3	4	4			2	1½	4 ,,	
	16	(1)			1	2	7	½	4			1½	2	4 ,,	Right eye probably amblyopic after strabismus
	17		—	< 1	—	5	—	6	—	½	—	2	—	2 ,,	Left eye not tested because of cellulitis left side of face
	18					6	6	4	4			1½	1½	2 ,,	
	19					8	8½	5½	5			2	2	2 ,,	
	20					5	5	5½	4			2	< 2	2 ,,	

TABLE IV.—CONTROLS (PATIENTS).

A			1	1	10	10	10	10	1	1	4	4	3	metres	Metatarsalgia
B			1	1	10	10	10	10	1	1	3	3	3	,,	Sprained ankle
C			1	1	10	10	10	10	1	1	4	4	3	,,	Sprained ankle
D			1	1	10	10	10	10	1	1			1	,,	Boil on neck
E			1	1	9	9	8	8	1	1			1	,,	Influenza
F			< 1	< 1	8	9	7	6	< 1	< 1	1	1	1	,,	Influenza
G			1	1	10	10	10	10	1	1	1	1	1	,,	Malaria

< 1 = Slightly contracted fields.

vitamines, and manifestations of scurvy will be liable to appear. In the country districts want of food, if it occur, is unlikely to cause scurvy, because there potatoes nearly always form an important part of a limited diet.

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Scurvy.

Very little bearing upon the symptoms of minor degrees of scurvy has been found in the literature accessible to us; the accounts of this disease deal almost entirely with the symptoms in well-marked cases.

In Fagge's "Principles and Practice of Medicine," 1886, ii, p. 584, is a short, excellent epitome, citing unhealthy gums, slight petechial eruption upon the legs, rheumatic pains and sallow complexion, as the symptoms in slight cases.

In his contribution to the "Discussion on the Treatment and Management of Diseases due to Dietetic Deficiencies,"[1] at the Section of Therapeutics and Pharmacology, reported in the *British Medical Journal* of January 17, 1920, Dr. W. H. Willcox refers to the condition of Indian troops arriving in Mesopotamia in 1916, which contained "a considerable percentage of men anæmic, debilitated, and suffering from pyorrhœa." They were not actually suffering from scurvy, but they were in a condition which strongly predisposed them to it. We suggest that these men did have scurvy in a slight degree: certainly the symptoms in the quotation above are two of those given in this paper for the diagnosis of this condition. And this will explain how large an amount of the ordinary type of scurvy occurred later among these troops; they started with a debtor balance, not with a merely diminished credit balance—to use Dr. Willcox's simile.

Night Blindness.

The references to this in literature are fairly abundant, but, as far as we have had access to them, deal with severe degrees of this symptom.

Bamfield, in the *Medico-Chirurgical Transactions* (Lond., 1814), includes night-blindness from scurvy among the varieties of hemeralopia.

In the *Royal London Ophthalmic Hospital Reports* (1859, ii, p. 40) an article by Bryson on "Hemeralopia in Scurvy," founded upon his experiences in the Navy, is preceded by a *précis* of the experiences of army surgeons in the Crimean War upon this symptom. From these and from Bryson's paper it is obvious that, as a symptom of scurvy, it occurred in three types: (1) As an early or initial symptom of scurvy; (2) as the main symptom, the other symptoms being slighter than usual, and (3) in scurvy in which the ordinary symptoms were dominant. No statement is made as to the proportional incidence of night-blindness in scurvy, so that no guidance is given as to what might be expected in slight cases of this disease.

[1] *Proceedings*, 1920, xiii (Sect. Therap. and Pharm.), p. 7.

Some Deficiency Diseases and Leprosy.[1]

By A. Stayt Dutton, L.R.C.P.Lond., M.R.C.S Eng.

In the following remarks matters concerning the causation and prevention of beri-beri, scurvy and leprosy are discussed :—

Beri-beri.

Owing to the numerous and accurate observations which have been made in regard to beri-beri, it is now generally recognized to be one of the deficiency diseases. It is found largely among those who subsist mainly on rice from which the pericarp and aleurone have been removed, while recovery may result when unpolished rice is substituted for polished. A diet chiefly composed of white bread can also induce the affection, although Sir F. Taylor says this does not often occur, as it is rarely or never the only food [1]. This circumstance seems to afford a clue to its causation, as it shows at any rate that it may be induced by more than one unsatisfactory kind of diet. Numerous investigations have lately been made in regard to so-called vitamines, but the characteristics and chemical composition of any such bodies do not appear to be at present determined. Whatever the particular defect in dietary may be, beri-beri is comparatively infrequent in the more highly civilized countries.

Dr. W. H. Willcox noted during the recent campaign in Mesopotamia that it occurred in British troops whose diet mainly consisted of bully beef and white flour, and that it did not occur among Indian troops who lived on whole cereal flour and different pulses [2]. The difficulties that were encountered in supplying these troops with sufficient variety, and particularly of fresh foods, can be readily realized. Something in foods, whether vitamines or whatever it may later be found exactly to be, is lacking in some degree in tinned meats and probably also in dried vegetable products. Although the whole wheat berry and unpolished rice may be needed to supply effective nutrition when either of these is almost or quite the only food, it appears to be unnecessary to burden the digestive organs with husks and other material which are

[1] At a meeting of the Section, held February 24, 1920.

not easily or wholly digested, if various foods contain similar needed nutrient matter. The bleaching of flour, which in the opinion of many observers unfavourably affects necessary constituents, seems especially likely to destroy delicately constructed substances of which anything in the nature of vitamines is composed, owing to the methods which are employed to effect it.

Scurvy.

It has been held for a considerable time that scurvy can be induced by a deficiency in needed constituents that are contained in different vegetable products, and the provision of fresh lime juice has been found to prevent as well as relieve it on many occasions. Willcox says it occurred in the Army in Mesopotamia among Indian troops, but not in the British, and attributes this to the diet of the Indians being lacking in anti-scorbutic vitamines [3].

Leprosy.

This disease is prevalent in many parts of the world, and has been so for an unknown and incalculable period of time. There are considered to be records of its prevalence in Egypt and the East for thousands of years. England has experienced its ravages on many occasions, although it has not been seen in this country for over a hundred years. Sir Jonathan Hutchinson made considerable effort to ascertain the cause of leprosy, and travelled a good deal in different countries for this purpose. He appears to have been convinced that the food supply has considerable effect in its appearance, and notes that it never developed in the United States, with the exception of parts of California, attributing this to the circumstance that it is almost the only country where colonization took place without an initial stage of considerable hardships, and that a good variety in foods was practically always available [4]. In holding this opinion I do not think the judgment of Hutchinson is likely to have been at fault.

The study of the affection from an ætiological point of view presents a number of especially interesting features. It is considered to be due to the *Bacillus lepræ* discovered by Hansen in 1871. It is remarkable how closely the nervous symptoms of beri-beri resemble those of lepra anæsthetica. The numbness, anæsthesia, loss of power in the legs, arms and hands are very similar, as well as other symptoms, and may in both diseases result in muscular atrophy, with foot-drop and wrist-drop. Each is also particularly prevalent in the Far East. Lepra

anæsthetica can occur without association with lepra tuberculosa, and be present for a considerable time before the latter may eventually make its appearance, but Hutchinson says the tuberculous variety never occurs before the anæsthetic kind is in evidence. This appears to support the food theory.

I do not suggest that leprosy is caused by the taking in of bacilli in fish, but there seems to be the likelihood that when the food supply mainly consists of it a deficiency in dietary occurs. Friedenwald and Ruhräh say salting modifies the flavour of fish [5]. According to Liebig one-third of the nutritive value of meat is lost by extraction during the process, and it may then be reasonably expected to have some such unfavourable effect on fish [6]. Leprosy is common in Iceland and on the west coast of Norway, and Hutchinson says it more generally prevails on the sea coast, or near lakes and rivers, where fish is largely or mainly consumed. On looking into the composition of fish, it is seen that none of them contain carbohydrates with the exception of shell fish [7]. The obtaining of a sufficiently liberal supply of the last-named fish would not be possible in some districts, but even if it were the amount of carbohydrates they contain might not be sufficient to maintain the proper balance in constituents. It seems to be the case that a diet chiefly composed of fish is unsuitable, perhaps especially so when salted, and it is likely that leprosy may eventually be recognized as partly a deficiency disease, sometimes owing to the predisposition caused by a diet mainly of fish, and at times to another dietary which is deficient in various necessary elements.

That leprosy is contagious has been a matter of belief for centuries, people having been accustomed to avoid the society of lepers and to banish them to out-of-the-way places, but although this is so, the theory of contagion is not generally accepted. Sanitation has advanced with rapid strides in different countries in comparatively recent years, but is well known to have been highly imperfect in ancient times, and even now is still little in evidence among some nations in comparison with that which is the case in Britain and elsewhere. That the attendants at the Hendela Leper Asylum in Ceylon do not contract it, is said to be unfavourable to contagion theories. On the other hand, Father Damien de Vensta, who left Belgium to go to a leper asylum in the Sandwich Islands in 1873, was observed to be suffering from leprosy in 1882, and died from it in 1889. Precautions as to person and clothing are usually some of the first things in which nurses and

hospital attendants are instructed, but a priest being comparatively unversed in medical affairs might tend to neglect them to a certain extent, while attendants are not likely to do so. He may also have been less inclined to be careful in dietary and environmental conditions, while the nature of his calling may have contributed to possible infection. That this seems to have been the case can be gathered from Edward Clifford's "Father Damien," in which is described how the latter often lived in a polluted atmosphere, washed the bodies, dressed the sores, visited the deathbeds of those who were suffering, and even digged their graves. His case appears to favour contagion.

Tubercle bacilli show a considerable resemblance to those of leprosy, but exactly by what agencies the tubercle bacilli enter the body is in all cases of course not wholly determined, as for instance in regard to the effects of a spray of secretion from the mouth of another, and of tuberculous meat. It may be, and it is I think likely, that the bacilli of leprosy also enter the system in various ways. That the reason of the occurrence, apart from bacilli and mode of infection, differs in the main in the two diseases is evident, as tubercle is still prevalent in all countries while leprosy is not. Leprosy is not considered to be hereditary, but is said to resemble tuberculosis in a possible predisposition [8]. The bacilli have not been found in earth, dust, water, food or air. They increase greatly and are highly infective, and it is stated that few diseases show equal infection [9]. The bacilli leave the body by the nasal, salivary, lachrymal, mammary, seminal, or pulmonary secretions, and the urinary and intestinal excretions [10].

Owing to the overcrowding, ineffective housing, and other unfavourable environment in eastern countries and elsewhere, the opportunities of contagion appear to be manifest, especially in districts far removed from modern methods. Sanitation and hygiene are quite ineffective in different parts where leprosy is prevalent, in the abodes which are largely one-storey wooden or mud erections, often pervious to rain and dust, all around them and in the streets, while public places, as inns, are also said to be particularly uncleanly and insanitary. Ablutions of person and clothing are often almost unknown; lepers sit about in doorways or in the streets, and are stated to believe that if they can communicate the disease to another their own infliction will be lessened or relieved [13].

Latrines and urinals may not be cleanly or satisfactory in position or may be used in common by many in out-of-door places, and the bacilli may enter the body of a person predisposed to infection by the urethral

or rectal orifices. The bacilli may be communicated from one person to another by the use of the same towels, handkerchiefs, or any material used for similar purposes; also by the nasal, pulmonary, or seminal secretions, and possibly by contact with those wearing sweat and skin-scale impregnated garments. Clothing of the feet and legs may be non-existent or imperfect, so that cuts and abrasions of the lower extremities would be likely to afford a ready entry for bacilli from excretions under generally insanitary conditions. Father Damien was scalded on the foot when he was known to have anæsthetic leprosy and later was told he had more serious complications [14]. The natives of Ceylon have attributed the onset to the bite of a rat [12], and such wounds may admit infection. Women are said to be less prone to the disease than men, and this may possibly be attributable to their having more regard for sanitary methods in certain respects. The question of the entry of bacilli through the medium of blood-sucking insects has been investigated to a considerable extent with negative results for the most part. If this were the usual mode of infection it is probable that the matter would have been already determined on the basis of knowledge now forthcoming in regard to other insect-borne diseases. But it may be possible that insects on occasions transfer bacilli on their exteriors from persons affected, or excreta to another having wounds or abrasions of the skin. There appear to be a number of ways coming under the head of contagion in which the bacilli may gain access to the system.

Hutchinson was of opinion for a time after the discovery of the bacillus of leprosy that the matter of causation required no further investigation, but later became aware that this was not the case. His death occurred in 1913, when the knowledge of deficiency diseases was not so far advanced as is at present the case, but owing to his efforts and those of numbers of other investigators, it now seems possible to arrive at conclusions more likely to be acceptable—namely, that leprosy is largely predisposed to by ineffective feeding, also by defective personal hygiene, sanitation, housing, and general unhygienic environment; the bacilli gain access to the body in manners such as described, with the possible subsequent addition of the whole chain of tuberculous manifestations and disabilities.

The question of prevention seems to resolve itself into the need of conforming to customs and habits found efficient in England and elsewhere, particularly in regard to food, and other effective predisposing agencies, with, in addition, the isolation of those infected with the

Bacillus lepræ. Needless to say, prevention is far from being a purely medical question, as matters relating to a number of other affairs are involved, and while housing and sanitation require attention in this country, the need of such is almost incomparable in degree with that in different oversea, eastern and other lands. With effectual organization, determination, will power, man power, and other necessary factors set actively in motion, it is in my belief possible that matters could shortly be greatly improved, and with output of sufficient energy, in a decade or two the devastating disease become almost, if not entirely, eradicated from countries where it is now prevalent, and relegated to the position of a disease of the past as has been the case in this country and in others.

On Dietary.

Although I am not acquainted with the habits in regard to diet of all kinds of birds and beasts, from observation of those in England, there seem to be none in their free state that confine themselves mainly to one kind of food. The early bird, after catching and disposing of the worm, continues throughout most of the day to seek for and obtain a good variety of different cereals, or other vegetable or animal matter, which quickly disappear into its crop; the domestic fowl will also actively pursue a similar course if it gets its own way sufficiently; while a farmer has too much common sense both from the needs of the case and considerations of his pocket to attempt to prepare well-fed beasts for the meat market by sustaining them on any but a good, wholesome, and varied dietary. The instincts of dumb creatures thus appear to afford indications to man in respect to feeding and the maintenance of health.

It seems to be evident that mankind partakes of foods both for enjoyment and sustenance, and one can hardly imagine practically any members of the white races in either social class anticipating with particular pleasure the ingestion of three meals a day consisting chiefly of rice, fish, or any one kind of food stuff from one year's end to another; or expecting to rise from the table after such meals with the sense of satisfaction they are accustomed to experience after meals consisting of a suitable and properly moderated variety of foods. Milk is generally considered the most perfect in its chemical composition, but requires to be supplemented at an early age by other foods if nutrition is to be continued according to the requirements of modern highly civilized races. Large numbers of persons in far eastern overseas countries and

elsewhere do not conform to the customs of modernized nations in regard to dietary. What the reason of this may be is not my purpose to attempt to discuss closely, but possibly it is largely owing to centuries-long habits and customs which tend to cling and are not easily cast off. It may also be that the failure fully to appreciate the value of advances in other nations, or unwillingness to bend the back sufficiently in honest toil, and cause the brow to sweat in turning over the soil, has something to do with it. The moral of these diseases as far as food supply is concerned appears to be that a mixed diet of fresh and wholesome foods is best, and that if such is not available, no matter what the cause of the defect may be, troubles in regard to health are liable to make their appearance. No one can deviate from the principles of health with safety, this being brought under the notice of most practitioners daily and often more than once in the twenty-four hours.

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Section of Medicine.

President—Dr. A. F. Voelcker.

Typhus and Relapsing Fever in Mesopotamia and Northern Persia.[1]

By W. H. Willcox, C.B., C.M.G., M.D.

Consulting Physician, Mesopotamian Expeditionary Force, 1916-19.

The term "typhus fever" is still used by native practitioners in Mesopotamia and Persia to designate both typhoid and typhus fevers and often little attention is paid to the differentiation of the two diseases. Typhoid fever is known as typhus abdominalis in contradistinction to typhus which is known as typhus exanthematicus. These terms are probably the result of German teaching, for they are still used in that country. The terms "jail fever," "ship fever," "hospital fever," "camp fever," "spotted fever," have also been applied to typhus in English-speaking countries.

Relapsing fever was commonly described in Mesopotamia by native practitioners as recurrent fever. It also appears to have been called chronic fever by the Turks. It has been known in Europe by the names famine fever, seven day fever, and in Africa as tick fever.

Ætiology.

In Mesopotamia and Northern Persia typhus and relapsing fever are endemic and amongst native population have probably a greater incidence than enteric. Typhus fever was non-existent among our troops until their arrival in Mesopotamia and all cases could be directly traced to infection from the local population or Turkish prisoners. All the

[1] At a meeting of the Section, held January 27, 1920.

evidence went to show that the main channel of infection was the body louse. Those suffering from epidemics of typhus and relapsing fever, namely, Arabs, Persians, refugees or Turkish prisoners, were invariably heavily infested with lice. In most cases in which our troops were attacked by typhus the cause could clearly be traced to personal contact with lice-infested persons of the above classes and lice would usually be found on their clothing on admission to hospital.

Those who contracted typhus fever in the Mesopotamian Expeditionary Force were medical officers and personnel of units in which patients suffering from typhus fever were being treated; those in charge of Native Labour Corps (Arabs, Persian, Kurdish) who were usually British officers and N.C.O.'s; the motor transport drivers who carried sick, the Military Police, who were constantly brought into contact with the native population; members of the Political Department, and frequently men who were brought into contact with Turkish prisoners, as guards, or travelling with them on river steamers. Indeed, it may be said that anyone brought into close contact with native Arabs, Kurds or Persians, or the refugee classes in Mesopotamia, or Turkish prisoners ran a distinct risk of contracting typhus or relapsing fever by the agency of infected lice. All of these classes of persons could be regarded as actual or potential carriers of the two diseases. Since so many of them were immune through having suffered from these diseases it was a common occurrence for apparently healthy natives to carry infection by means of infected lice on their bodies. If special care were taken to prevent the access of lice by means of the wearing of overalls tightly fitting round the wrists, and of top boots or breeches and leggings the risk would be fairly effectually avoided.

There was distinct evidence from Mesopotamia that the lice on natives suffering from typhus or relapsing fever might carry the infection of both the diseases; thus on February 17, 1918, a driver of a motor ambulance convoy was taken ill with typhus fever exactly twelve days after carrying in his car from Bakubah to Baghdad two Arab patients suffering from relapsing fever.

Again, on March 6, 1918, a medical officer was taken ill with relapsing fever exactly six days after attending two cases of typhus fever. It seems likely that a native Arab may carry on his person lice, some being infected with typhus and others with relapsing fever. If individual lice were infected with both diseases the patient who was inoculated by them would presumably develop both concurrently, but this was very uncommon.

A very interesting example of an experimental inoculation of typhus fever by a louse bite occurred in the case of a medical officer who in the course of an experimental investigation was bitten by a louse on March 26, 1918, and possibly accidentally on March 27. On April 5, he developed a typical attack of typhus fever (Chart I).

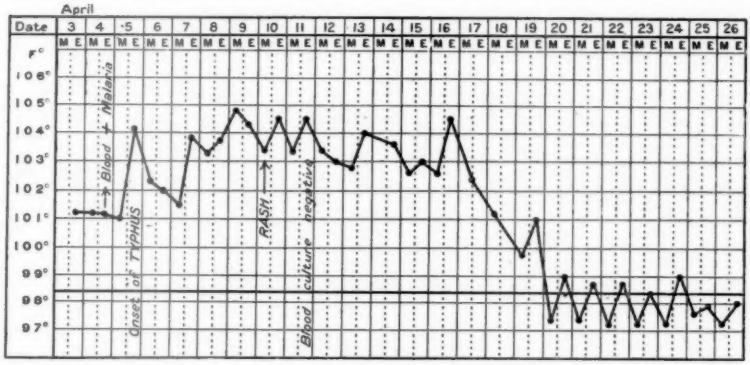


CHART I.

Typhus fever (louse bite, March 26, 1918).

CLIMATIC CONDITIONS, OVERCROWDING, FILTH, FAMINE, DISTRESS.

All of these ætiological factors played a part in as far as they contributed to the spread of lice amongst those affected. In Mesopotamia, owing to the great heat, lice would not be prevalent during the hot months July and August and would begin to disappear in June. This was partly due to the direct action of the sun's rays, and partly, no doubt, to the more frequent washing of the body and clothing in the summer months. As the cold weather came on lice would begin to be common; this would be in November and December but it was usually some weeks after this that typhus and relapsing fever would appear. It seemed to take some time for the lice to become infected, thus in Mesopotamia we found typhus and relapsing fever commencing usually about February and March and the disease would rapidly decline in June (*vide* Charts II, III, IV, V). In Northern Persia, on the other hand, where even in the summer the nights are cold, typhus and relapsing fever were present all the year round.

Northern Persia had been much affected by the war. In 1914 and

1915 the Turks were in occupation there and they retired before the advancing Russian troops who occupied Northern Persia in 1916 and 1917. The local population in consequence of the using up of the local food supplies by the invading armies suffered terribly from famine and many of their primitive dwellings were wantonly destroyed. They were indeed in a desperate state. The appalling conditions led to their clothing being of the filthiest and scantiest description and to an

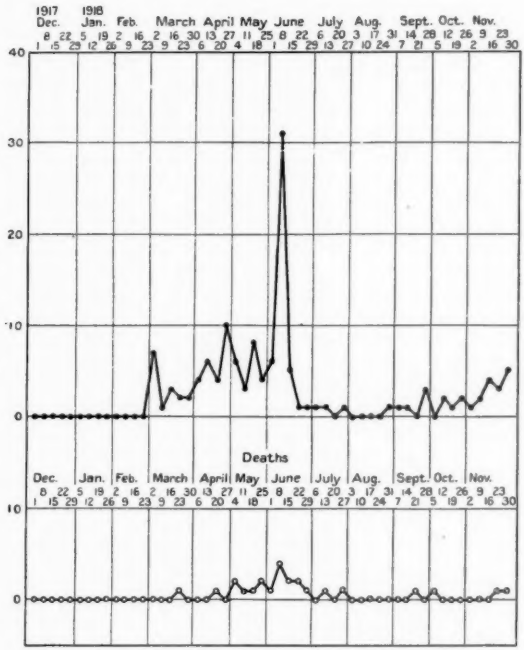


CHART II.

Typhus fever (British), Mesopotamia, return of cases per week.

extreme degree of starvation. The famished inhabitants were crowded together in the rudest of dwellings and under most insanitary conditions; they thus became very heavily infested with lice and in consequence of this typhus and relapsing fever were rampant amongst them. Indeed, during 1918, typhus and relapsing fever were extremely common amongst the inhabitants of Northern Persia, and several of our men who by their military duties were brought into contact with the local population contracted these diseases.

In the latter half of 1918 some 70,000 refugees from the district round Lake Urmia and Lake Van were driven by the Turks to seek refuge in Mesopotamia and they travelled down this great distance

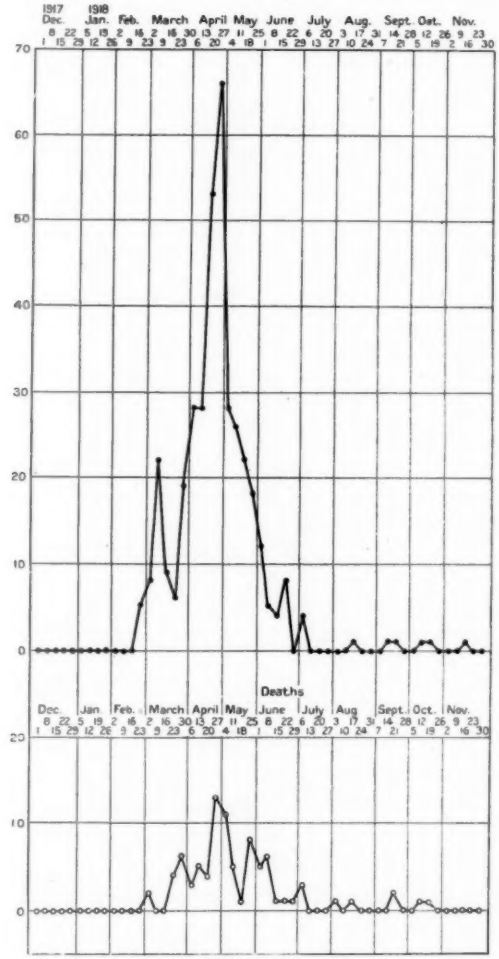


CHART III.

Typhus fever (Indians), Mesopotamia, returns per week.

(some 500 miles) with their families and household goods under conditions which it is difficult to describe but can perhaps be imagined. The remarks about the inhabitants of Northern Persia applied to some

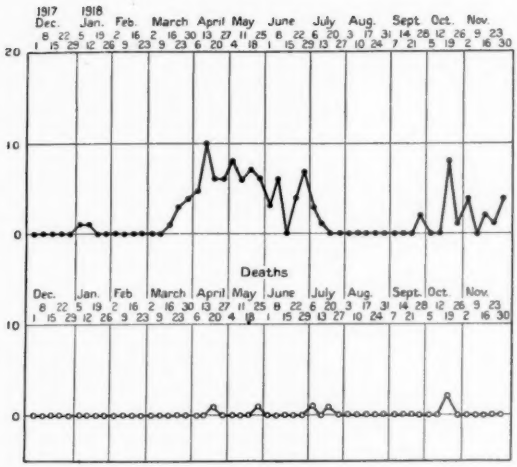


CHART IV.

Relapsing fever (British), Mesopotamia, returns per week

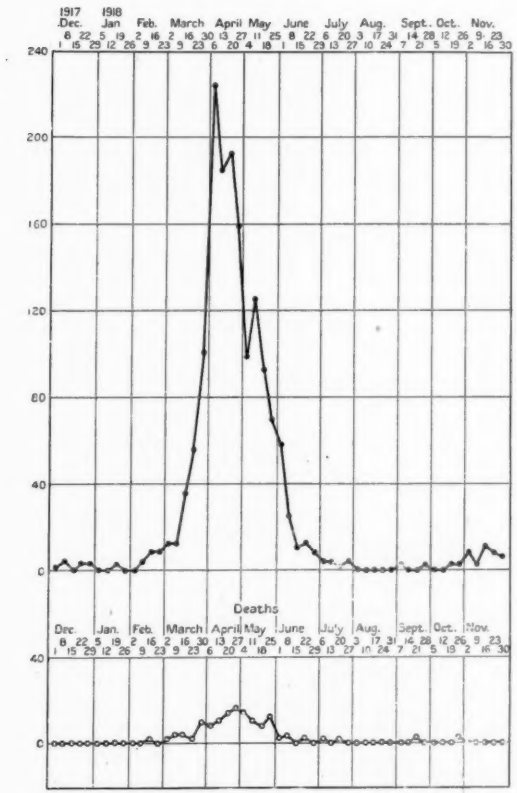


CHART V.

Relapsing fever (Indian), returns per week.

extent to the refugees so that many of them were heavily louse-infected and as a consequence typhus and relapsing fever cases occurred in considerable numbers.

Other Channels of Infection.

In the case of typhus fever it appeared from the evidence in Mesopotamia that the cause of the disease was almost invariably an inoculation by an infected louse. No doubt a patient suffering from typhus fever who has been effectively de-loused is still a danger of infection, the blood and excreta probably containing the virus; and unless care be taken infection may spread to neighbouring patients or those in attendance. A few cases occurred amongst medical personnel in charge of typhus patients where great precautions had been taken to prevent the access of lice to their bodies, but these may have been inadequate. I cannot recall a single case in which typhus fever had spread from one patient to another in a ward where the agency of the louse could be excluded.

In the case of relapsing fever I think that the danger of infection via breath, excreta, blood, &c., from a patient suffering from acute symptoms and who has been effectively de-loused is greater. Several cases came to my notice in which those nursing patients suffering from relapsing fever contracted the disease in spite of every precaution as regards danger from lice, and I met with two cases in which bacteriologists developed the disease after taking blood films from cases of relapsing fever, the usual incubation period intervening. In the case of a medical officer in Mesopotamia who accidentally got blood on to his hand from a patient the incubation period was five days. This medical officer had some abrasions on his hands. It may be possible for the spirochæte of relapsing fever to gain entry through the unbroken skin, just as is the case with the spirochæte of hæmorrhagic jaundice.

Carriers of Infection other than the Louse.

In Northern Persia a fever of the relapsing type is well known at "Mianeh," and is called Mianeh fever. A large tick known as the "Mianeh bug" is prevalent here, and this animal seems to have a predilection for strangers, but is said not to attack the native population. A fever appears to follow the bite of this, commencing after a short period of a few days, and is of the relapsing type. I have seen

charts of this fever, and the period of pyrexia appears rather longer than that of ordinary relapsing fever, and the attacks perhaps more severe. No opportunity occurred to me of seeing a patient suffering from the disease, and no blood film examinations were made in cases as far as I am aware, so that the causal agent has yet to be satisfactorily demonstrated. It is probably a spirochæte. A paper on "Tick Fever in Palestine," by Dr. F. D. Nicholson, late Captain R.A.M.C., is very interesting in this respect.[1] In this case a spirochæte was demonstrated.

Association of Typhus and Relapsing Fever.

There is a close association between these diseases, as would be expected from the common channel of infection, namely, the louse. Wherever cases of one appeared within a short time, cases of the other

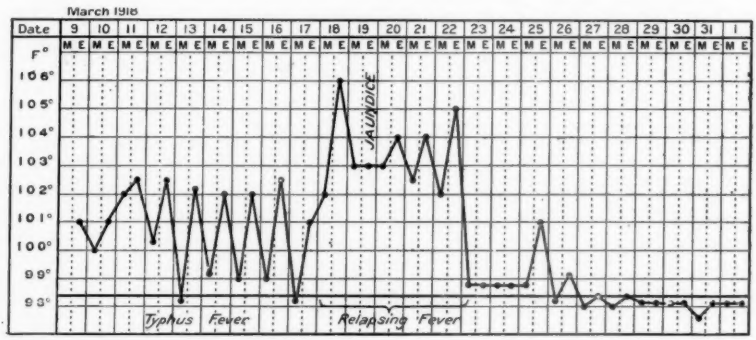


Chart VI.

Typhus and relapsing fever (Arab).

disease would be almost certain to follow. In the Serbian epidemic, so fully described by Colonel Hunter, it was pointed out that at its height cases of the two diseases were grouped indiscriminately together into shelters, and little attention was paid to their differentiation by the local population. Careful medical examination was needed in order to differentiate the two types of illness.

In the epidemics amongst refugees in Mesopotamia one often found grouped together sick persons suffering from acute illness, some being relapsing fever and others typhus (*see* Chart VI).

[1] *Brit. Med. Journ.*, 1919, ii, p. 811.

Occurrence of Outbreaks.

In the Dardanelles relapsing fever occurred amongst some of the Indian troops, and it is interesting to note that a few of these cases were evacuated as acute mania owing to the sudden onset of delirious symptoms. These cases were found to be relapsing fever when opportunity for blood examination occurred.

After the evacuation of Gallipoli the troops of the Thirteenth Division were temporarily stationed at Mudros, and in January, 1915, I saw a few cases of fever of obscure type in men of the Gloucesters and Staffords battalions. These were diagnosed by me as relapsing fever on clinical grounds, and spirochætes were afterwards found in the blood. The units affected had been encamped near to Indian troops, who had suffered from relapsing fever.

When the Thirteenth Division was transferred to Mesopotamia early in 1916, a few cases of relapsing fever occurred in the above named battalions. This infection was no doubt brought from the Dardanelles, and was not due to infection from the ship which conveyed the troops, as was, I believe, suggested. The disease was quickly arrested by the sanitary precautions taken, but I saw several cases of typical relapsing fever in these units after their arrival in Mesopotamia.

Early in 1916 a few cases of typhus fever occurred at Busrah amongst the Turkish prisoners, and these were treated at the American Mission Hospital. The doctor in charge and his wife both contracted the disease, and the latter unfortunately died of it. I saw the notes and charts of some of these patients, and there was no doubt as to the diagnosis of typhus.

Typhus and relapsing fever did not occur to any extent at Busrah and Amara among the local population. It was in the Baghdad and Northern Persian area that the diseases were epidemic; thus, up to March, 1917, when Baghdad was captured, the local population was not a source of danger to the troops as regards typhus infection. Up to this time the only danger was from Turkish prisoners. In 1917, after the brilliant advance to Baghdad by General Maude, Turkish prisoners came down the line in large numbers, and many of them were, or had been, suffering from typhus and relapsing fever. The prisoners were responsible for a few cases of the diseases occurring amongst our troops, who had been brought into contact with them, but early diagnosis and careful isolation and disinfection prevented any spread of the disease.

On February 24 the Turkish hospital ship *Busrah* was captured by us; this was in an appalling condition, and many cases of typhus and relapsing fever were present on her, the patients being in a condition of dreadful neglect and filth. The medical arrangements and sanitation generally in the Turkish Army, judging from the conditions found by us on the hasty retreat of the Turks on numerous occasions, were of a most primitive description, and no attention appears to have been paid to cleanliness and antiseptic precautions generally. I cannot do better than read a most interesting account written by a Turkish doctor who was with the Turkish Army from 1914 to 1917, and afterwards served with us. He wrote:—

"*Exanthematic Typhus and Chronic Fever.*—Two faithful followers of the Turkish Army, itself a friend of lice. Were epidemiological proof lacking of the relation between lice and these two diseases, the Turkish Army at Kut during the winter of 1915 and 1916 would dispel any further doubt. The entire Sixth Army was plagued. Von der Goltz Pasha, who commanded it, died from typhus. The number of cases was enormous. More than 15,000 cases in Kut alone. Relatively the death roll was low; in Kut it was 8 per cent., in Baghdad 15 per cent. On the other hand, thanks to neo-salvarsan, deaths from 'chronic fever' were almost *nil*. When these two diseases assume such proportions, when, especially, lice invade an army like that at Kut, the hygiene specialist can do nothing. He must just let things take their course. Isolation of sick is impossible. Moreover, early diagnosis of typhus (that is before the fifth day), is difficult, especially when, from the start, the fever assumes an intermittent form like a malaria attack. One of the symptoms peculiar to the disease, the first to appear and the last to go, is considerable vertigo. Thanks to this symptom, I diagnosed the disease in my own case on the first day. But an entirely sure diagnosis of typhus is impossible previous to the fourth day: the patient is usually not isolated until the fourth or fifth day. I experimented with a vaccine advised by the bacteriologist of the Sixth Army. The results of 500 inoculations were inconclusive. In my own case, I developed typhus forty-five days after inoculation. The vaccination consisted of a subcutaneous injection of 5 c.c. of blood taken previous to the sixth day from a patient, and retained for an hour at a temperature of 55°. It is a clumsy form of vaccination causing great pain."

From June, 1917, typhus and relapsing fever practically disappeared from the force for several months; the reappearance was interesting. In February, 1918, very obscure cases of fever occurred in the personnel of the casualty clearing station at Bakubah, 35 miles north of Baghdad. I was asked to see these cases, and found the clinical symptoms of typhus. Colonel Ledingham, who saw the cases in consultation with

me, carried out bacteriological examinations, and confirmed the clinical diagnosis. All the patients affected had been in charge of sick Arabs and Kurds from local Labour Corps. On inquiry, it was found that the Arabs and Kurds of the Labour Corps were recruited from refugees wandering down from Northern Persia, and that there had been several mysterious deaths amongst these people. Post-mortem examinations had been made of some of them by Captain Ambler, I.M.S., who found signs consistent with those of typhus fever. It was thus evident that cases of typhus and relapsing fever were streaming down to Mesopotamia from Northern Persia. Steps were taken to deal with this by having quarantine stations at Khanikin and Bakubah, sixteen days' quarantine for all refugees being insisted upon and effective disinfection being carried out.

Some cases of typhus and relapsing fever occurred amongst the civil population of Baghdad in spite of the precautions taken, and in 1918 a number of cases spread to our troops, who were brought into close association with the native population. A very interesting outbreak of typhus fever occurred at Akab in April, 1918, at a combined Field Ambulance. Fourteen Indian patients were suffering from fever of an obscure type, eleven of whom were personnel of the Field Ambulance, and the others had been patients in hospital previous to their attack. I saw all of these cases in consultation with Lieutenant-Colonel Lloyd, I.M.S., on April 18, 1918, and there was no doubt as to the diagnosis of typhus fever. A typical rash was present in several of the cases. One of the patients had definite retraction of the head and meningeal symptoms, like the case that had been the cause of the outbreak. The cerebro-spinal fluid was found to be clear, and did not contain any organisms, and the tests—namely, negative blood culture and leucocytosis—confirmed the clinical diagnosis. An Arab Labour Corps was stationed at Akab, and had been free from typhus and relapsing fever. Towards the end of March, however, an Arab from the Bakubah area joined this corps, and on March 30 he became ill, and was admitted to hospital. He had marked cerebral symptoms, and was at first thought to be suffering from cerebro-spinal meningitis. He died on April 5, 1916, and there is no doubt that he was suffering from typhus fever.

An April 1 a violent thunderstorm visited Akab, and all the tents of the Field Ambulance were blown down, as a consequence of which the sick Arab was brought into contact with the other patients and personnel. There seems little doubt that in this way the infection spread to the patients and personnel of the Field Ambulance. The outbreak was quickly stamped out by effective disinfection.

I have alluded to the epidemics of typhus and relapsing fever in the refugees from the Lake Van and Urmia districts.

TABLE I.—MESOPOTAMIA: TYPHUS FEVER.

Cases.

	British	Indian
1917	12 ...	8
1918 (eleven months) ...	133 ...	377

Deaths.

1917	7 (58·3 per cent.) ...	4 (50 per cent.)
1918	24 (18·05 per cent.) ...	85 (22 55 per cent.)

TABLE II.—MESOPOTAMIA: RELAPSING FEVER.

Cases.

	British	Indian
1917	20 ...	196
1918 (eleven months) ...	110 ...	1,557

Deaths.

1917	— ...	8 (4·08 per cent.)
1918	6 (5·5 per cent.) ...	126 (8·09 per cent.)

SYMPTOMS OF TYPHUS FEVER.

The Incubation Period.—This is usually given as being twelve days but it can only be determined by knowing the date of inoculation by the infected louse. In the case above quoted it was ten days, and ten days is the period found by animal experiments.

The symptoms of the disease varied in severity according to the immunity of the persons affected; thus the native population often had the disease in such a mild form as scarcely to compel them to seek hospital treatment or to rest in bed, and in them the symptoms would not infrequently resemble those of a mild case of paratyphoid fever, except that the rash would probably be more profuse, though this was sometimes very slight. British and Indian patients usually had the disease in an acute form. The onset would be sudden, with the usual severe symptoms of an acute fever—e.g., headache, shivering, vomiting, pains in the limbs and back. The face would usually be flushed and congested and the conjunctivæ showed marked injection; the tongue would be furred and often dry, and tremulous in the first few days of illness. Epistaxis was a fairly common early symptom and a general mild bronchitis was usually present. The spleen almost always could

be felt to be enlarged and soft during the first few days. Delirium and marked mental torpor occurred early, the so-called "typhoid state" often being present in the first three or four days of the disease; indeed, at this time, the patient had many of the symptoms of a severe case of enteric fever in the third week of the disease. Deafness was a very marked and constant symptom and appeared to be of nervous origin, for it was unassociated with otitis media and often was not recovered

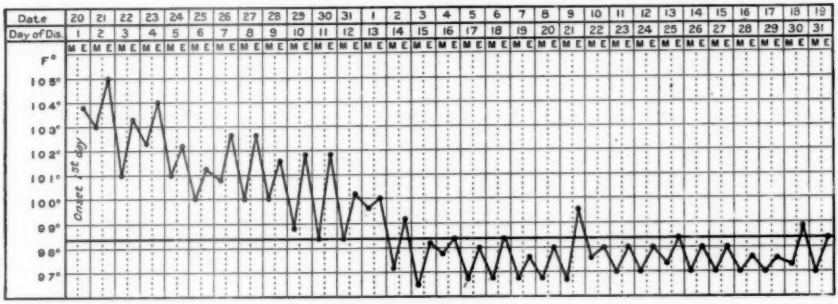


CHART VII.

Typhus fever (British).

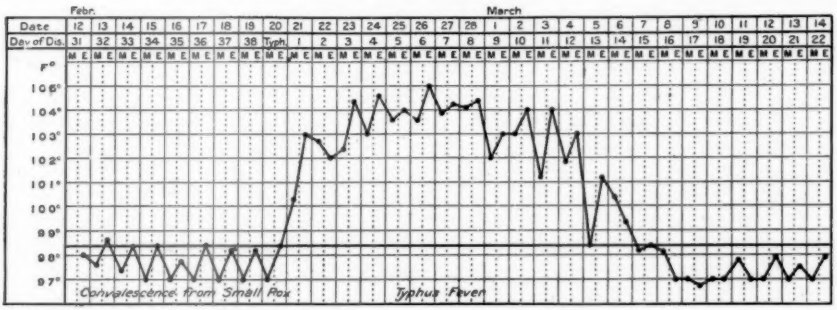


CHART VIII.

Small-pox and typhus fever (British).

from until some months after the fever had subsided. A few cases had tonsillitis as an early symptom. A peculiar offensive mousy smell was very often present. The pulse was usually rapid, 100 or more, and dicrotism was not marked (distinction from paratyphoid fever).

Temperature. *See* Charts VII, VIII, IX, X.—This would quickly rise to 103°, 104°, or more, and remain fairly steady, but there was frequently a drop of a few degrees for a few hours, followed by a rise causing the chart to show an appearance like a square root sign. The

temperature would begin to fall about the twelfth or thirteenth day, and usually became normal from the fourteenth to the sixteenth day. This was a valuable diagnostic sign of typhus fever, for an enteric case of like severity would probably run a temperature for a month or more. In the Mesopotamian cases the temperature did not fall by crisis but by fairly rapid lysis.

Nervous Symptoms.—The word typhus means a mist, and this expressed the dazed, confused mental condition of severe cases.

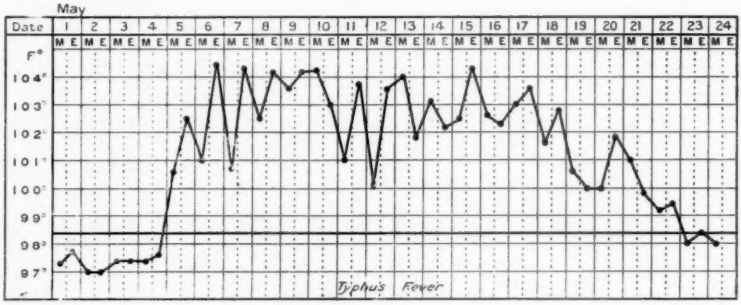


CHART IX.
Typhus fever (Indian).

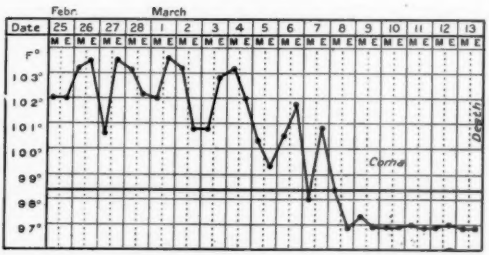


CHART X.
Typhus fever (British).

Delirium was common, and in some cases dreams of frightful nature occurred. Twitchings of the muscles, tremors of the tongue and stupor commonly occurred during the first week, and in severe cases the condition of coma would supervene sometimes as early as the sixth or seventh day, and this might last for many days. The knee-jerks were lost quite early, often about the fifth day—an important distinction from typhoid, paratyphoid, small-pox, relapsing fever, malaria, &c. It was a valuable diagnostic sign. Incontinence of urine

and fæces in severe cases occurred as early as the first week, and I have known incontinence of urine last for several weeks after the temperature had become normal. Symptoms of a profound effect on the cerebrum were very marked in the typhus cases, more so perhaps than in any other acute general disease, and after an attack of moderate severity the mental power would not be fully recovered for six months or more. Inability to concentrate, and to put forth mental effort lasts for a long time after the fever has subsided. Some cases presented marked retraction of the head and meningeal symptoms, the condition being due entirely to typhus fever, the cerebro-spinal fluid obtained on lumbar puncture showing no evidence of cerebro-spinal meningitis.

Rash.—No rash occurred on the face, there being only the general flushed congested appearance there. On the fifth day rose papules like enteric spots appeared on the chest, abdomen, and back, and extremities; these would fade on pressure during the first two days, but afterwards became dull red, not fading on pressure. By about the tenth day they became brownish red. Further groups of these spots did not occur. About the same time (fifth day) an eruption, consisting of blotchy red macules about the size of a threepenny-piece would usually appear and would be best seen round the shoulders, arms and axillæ, and would often extend over the back and front of the chest and abdomen and lower extremities; it might often be made more distinct by tying a bandage round the upper part of the arm, the macular eruption showing below the bandage. The macules behaved like the papules as regards their change in appearance: thus they would fade on pressure during the first day or so and afterwards become dull red, not fading on pressure, and by about the tenth day would have a brownish red appearance. *Purple petechiæ* occurred in a few of the very severe cases and sometimes also purpuric patches. These hæmorrhagic cases sometimes had hæmatemesis, melæna or hæmaturia. In Indians and Arabs very careful examination was necessary to demonstrate the rash, which was rather obscured by the natural pigmentation of the skin. It usually could be found on carefully examining the axillæ and back in a good light. I do not believe that there is a type of typhus without a rash: in my experience the rash was always present, but it was in some cases very slight and difficult to demonstrate. In typhus fever no rash appears on the palate: this served as an important distinction from variola and varicella.

Respiratory System.—Bronchitis was generally present, and in some of the severe cases broncho-pneumonia occurred.

Cardio-vascular System.—Cardiac dilatation was common and in severe cases the pulse became very rapid and feeble, cardiac failure occurring.

Venous Thrombosis.—This was fairly common and resembled that of enteric fever, the femoral vein being often affected.

Arterial Thrombosis.—This occurred in a few cases, and this was followed by the usual symptoms of gangrene. Amongst Turkish prisoners gangrene of the toes was seen in several cases; fortunately, however, arterial thrombosis was a rare complication amongst our troops. One British case was seen by me in which thrombosis of both radial and dorsalis pedis arteries occurred. This patient died on the tenth day before definite symptoms of gangrene could develop (*see* Chart XI).

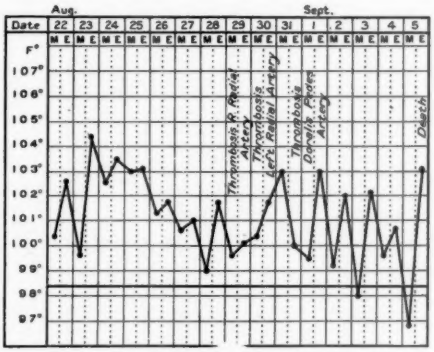


CHART XI.
Typhus fever with gangrene (British).

Abdominal Symptoms.—These were usually absent and served as a distinction from enteric group diseases. In some cases diarrhœa was present, and this was especially in patients who had been subjected to famine conditions. Some of the Persian refugee patients suffered from a loose bloody diarrhœa, the stools being negative to dysentery and not having the naked-eye appearance of that disease. This complication rendered the prognosis much more severe, and most of the cases were fatal.

Jaundice was a very rare complication and only seen in two cases. *Albuminuria* of a febrile type was usually present, but nephritis was only seen in one case. *Parotitis* was occasionally seen.

After the fever subsided the symptoms of profound prostration and cardiac weakness often remained for several days. In some cases the condition of coma with incontinence of urine and fæces lasted for a week or more after the temperature had become normal, and death sometimes occurred at this stage (*see* Chart X, p. 72.)

Diagnosis.—This was made from the clinical symptoms and also from the fact that blood cultures were negative to enteric group organisms, and blood films negative to malaria spirochætes. Leucocytosis also served to distinguish the disease from enteric. The "Weil-Felix" reaction was not used until December, 1918, and this was found to be of great help in diagnosis (*see* paper by Captain L. E. Napier, R.A.M.C., *Lancet*, November 15, 1919).

Differential Diagnosis.—From enteric fever, paratyphoid A and B, sand-fly fever, small-pox, relapsing fever, cerebro-spinal meningitis, measles, scarlet fever, influenza, septicæmia and uræmia. Before the appearance of the rash the diseases that perhaps most closely resembled typhus were sand-fly fever, specially if associated with effects of heat, and influenza complicated by malaria. Both of these conditions presented the characteristic facial appearance of typhus and the acute general symptoms. Blood examination and the course of the disease quickly served to differentiate them, however.

Treatment.—The general line of treatment was similar to that in cases of enteric. Hydrotherapy was important, cold sponging being done when the temperature exceeded 103°.

Saline Treatment.—This was found to be a most valuable remedy in the severe toxic cases. About the eighth day, or even earlier, normal saline was given by the rectum every four hours, and in the later stages subcutaneous and intravenous salines were given in the very severe cases. There was no doubt that the saline treatment saved many of the severe cases. In cases in which symptoms of venous thrombosis were threatening, normal saline containing ½ to 1 per cent. sodium citrate was given intravenously. It was found most important that patients should be protected from exposure to heat as far as possible, and during the convalescent stage a distinctly increased susceptibility to heatstroke was present (*see* "Heat Hyperpyrexia," by W. H. Willcox, *British Medical Journal*, March 20, 1920, p. 392).

It was found very important to continue the stimulant treatment in typhus cases—e.g., normal saline administration and cardiac stimulants for some days after the temperature had become normal, since during this stage there was great liability to sudden collapse.

Relapsing Fever.

Symptoms.—The incubation period was five to seven days usually, in one case of accidental inoculation from the blood of a patient it was five days. It is said to be two to twelve days, but obviously could only be determined by experimental inoculation from an infected louse. The onset is sudden, with headache, high temperature, sometimes vomiting and rigor, pains in the back and limbs, epigastric pain and tenderness, sweating. Epistaxis sometimes occurred; the face was flushed and the eyes congested.

Mental Symptoms.—In perhaps no disease did acute mental symptoms develop so early. Marked delirium occurred within twenty-four hours in severe cases, and was sometimes accompanied by hallucinations. The short periods of sleep were commonly associated with horrible dreams, in which murder and bloodshed played an important part.

The knee-jerk was not lost and coma only occurred in the very severe cases. The spleen was enlarged early. The liver was enlarged and jaundice commonly occurred about the second or third day.

Rash.—A general erythematic flushing sometimes occurred and some patients showed numerous marks on the skin due to louse bites, but no true rash was seen in this disease.

Herpes, parotitis and pneumonia occasionally occurred.

Captain M. A. Nicholson, I.M.S., O.C. of the Isolation Hospital, Baghdad, made a careful analysis of the symptoms in 241 cases amongst British, Indians and Arabs (1917 and 1918) and found headache in 5 per cent., vomiting in 9 per cent., mental symptoms in 29 per cent. which took the form of frightful dreams, marked enlargement of spleen in 70 per cent., jaundice in 64 per cent., and marked enlargement of liver in 41 per cent. These figures must only be taken as representing the type of the disease in Mesopotamia. Epidemics vary so much in severity that the percentage figures as regards occurrence of symptoms would vary with each epidemic.

Captain S. W. Page, R.A.M.C., of the Isolation Hospital, Baghdad, made a special study of severe cases of the cerebral type of relapsing fever. In these cases marked delirium and restlessness occurred, passing on into stupor and coma. The patient lay curled up in bed and Kernig's sign was present in some of the cases, also distinct abdominal tenderness and rigidity were present. Post-mortem examination showed pronounced congestion and hæmorrhagic oozing on the surface of the cerebrum, and spirochætes were found in the blood from the cerebral

vessels. There was no evidence of cerebro-spinal meningitis in these cases, and the symptoms were due to cerebral irritation caused by the intense spirochætal infection.

Temperature.—This would rise very suddenly to 103° or 104° F., and remain fairly steady for about six days and then fall by crisis, the patient suddenly becoming practically free from symptoms. After an interval of about eight days of apyrexia the temperature would again suddenly rise and remain up for about four days; usually it would then fall by crisis, and after another apyrexial period a third relapse might occur, this usually being very mild in character and lasting often only two or three days. Amongst British patients the apyrexial period was usually eight days, in Indians it was shorter, being usually six or seven days (*see* Charts XII, XIII, XIV).

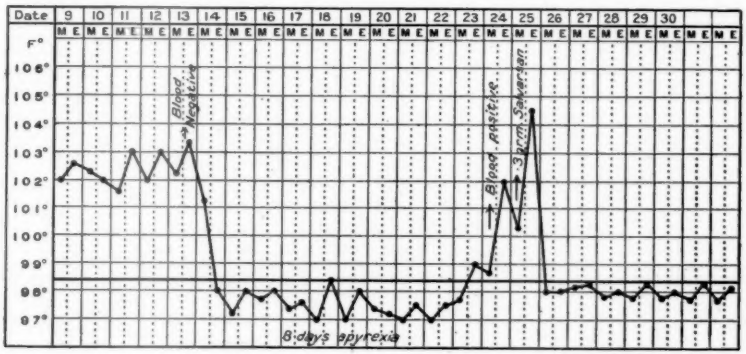


CHART XII.
Relapsing fever (British).

Mortality.—The disease was more severe amongst Indian patients; thus out of 44 British, 135 Indian, and 62 Arab patients, no deaths occurred amongst the British, 8 per cent. amongst the Indians, and 1 per cent. among the Arab patients.

Treatment.—The usual treatment for an acute febrile case was adopted. As soon as the diagnosis was established by blood examination, kharsivan 0·3 grm. was given intravenously; this often caused an additional rise of temperature of a degree or two, but within eighteen hours, usually by about twelve hours, the temperature fell to normal and remained so, the symptoms disappearing. The dose of kharsivan was at my advice kept at 0·3 grm., since this was safe. A larger dose

would have entailed risk from hyperpyrexia, which was a complication which always had to be carefully guarded against in Mesopotamia. The results were extremely good, since in only 5 per cent. of cases did relapses occur and they were of a very mild nature. 0·3 grm. was

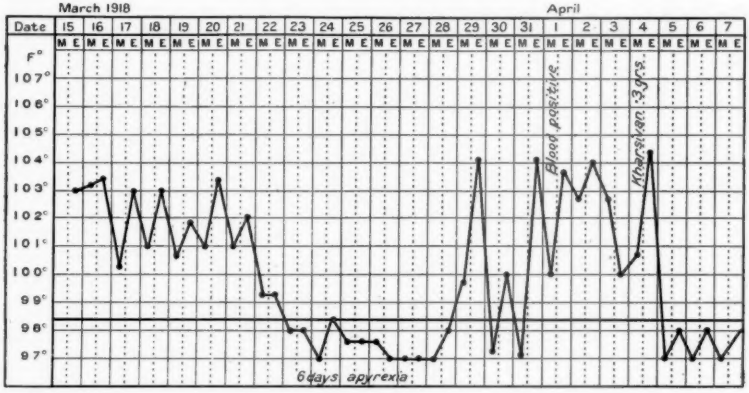


CHART XIII.
Relapsing fever (Indian).

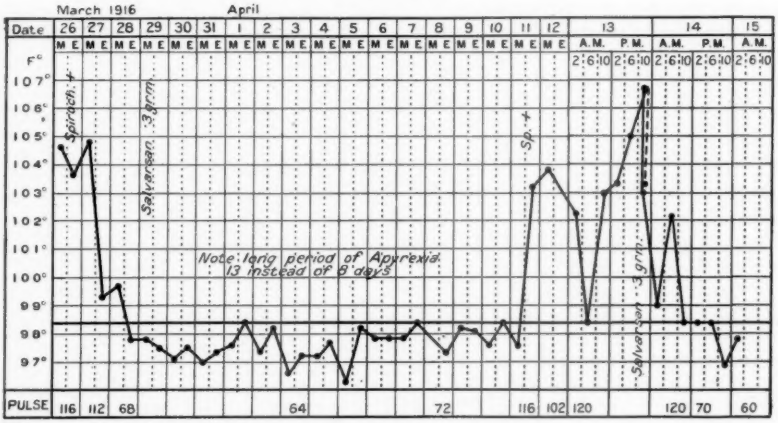


CHART XIV.
Relapsing fever (British).

the minimal dose which would give efficient results. Lieutenant-Colonel Leonard, I.M.S., tried the effect of smaller doses—e.g., 0·2 grm. kharsivan in several cases of relapsing fever, but found that with the smaller doses a much higher percentage of relapses occurred (*see* Chart

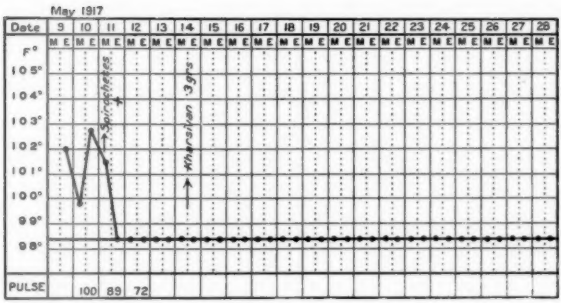


CHART XV.

Relapsing fever (Arab).

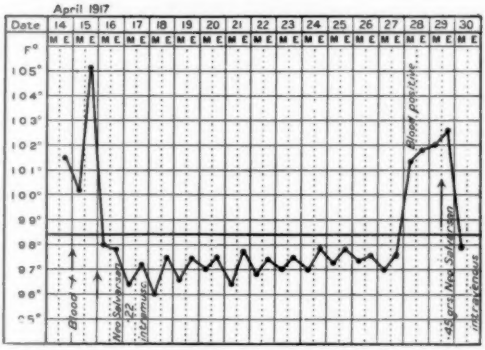


CHART XVI.

Relapsing fever (Indian).

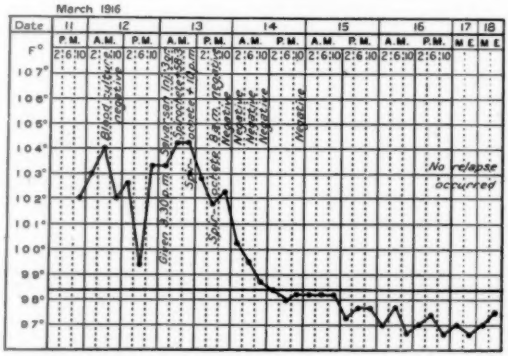


CHART XVII.

Relapsing fever; disappearance of spirochætes.

XVI). When a relapse occurred after the administration of kharsivan the apyrexial period was much prolonged, being often, instead of eight days, fourteen days or more (*see* Chart XIV). A larger dose of kharsivan than 0·3 grm. would have probably prevented the occurrence of relapses but the toxic action of the drug would no doubt have been dangerous and have caused some deaths (*see* Chart XV). In the apyrexial period following an attack the administration of kharsivan undoubtedly prevented the occurrence of relapses which were due to occur. After a cure by salvarsan, the patient is not immune, thus a Russian sister developed relapsing fever and was treated with kharsivan. Thirty-six days later she was again exposed to infection, and after a period of eight days developed the disease again (Bact. +, *see* Chart XVIII).

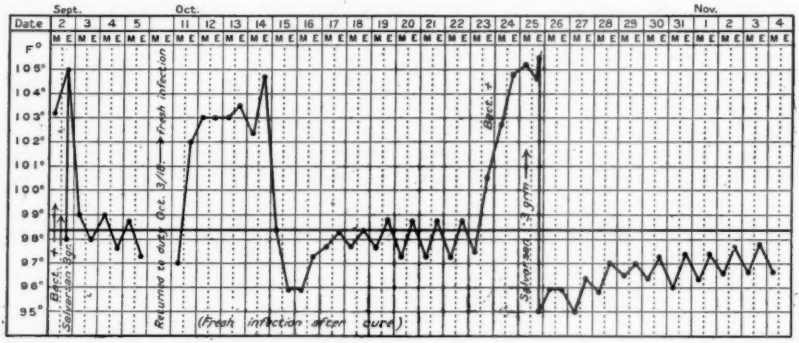


CHART XVIII.

Relapsing fever (double infection).

Bacteriological Diagnosis.—Blood films in the first two or three days of fever were usually positive, but in the later febrile stage were frequently negative. In the apyrexial period they were usually negative. A leucocytosis was usually present.

Prophylaxis.—The early diagnosis of typhus and relapsing fever was most important, since the earlier preventive measures were adopted the more sure would be the success following on them.

Quarantine and Segregation.—The quarantine and segregation of refugees who were probably carriers of the disease were important measures of prevention.

Louse Destruction Campaign.—The destruction of lice did away with possibility of infection. This was effected by means of various forms of steam disinfector, the best being the railway coach. Sulphur

in the form of the Clayton process was used, but was less reliable than the steam. The personal use of louse repellants, vermijelli, &c., was of value.

Isolation of cases and disinfection of contacts were essential.

Care of medical officers and hospital personnel against contamination with infected lice from patients was most important. This was effected by the wearing of overalls with tight fitting sleeves, top boots and caps and the free use of vermijelli and other louse repellants. In the case of Arab patients it was found safest to entrust their nursing to trained Arab personnel. It was very important to instruct the troops that the native population, refugees, or Turkish prisoners were possible carriers of the disease and that every precaution should be taken to prevent infection with lice from these people.

My thanks are due to Major-General Sir Francis Treherne, K.C.M.G., D.M.S., Mesopotamian Expeditionary Force, and to Major-General A. P. Blenkinsop, C.B., C.M.G., D.M.S., Mesopotamian Expeditionary Force for the statistics given, and for help and encouragement.

The Bacteriology of Typhus Fever.

By J. C. G. Ledingham, C.M.G., M.B.

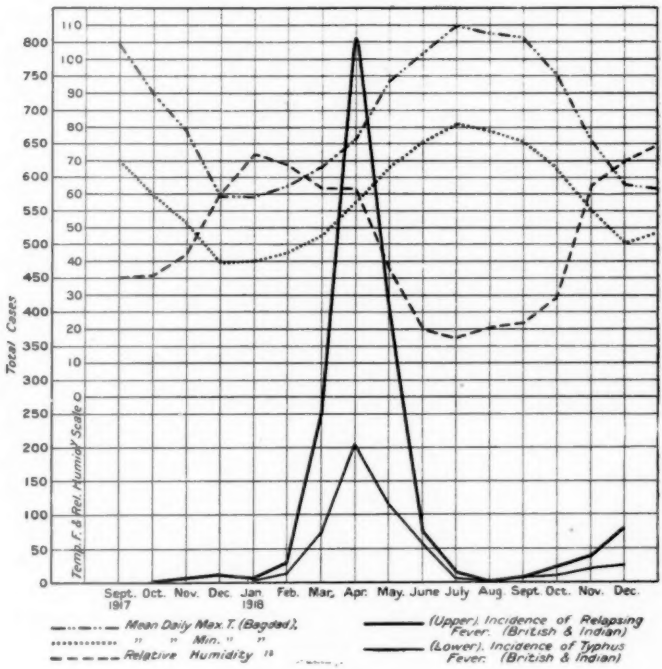
At the request of Dr. Willcox and the Secretary of the Section, I propose to deal briefly with the bacteriological side of the typhus problem especially in its bearings on the question of diagnosis, but before doing so I wish to show you a chart illustrating the course of the typhus and relapsing fever prevalences, month by month from October, 1917, onwards, in relation to the meteorological conditions peculiar to Mesopotamia (*see* p. 83). The correspondence in time and shape of the two curves of lice-borne diseases is most striking. Both diseases start from small beginnings in the last quarter of the year and both attain maximal prevalence in April, thereafter to fall abruptly to minimal or zero figures in the hot season of the Mesopotamian year. The volume of the epidemics is mainly Indian, the British factor forming but a small and insignificant portion. Thus of the 800 cases of relapsing fever in April only forty occurred in British troops, and similarly with the typhus outbreak which was in the main Indian or Persian. You will note that during the rise of the two prevalences, when one might expect optimal

conditions for the propagation of the disease by lice, the mean daily maximum temperature is gradually rising from a level of 60° to one of 75°, while the relative humidity which reached its maximum in January, is now slowly falling, to reach its minimal value of 10 to 20 per cent. in July, the hottest month of the year. According to Moore and Hirschfelder a relative humidity of between 60 and 80 per cent. with a temperature of 28° to 32° C. represents the most favourable conditions for the rearing of lice in the thermostat. A high relative humidity of 90 per cent. or more was injurious to rearing, more so even than a low relative humidity. After April, conditions become increasingly unfavourable for the louse, the very high temperature with rapidly diminishing humidity interfering with breeding while the hot weather leads also to shedding of superfluous clothes and to excessive sweating and consequently deprives the louse of comfortable shelter. According to Nuttall, if the temperature rises above 40° C. lice get wildly active and soon succumb to exhaustion and they die in a few minutes at a temperature of 45° to 50° C.

Coming now to the question of bacteriological diagnosis, this in the case of relapsing fever was entirely determined for notification purposes by the demonstration of the spirochæte. No case was returned as relapsing fever in the absence of a microscopic finding and from my survey of the laboratory findings analysed by me, there is the closest correspondence between the laboratory returns and the official figures, which I had an opportunity of scrutinizing. I need not enlarge on the microscopic diagnosis of relapsing fever. It is, as a rule, a simple and straightforward procedure. Whether the species of spirochæte present was any other than the European or Russian form of *recurrentis* was not determined. The determination of species is a difficult matter at the best and facilities for extensive animal experiments including the employment of monkeys were not available. In any case there was no reason to suppose that any insect other than the louse played the part of vector. I note in a recent number of the *British Medical Journal* that in North Persia there was some evidence that a form of relapsing fever was associated with ticks (*Argas persicus*). This is an important observation and waits confirmation.

The diagnosis of typhus fever in Mesopotamia was, up to the last quarter of 1918 on a clinical basis, supported however by results of blood culture to exclude a typhoid or paratyphoid origin and microscopical examination of blood films to exclude relapsing fever and malaria. With regard to the latter, however, the fact that the onset of

typhus or any other intercurrent infection might cause malarial parasites to appear in the peripheral circulation in a quiescent malarial subject, and thereby confuse diagnosis, had to be borne in mind. I might quote an instance of this occurrence more especially as the subject of it was the Entomologist to the Force with whom I had arranged to carry out some experiments designed to study the *Rickettsia* bodies then coming into prominence through the work of Ricketts and Rocha-Lima. The



investigation was shortlived. My colleague one day was making a roll-call of the lice which had been allowed to feed on typhus patients and found that two were missing from the pill-box. One was found crawling in the cigarette tin in which the pill-box was returned to the laboratory while the other could not be found. Exactly nine days later my colleague developed typhus fever and he passed through a very severe infection. He had previously contracted malaria and the onset of fever with the reappearance of malarial parasites only temporarily confused

the issue. This case is only one of the many and sometimes tragically fatal laboratory infections which have occurred among typhus fever investigators. I need only mention Ricketts and Prowazek in recent years.

In the last quarter of 1918 when typhus fever again appeared, we had an opportunity of supplementing the purely exclusive methods of blood culture and microscopic examination of blood films by the employment of the Weil-Felix reaction. I had fortunately been able to secure a culture of the proteus strain X19 now used almost universally for this test. There is no question whatever that, though the actual materies morbi of typhus fever is still unsettled (but, I believe, rapidly approaching settlement), the bacteriologist is now in a position to give most valuable help of a positive nature in the diagnosis of typhus fever. Captain Napier has recently reported favourably on his experience with this test at the refugee camp of Bakubah, near Baghdad. At the Central Laboratory, Baghdad, tests were carried out by Captain Bowen, R.A.M.C., under the superintendence of Major Mackie, I.M.S., and also personally controlled by myself. The results impressed me greatly, and one almost became persuaded that an organism which reacted so strongly and with such regularity to the sera of typhus patients could be no other than the actual materies morbi. Though certain bacteriologists of repute support this view at the present time, it is, in my opinion, highly improbable, and the phenomenon is capable of other explanation. Perhaps the best preliminary to the formation of any reasoned opinion on the matter would be a survey of the extraordinarily chequered history of research into the ætiology of typhus fever. In the eighties bacteriological methods, then only in their infancy, sufficed to demonstrate the causative agents of a long series of infective diseases. Why did not typhus fever yield to similar methods? Many claimed that it did, while many more failed to demonstrate anything by the usual methods of bacteriology then practised. Thus, as early as 1883, we find a record of the finding of motile spirilla in typhus blood. Since then scarcely a year has passed without some claimant coming forward with a new organism of typhus fever. Cocci of various kinds, including streptococci, bacilli, diplobacilli, spirilli, spirochætes, and also organisms of more protozoal structure have been incriminated, without receiving much encouragement or confirmation at the hands of other workers. I need not enumerate them. The fact that many of the names are Russian is in consonance with the fact that Russia and Poland have long been important foci of typhus fever. Doubtless many of the findings

recorded in the literature were the ordinary contaminations due to imperfect technique associated perhaps with imperfect judgment, but, none the less, while possibly the great majority of observers have recorded completely negative results from blood culture, and as many more have not recorded at all their negative findings, the fact has to be reckoned with that at least some of the organisms recovered from typhus cases represent genuine blood invaders. Of the more recent of these one might quote the cocci recovered by Wilson of Belfast in 1910. Very similar cocci of the *Streptococcus viridans* type were recovered by Penfold from the blood of typhus cases in Ireland in 1916, and also by Miss Robertson. Penfold has been able to cultivate the same coccus from monkeys inoculated with typhus blood, and with pure cultures of this coccus he noted a pyrexial phase in inoculated monkeys. Miss Robertson immunized monkeys with this coccus of Penfold's, but they did not prove to be immune when tested with virulent typhus blood, and reacted similarly to the untreated controls. In 1909 Horiuchi recovered from the urine of some cases of typhus fever in Manchuria an organism (motile, non-lactose fermenter and indol-former) which was agglutinated by the sera of typhus patients only, and that in high dilution. Wilson of Belfast in 1910 also had a similar experience. In 1916 Weil and Felix recovered from the urine of a typhus patient organisms of the *proteus* type which were agglutinated in high dilution by typhus sera only. Later they recovered another *proteus*, which they named X19, and which was found to act still better in this test. During the last three years an enormous literature has accumulated on the diagnostic value of what is now called the Weil-Felix reaction (but which, perhaps, should more properly be called the Horiuchi-Wilson-Weil-Felix test), and its probable significance. As to the diagnostic value of this test in typhus no doubts have been uttered. The serum of 90 to 100 per cent. of typhus cases will give this reaction in high dilution at some stage of the disease. Reactions have been obtained with dilutions of serum up to 1 in 30,000, while control sera either do not agglutinate the organism at all or, in a small proportion only, react with it in dilutions not exceeding 1 in 50. It is calculated that about 50 per cent. of cases will give a reaction by the fifth day, while by the tenth day practically all cases of typhus fever will have reacted. I show you here some illustrative examples. In several cases samples came in from suspected typhus, which proved completely negative in dilutions of 1 in 10. In these cases the relapsing fever spirochæte was ultimately demonstrated, so that where both diseases are prevalent, as they

frequently are, a negative reaction is sufficient to suggest another diagnosis than typhus fever. *Proteus* X19 has recently been recovered from the blood in a varying percentage of typhus cases. Thus, Zeiss got *proteus* from the blood in nineteen out of 278 cases, but never from the blood of 167 cases of other disease. Dienes got *proteus* from the blood in 30 per cent. of fourteen cases. Others have been less fortunate, and Felix in Asia Minor failed to recover the organism in a single case out of 250. *Proteus* X19 is highly pathogenic for guinea-pigs and mice, but it does not produce in these animals the pyrexial phase which follows the inoculation of typhus blood, and which can be repeated from one guinea-pig to another. Nor are animals immunized with *proteus* rendered immune to typhus blood. The Weil-Felix reaction comes most probably under the category of paragglutination phenomena, from which, however, it differs in one somewhat material point—viz., the permanence of the agglutinability of X19 with typhus sera in spite of long-continued laboratory growth. *Proteus* X19, and probably other varieties of *proteus*, gain an entrance to the blood stream probably from the intestine in certain cases of typhus, while other cases are more liable to be invaded with organisms of coccal type. One can only assume that a particular *proteus* has become so serum-impressed as to retain its agglutinability for an indefinite period, and to serve as a test organism for the polyagglutinating capacity of typhus serum. It is said, for example, that typhus serum agglutinates *Micrococcus melitensis* in 20 to 60 per cent. of cases, *Bacillus typhosus* in 30 per cent. of cases, Rabinowitsch's organism in 80 per cent. of cases, and Plotz's organism in 30 to 90 per cent. of cases, and so on. We have certainly, therefore, to reckon with this fact of the polyagglutinating capacity of typhus serum in assessing the position of *proteus* X19 with respect to typhus fever.

While knowledge has increased in the direction of typhus diagnosis, knowledge has also increased of the part played by the louse. On this subject another will speak. Here I would merely add that it has become necessary for claimants coming forward with new organisms and for sponsors for older organisms, to bring their findings into line with this new knowledge. Thus, if the louse is admitted to be the only vector, it is necessary to demonstrate these presumed typhus organisms in the louse or in louse excreta. Accordingly, we have the adherents of Rabinowitsch's organism, of Plotz's anaërobic organism, and of *proteus* X19 coming forward to identify their respective organisms with the *Rickettsia* bodies discovered in infected louse excreta by Ricketts in

1909, and later confirmed by Rocha-Lima and Prowazek in 1916. Now these *Rickettsia* bodies have not yet been grown, and if they had anything in common with the organisms identified with them they should have been readily cultivable. This question, however, will be discussed more fully by Dr. Arkwright.

Remarks on the Virus of Typhus Fever and the Means by which it is Conveyed.

By J. A. Arkwright, M.D.

A number of bacteria have been cultivated from the blood, organs and excreta of typhus patients and proposed as the causal organism, but they have for the most part lost any passing prestige which they may have enjoyed. Plotz first published an account of his bacillus in 1914, and for some time it appeared to be the best accredited of the claimants for the first place in the crowd of competitors. The *Bacillus proteus* X19, of Felix and Weil, discovered in 1916, has very strong serological claims to recognition, but as the primarily infecting and causal organism it has now few supporters. In the meantime the work of Nicolle and his collaborators in 1909 and afterwards, as well as that of Ricketts and Wilder in Mexico, and Goldberger and Anderson in connexion with Mexican typhus and the form of the disease known as Brill's disease in New York, disclosed some very important facts relative to the properties of the virus, its location in the body and the means by which the disease is spread.

The first and most important step in this series of advances was made by Charles Nicolle (July, 1909), when he showed that typhus fever could be transferred to monkeys by injecting the blood of patients taken during the febrile stage. He first infected a chimpanzee and from it a *Macacus*, and it was subsequently shown that macaques could be directly infected from man. In 1911, he demonstrated the further fact of great experimental importance that the virus could be kept active without losing its potency by passage through guinea-pigs. These animals react with fever, and their blood and organs contain the virus with which further guinea-pigs or monkeys can be infected. In 1918, Nicolle was able to report that the virus had been kept active for four and a half years by 150 serial passages through guinea-pigs, after one passage

through a monkey. Another strain of virus was lost only after ninety-two passages. It was shown by Landsteiner and Hausmann that so small an amount as 0·005 c.c. of guinea-pig's blood might be sufficient to transmit the disease. Ricketts had been able to infect a monkey with 0·2 c.c. of human blood. Nicolle, however, found that 2 to 2·5 c.c. was the most suitable amount of blood with which to infect a guinea-pig by intraperitoneal injection. In September, 1909, Nicolle, Comte and Conseil also reported the transmission of typhus from man to the monkey and from monkey to monkey by the bites of *Pediculus corporis*.

The conclusions reached, about 1910-11, by the French and American workers may be summarized as follows:—

(1) The blood of human cases of typhus is infective for monkeys, and the disease can be transmitted by the blood from monkey to monkey, from monkey to guinea-pig, from guinea-pig to guinea-pig and back again to the monkey after many passages through the guinea-pig without loss of virulence.

(2) The virus can be conveyed by the plasma and by the serum obtained by centrifuging defibrinated blood, but not readily, if at all, by clear serum taken off the clot.

(3) The virus in blood is destroyed by heating to 55° C. for fifteen minutes and by drying *in vacuo* over sulphuric acid.

(4) The virus does not pass through an efficient Berkefeld filter in sufficient amount to cause an infection.

(5) Lice which have been infected on a human case or on a monkey can convey the disease to a monkey by their bites, and inoculation of their gut contents by scarification or subcutaneous injection will infect a monkey or a guinea-pig.

(6) Typhus may be transmitted experimentally by either *Pediculus corporis* or *Pediculus capitis*.

(7) The experiments of Ricketts and Wilder point to a necessary incubation period in the louse before it becomes infective, but their evidence is not very complete, and at this period different observers did not agree on this point.

With regard to the evidence as to the sufficiency of the bites alone of lice to convey the disease or the necessity for the introduction of the fæces or body contents of lice by scratches or through previously existing abrasions, no very exact experiments appear to have been attempted. Nicolle, Ricketts, Rocha-Lima and others appear to accept unhesitatingly the evidence that bites alone are sufficient to cause

infection. Cantacuzene (July, 1919) has recently narrated instances of fifteen medical men who were all infected by the bites of usually one or at most two lice, in some instances the exact time when the bite took place having been accurately ascertained, and the incubation period precisely determined. This was usually eight to ten days, but in one case as long as twenty-three days.

The most advanced phase in our information about the virus of typhus is chiefly concerned with it as it occurs in the louse. The existence of the virus in the louse is closely associated with the presence in the gut of very small bacillus-like bodies now known as *Rickettsia Prowazeki*. This organism was first described by H. T. Ricketts and Russell M. Wilder, of Chicago, in April, 1910, as occurring in the blood of patients suffering from tabardillo or Mexican typhus and in the body of the louse. In blood films of patients taken on the seventh to the twelfth day of the disease they invariably found a short bacillus of the same morphology as bacteria of the hæmorrhagic septicæmia group which stained either solid or only at the poles and was not more than 2 microns by 0·3 microns in size. These forms stained purple by Giemsa and were not very numerous in the blood; not more than one was seen in once crossing a $\frac{3}{4}$ in. coverslip; they estimated that there were between 300 and 2,000 in a cubic centimetre of blood. In the fæces and intestine of the louse they were very much more numerous. They also describe other bacteria found in smears of lice. They state that polar staining organisms are occasionally found in the fæces of normal lice, but that they are almost invariably present in large numbers in infected lice.

Ricketts likened the organisms in typhus blood and infected lice to similar bodies which he had found in the blood of guinea-pigs and monkeys and to a less extent of men infected with Rocky Mountain spotted fever, which were also present in enormous numbers in the gut and solid organs of the ticks infected with this latter disease. He unhappily died of typhus contracted during his researches, and left his work unfinished.

Edm. Sergent, Foley, and Vialatte in June, 1914, described bipolar bacteria in lice infected with typhus, and they stated that they never found them in thousands of lice dissected in former years for other purposes nor in lice which had sucked only healthy persons or those suffering with relapsing fever. They call them cocco-bacilli, and give their measurement as slightly larger than Ricketts did. They say that they are found rarely in lice taken in the early days of the disease, but

that they increase in numbers progressively as the disease advances and that the number of lice infected also increases at the same time. The French observers were apparently unaware of Ricketts' work.

Our knowledge of these minute micro-organisms has been largely advanced by Rocha-Lima, who first worked with Prowazek in Poland, 1914-15, where both caught typhus fever of which Prowazek died. Rocha-Lima rediscovered these forms in the louse and Prowazek considered that they were the same that he had seen and described in the blood of patients chiefly inside leucocytes when working at typhus in Poland in 1913. Rocha-Lima was much helped in his work by Sikora, who had previously made herself an expert in the rearing, manipulating, feeding and general management of lice and had also studied them by means of large numbers of serial sections.

Rocha-Lima had two great advantages over Ricketts and Wilder in his work: (1) his association with a highly skilled entomologist, who had made a speciality of pediculi, and (2) the additional knowledge due to Nicolle that guinea-pigs are susceptible to the virus of typhus, and can be used as a test of its presence without recourse to the more expensive and in some ways less satisfactory monkey. He was able by improved technique to keep the pediculi in better health, so that fewer died, and to get more regular results as regards their infection and the time of appearance of the virus and the microscopic bodies in the louse.

Rocha-Lima was primarily a protozoologist, and was inclined to consider the organisms in the louse as protozoa strongyloplasma on account of their staining properties and intracellular position, and also from regard for Prowazek's opinion. He called them *Rickettsia Prowazeki*. By collecting lice from patients and other persons and also feeding normal lice from Hamburg on them and subsequently examining the lice microscopically by means of sections and smears of the gut and by inoculating guinea-pigs with their gut contents he was able to determine the following points:—

(1) When pediculi (*Pediculus corporis* or *capitis*) were fed for five to eight days on a typhus patient, beginning preferably between the fourth and the eighth days of illness a certain proportion of them were found to contain very large numbers of *Rickettsia* often in apparently pure culture, and these were found not only in the lumen of the gut but also were seen to be present in the epithelial cells of the mid-gut in masses. Before the fourth or fifth day from the commencement of feeding the lice did not show these forms.

(2) Lice so infected were able to give the disease to guinea-pigs

when crushed and injected subcutaneously. Even part of the gut of one louse was sufficient to infect.

(3) Lice fed only after the defervescence did not contain *Rickettsia* and were not infective.

(4) Lice kept at a temperature of 23° C. did not become infective and did not contain *Rickettsia*, and a temperature of 32° to 34° C. was the optimum for producing both these conditions.

(5) In order for lice to become infected it was not necessary that they should continue to feed on a febrile patient. If after one or two such feeds the lice were subsequently fed on a convalescent patient they became infected, and since convalescents are immune, no risk of infecting them was involved.

(6) If lice were fed daily on a patient the proportion of infected lice rose as time went on and after ten days or so almost all the lice were infected. He found that 95 per cent. of the lice taken from advanced cases of typhus were infected. Lice once infected usually remained so until they died, but sometimes they appeared to get rid of their infection and recovered.

The lice used by Rocha-Lima for feeding experiments were derived from a source free from typhus or were bred in captivity and previously only fed on healthy persons.

(7) Almost all attempts to demonstrate the inheritance of infectivity or of *Rickettsia* bodies in the louse have failed, but Ricketts, Sergent and Rocha-Lima each thought they had one successful experiment showing inheritance of the virus. Rocha-Lima and others have had many failures.

(8) No attempts to cultivate the *Rickettsia* from lice have succeeded.

Other workers have comfirmed Rocha-Lima's results with some divergence of opinion in matters of detail. One of the most complete confirmations is from Toepfer and Schüssler, the latter of whom was another investigator of typhus who died from this disease contracted during his work.

Control Observations.—Rocha-Lima found that lice fed on men who were healthy or were suffering from other diseases—e.g., scarlet fever, pneumonia, &c., and especially lice from places such as Hamburg, which were not typhus areas, very rarely showed *Rickettsia* and never the intracellular form which he associates with typhus. Nevertheless the most serious criticism of the rôle played by *Rickettsia* comes in part from those who are most impressed with its importance. Ricketts, Rocha-Lima and Otto and Dietrich all found these bodies or forms

suggesting their presence in lice believed to be normal. The frequency of these finds apparently varied, but they are usually described as rare. Nicolle and Brumpt however believed that they occurred so frequently in controls as to discount their importance. Nicolle (1914) found five lice containing bodies like those described by Sergent amongst 100 lice collected far from an epidemic focus of typhus. Brumpt (1918) examined seventy-two *Pediculi corporis* from seven German prisoners at Rennes; the men appeared to be healthy and there had been no typhus in Rennes. Fifty-three—i.e., 73·6 per cent. of these lice contained *Rickettsia*. This conflicting testimony is probably to be explained quite readily: (1) By the great difficulty or impossibility of being sure of the nature of very small diplococcal bodies seen in a film when only a few are present; infected lice usually have massive infection. (2) By the existence of at least two kinds of *Rickettsia* in human lice. These have been named by Rocha-Lima *Rickettsia Prowazeki* and *Rickettsia quintana*, the latter being as clearly and closely associated with trench fever as is the former with typhus.

Arkwright, Bacot and Duncan (1919) were able abundantly to confirm the association of *Rickettsia* with trench fever cases. We found that appearances suggesting these bodies in lice from healthy persons or those suffering from other diseases here in London were exceedingly rare and believe that the one instance in which the lice were above suspicion can be ignored as an error of observation made at an early stage of our work. We were in a fortunate position as regards controls, since trench fever was the only disease to be considered.

Rocha-Lima believes that there is also another species, found in lice from normal persons, which he calls *Rickettsia pediculi*. Considering the very large number of cases of trench fever in all the armies in Western Europe during the war and the fact that trench fever patients remain infective for weeks or months and can infect lice when not febrile, it seems very probable that most if not all the irregularities in occurrence of *Rickettsia* in Germany, Poland and France can be attributed to trench fever infections, especially as the association of trench fever with *Rickettsia* was not recognized till about 1916.

Arkwright, Bacot and Duncan (1919) have confirmed part of the work on *Rickettsia* and typhus fever. I was able through the kindness of Dr. Frederick Craig to see some cases of typhus in Ireland in 1917 and collect lice from their clothes and feed normal lice on them. We found *Rickettsia* in the lice and their excreta in a few specimens, but

I have now no doubt that the lice were kept between the feeds at too low a temperature for a free development of *Rickettsia*, as I had no incubator. However we were able to infect a monkey by injecting twenty lice and we passed on the infection to two other monkeys and to guinea-pigs. Besides demonstrating the *Rickettsia* in pediculi fed on a monkey, we were able to show that the monkey louse *Pedicinus longiceps*, infesting these *Macacus rhesus*, became infected with *Rickettsia*. Of ninety-three pedicini from three infected monkeys, 38·7 per cent. were infected, and of over 100 pedicini from healthy monkeys none showed these forms.

The means by which the virus of trench fever is kept alive during non-epidemic times is uncertain. Ricketts, Rocha-Lima and Sergent each believed that they had obtained some evidence of heredity of the virus in the louse, but the great majority of experiments with this object failed.

There is no evidence that human cases of typhus continue to carry the virus after they have become convalescent, but such evidence may yet be found. Unrecognized mild cases no doubt occur.

The microscopical pathology of typhus has comparatively recently given interesting results. Fraenkel, Jaffé, and others, described minute nodules of cellular infiltration in the course of the arterioles and changes in these vessel walls in various organs and in the skin lesions, but especially in the brain in the region of the "iter." It is even claimed that a diagnosis can almost certainly be made post mortem by means of frozen sections of the mid-brain. *Rickettsia* have also been described in these nodules as Wolbach has described them in the vascular lesions found in man and in guinea-pigs dead of Rocky Mountain fever. Kuczinski described *Rickettsia* in the endothelial cells of the liver of patients dead of typhus, and Jaffé has confirmed the occurrence of similar appearances. Quite recently Wolbach and Todd have given an account of very small bodies in the endothelium of small vessels in the skin lesions of typhus. These minute forms they regard as the causal organism and name *Dermacentroxenus typhi*, since they consider it another species of the same genus as *D. Rickettsi*, by which name Wolbach called the organism which he considers the cause of Rocky Mountain spotted fever. Such small bodies as *Rickettsia* are, however, very difficult to recognize in the blood or tissues on account of the presence of other granules of approximately the same size.

Attempts to strengthen the position of *Rickettsia* and to establish

a diagnostic test by agglutination or other serum reactions have been made, and Otto and Dietrich described agglutination of typhus *Rickettsia* by the blood of typhus convalescents, as Ricketts had done with the Rocky Mountain fever organism. Ledingham has recently shown that the serum of rabbits immunized with trench fever *Rickettsia* will agglutinate these bodies up to a dilution of 1 in 320 or more, the control sera leaving the emulsion unaffected.

Many experiments with a view to producing a prophylactic vaccine or a therapeutic serum have been made, but none so far has given very striking results. Perhaps the most hopeful is that of Nicolle and Blaizot who have immunized horses with extracts of organs of infected guinea-pigs. This serum is however not easy to test as it is very toxic for guinea-pigs on account of the virus from which it is prepared, and the results of any extensive trial on human cases is not yet reported. Vaccination experiments with the heated blood of patients, or with virus from other sources treated in various ways to make it innocuous, have not given encouraging results.

Two accounts of encouraging attempts to immunize human beings against typhus are given (1) by C. Lebailly and H. Poirson, and (2) by H. Poirson. They used for inoculation (1) immune horse serum, (2) the serum of typhus patients, (3) the serum of infected guinea-pigs.

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Section of Medicine.

President—Dr. A. F. VOELCKER.

Non-nephritic Albuminuria.[1]

By R. L. MACKENZIE WALLIS, M.D.

THE subject for discussion to-night is one which should commend itself to all medical men, since I have no doubt many will recall cases which may be legitimately included under this heading. In the first place I would offer my apologies for the title selected which, though adequate as far as it is comprehensive, yet lacks that word or combination of words necessary to convey its true meaning and significance. It is designed to include all those forms of proteinuria which are not due to organic disease of the kidney, viz., physiological albuminuria, transient albuminuria, intermittent albuminuria, functional albuminuria, and a group designated "leaky" kidneys. My remarks will be confined to the purely chemical side of the question, and I leave the comment on the subject from the clinical aspect in more capable hands.

We are all familiar with the physiological albuminuria which occurs after severe exercise. It is generally transient in character, and varies with the severity of the exercise—in some cases large amounts of protein being excreted in the urine. The protein present is largely serum albumin, which may readily leak through the kidneys with the excessive amounts of normal urinary constituents secreted after exercise. The same applies to alimentary albuminuria if such a condition ever occurs after ingestion of excessive protein in the food. It is much more likely to be due to the same cause as exercise albuminuria. The same applies to albuminuria following cold baths. The kidney is called upon to excrete

[1] At a meeting of the Section, held March 23, 1920.

an excessive amount of urea and other metabolic products; the urea exerts a diuretic action, and it is conceivable that under these conditions an over-worked kidney may leak serum albumin. The conditions under which the albuminuria occurs, quite apart from exercise or possible excessive protein diet, we describe as "functional" albuminuria. This term I take to include adolescent, cyclic, and postural or orthostatic albuminuria. It is a fairly common condition, especially in public school boys, but by no means confined to them. Then there is the group of familial intermittent albuminuria. Lastly, we come to an interesting class which perhaps will arouse considerable discussion and I hope added interest—viz., the "leaky kidney." The cases to be described by Dr. Batty Shaw and Dr. Tyson are included in this group. Here we meet with cases of long-standing proteinuria, where the kidneys do not show any evidence of organic disease, or at least the lesion, if any, is not progressive. Further, throughout their life history there has been no evidence of nephritis.

It would appear of the utmost importance to be able to differentiate these forms from the true organic albuminuria of nephritis, not only from the point of view of prognosis, but also for the benefit accruing to the patient. It is my object to-night to prove to you that by means of simple tests these two forms of proteinuria may be differentiated. My interest in this subject was stimulated some years ago by my colleague, Dr. Thursfield, and I would here express my indebtedness to him for his help and encouragement.

The Proteins in the Urine.

The term "albuminuria" has been used for many years to designate the presence of serum albumin in the urine, and has assumed such importance that it is frequently regarded as definitely pathological. The presence of protein in the urine nearly always results in total rejection or heavy loading in people examined for life insurance. Why this importance should be given to albuminuria I am unable to comprehend.

The serum contains two proteins, serum albumin and serum globulin; and in organic albuminuria the serum albumin in the urine is generally in excess. Nearly all the tests applied to urine are designed for the detection of serum albumin, and the presence of serum globulin is rarely sought, and if so, not regarded as of any significance.

The best test for proteins in urine is the salicyl-sulphonic acid test.

The reagent can either be applied to the urine in the solid form or in solution. In carrying out the test the following procedure is adopted. To 10 c.c. of the urine in a test tube, add about six drops of a saturated watery solution of salicyl-sulphonic acid. The presence of protein in the urine is indicated by varying degrees of opalescence up to a dense flocculent precipitate or even almost complete solidity. This test is exceedingly delicate and at the same time reliable, and enables us to assess roughly the amount of protein present in any given specimen. It is of about the same delicacy as the boiling test, but the salicyl-sulphonic acid has the advantage of being easily applied, and does not necessitate heating. Having decided that the urine contains protein we can at once proceed to differentiate the type or types of protein, and the relative amounts of each present in a given specimen of urine. If there is an increased amount of globulin present the urine will give the following test. To about 10 c.c. of the urine add drop by drop a dilute solution of acetic acid (33 per cent.). The presence of globulin will be indicated by the appearance of a white precipitate produced without the aid of heat. If the test is repeated but the urine diluted with distilled water, the result is even more marked. It is important to use dilute acetic acid, and not to add excess of the reagent as otherwise the globulin precipitate re-dissolves.

The presence of globulin may be confirmed by applying the simple test known as Roberts' test. This consists in filling a tall jar or cylinder with distilled water and adding the urine drop by drop from a pipette. As the drops descend in the water white rings form, just like smoke rings, when globulin is present in large amounts.

For determination of the total globulin content of the urine it is first made alkaline with ammonia, and then treated with ammonium sulphate to half saturation. On standing, a precipitate of globulin occurs which is separated, dried, and weighed. At the same time the total proteins are estimated, and the difference between this result and the weight of globulin gives the amount of serum albumin present in the urine. Having ascertained these figures the calculation of the ratio of albumin to globulin is an easy matter. The following table gives the ratios obtained in the following conditions:—

	Albumin to globulin
Functional albuminuria	2 : 1
Leaky kidneys	1 : 2
Chronic nephritis	6 : 1
Toxic nephritis	6 : 1

The Presence of Casts.—The urines examined often showed the presence of a few hyaline casts, but as these are so frequently present in normal urine, especially when a centrifugalized deposit is examined, no particular pathological import is ascribed to them. The cases which I have examined have been nearly all males, in whom pseudo-hyaline casts are frequently met with; these casts especially occur in morning specimens, and are due to contamination with semen and spermatozoa.

A striking feature of nearly all the urinary deposits examined was the presence of calcium oxalate crystals. In looking through my records I have been struck with the constant occurrence of these crystals, but as to their significance I am unable to express an opinion.

Having ascertained the nature and relative amounts of the proteins present, we can now apply various functional tests to determine as far as possible the efficiency of the renal epithelium. The kidney normally excretes those substances which the blood brings to it, and which are of no further use to the body. When there is organic disease of this organ then it shows a tendency to retain certain substances which it should excrete, and to separate out other blood constituents which it should normally retain. Consequently in non-nephritic albuminuria we find that the analysis of the blood shows no marked deviations from that found in a normal individual, in other words there is no retention of urea or chlorides. The analysis of the urine also corresponds to the findings in the blood. In the estimation of urinary diastase we have a ready means of differentiating these forms, as the following list clearly demonstrates.

		Diastase in urine in units
Functional albuminuria		10—22·2
Leaky kidneys		10—33·3
Chronic nephritis	Mild	10
	Moderately severe	5
	Severe	Less than 5
Toxic nephritis		Very high, 50—200

The simple urea concentration test, devised by MacLean, should receive a trial, as it bids fair to become a most useful test for renal efficiency. Unfortunately, I have not had an opportunity of trying this test on cases of non-nephritic albuminuria, but I have applied it with successful results in several cases of chronic nephritis, and also the toxic nephritis of pregnancy.

With these tests it should be possible to draw conclusions as to the state of the kidneys in any given case.

The Significance of Globulin in the Urine.

The fact that all the urines I have examined of cases of non-nephritic albuminuria gave the well marked precipitation with acetic acid in the cold led me to investigate the nature of the protein present in such urines. In 1909, my attention was called to the fact that a number of milky effusions which I examined gave a similar reaction, and the globulin present was found to be of an abnormal type. The globulin existed in combination with lipoids, and this combination was responsible for the opalescence of such effusions. Now we know that the globulins in serum are separated into two forms, one insoluble in water and known as euglobulin, and the other soluble in water and termed pseudo-globulin. By adding various lipoids to pseudo-globulin I found it possible to render this insoluble in water, in other words to convert it into euglobulin. In my opinion euglobulin is really globulin which has associated with it a small quantity of lipoids. In this respect euglobulin is identical with the lecithin globulin isolated from pseudo-chylous effusion, the degree of opalescence depending on the amount of associated lipoids. This euglobulin is precipitated by acetic acid in the cold, and is in my opinion the protein present in the urine which gives this test. An excess of this euglobulin containing excess of lipoids either in the blood, pleural, pericardial, or peritoneal effusions, or in the urine, gives rise to a marked opalescence, and when this globulin is removed then the opalescence disappears. A remarkable case of this type of proteinuria was described by Bramwell and Paton in 1892.[1] Globulin was present in such quantity that the urine spontaneously deposited rhombic crystals of globulin. Of a total protein content of 2 per cent., 1·92 per cent. was globulin. A feature of great interest was the observation that the kidneys showed no obvious pathological changes. The cases to be described by Dr. Tyson (p. 108) appear to be of a similar nature, and certainly this was the case in the urine of one of his cases which he kindly gave me the opportunity of examining. The case which Dr. Batty Shaw is describing in detail (p. 105) was a case of true lecithin-globulinuria, and again I was able to make a complete analysis. In a number of cases of functional albuminuria the protein present has proved to be largely euglobulin with associated lipoids. The urines I have examined have all given the same reactions, and analysis has shown that the protein precipitated by acetic acid in the cold is without

[1] Bramwell and Paton, "Laboratory Reports of the Royal College of Physicians, Edinburgh," 1892.

exception euglobulin. Langstein[1] has made similar observations, and describes the protein as euglobulin, which on analysis was found to contain small amounts of phosphorus. In my opinion this phosphorus was derived from the lecithin associated with the euglobulin.

Pollitzer[2] on the other hand regards the protein as a combination of albumin with chondroitin sulphuric acid.

The results I have obtained lead me to the opinion that in non-nephritic albuminuria the chief protein present is euglobulin associated with lipoids, and that this protein is responsible for all the tests described above. Ordinary serum albumin is present as well, but being soluble in water does not give these tests.

Albuminuria associated with organic disease of the kidney is of a totally different type. Here we almost invariably find the chief and predominant protein in the urine is serum albumin, there being usually only a small amount of globulin. This is the result one might expect since albumin is much more diffusible than globulin. Further, the proteins present in the serum may be arranged in the order of increasing viscosity as follows: Serum albumin, pseudo-globulin, and euglobulin. This explains the relative abundance of the first in exudates, and the increased amount of the last in transudates. It would appear from this that the albuminuria of nephritis was similar to an inflammatory exudate, whilst the globulinuria of non-nephritic albuminuria was of the nature of a passive transudate. That it is due to mere molecular concentration is possible, owing to the fact that globulin is less diffusible than albumin. We cannot, however, explain why the pigment urochrome, which is of much larger molecular size, readily passes through the kidney, as also does the ferment diastase. In severe albuminuria, however, the kidney does not excrete much urochrome or diastase, and pigments, such as urobilin and uro-erythrin, are usually absent. The presence of increased globulin in the urine in chronic nephritis is regarded as an unfavourable sign, and this applies with even greater force to the blood. Bright noted the fact that when the serum of a case of nephritis became milky this was a danger signal, and suggested that the milkiness was not due to fat. In all the cases of milky sera which I have examined, the milkiness has been found to be due to an excess of globulin combined with lipoids and not to fat. This condition is not a true lipæmia, but rather a lipoid globulinæmia. In nephritis

[1] Langstein, "Monographie die Albuminurien die älterer Kinder," Leipz., 1907.

[2] Pollitzer, *Deutsch. med. Wochenschr.*, March 14, 1912.

without œdema or uræmia, an increased amount of globulin may be found, and the same occurs in parenchymatous nephritis.

In eclampsia there is almost always a large proportion of globulin present, and this decreases after delivery. This globulinuria is associated with an increased amount of globulin in the blood. In these cases the globulin is not of the type of euglobulin, but agrees in its properties with pseudo-globulin.

The protein precipitated by acetic acid in the cold is therefore euglobulin, and this protein occurs in non-nephritic albuminuria. In functional albuminuria it is present generally in small amounts, and tends to disappear when the patient reaches a mature age. Further, it is generally controlled, though not always, by the administration of calcium lactate. The group of so-called leaky kidneys exhibits this globulinuria in a more marked degree. It does not disappear so readily with treatment, but the characteristic feature is that, although globulinuria is constantly present, the lesion, if any, is not progressive. Further, like the case of Bramwell and Paton, there is no evidence of renal disease, and in all probability no structural changes would be observed in such kidneys. In certain cases the lecithin-globulinuria has been associated with syphilis, but since this condition has persisted long after the disease has yielded to treatment I am inclined to regard these cases as belonging to the group of non-nephritic albuminuria. They certainly have not shown any evidence of organic disease of the kidney, and none of the signs and symptoms usually associated with nephritis. Three of the cases were under the care of Mr. McDonagh, who has kindly sent specimens to me at intervals for examination.

The chief conclusions to be drawn from these observations is that there are a large number of cases of proteinuria in which the predominant protein is lecithin-globulin (euglobulin), and not serum albumin. These cases belong to the non-nephritic albuminurias. By means of simple clinical tests, particularly the acetic acid test, these may be differentiated. Further, the estimation of the urinary diastase and other tests for renal function provide evidence to show that there is no marked impairment of function in such cases. The ratio of albumin to globulin is also of importance as a means of differentiation. The clinical observations, combined with simple clinical pathological methods, enables us to give a favourable prognosis in such cases—a point of considerable importance for the well-being and future happiness of the patient. The tests suggested in this paper might also be considered by medical men engaged in assessing the individual for life insurance.

The work of MacLean[1] is of interest in connexion with the incidence of albuminuria amongst troops in training. Considering the conditions under which this work was carried out it represents a most praiseworthy effort. In 50,000 men he found albuminuria of all kinds in 6·48 per cent., and when pus, spermatozoa, &c., were excluded, the incidence worked out at 5·62 per cent. The figures for well-marked albuminuria came to 2·55 per cent. With regard to casts, the number of urines found to contain casts was 1·87 per cent., and of these 0·84 per cent. had definite epithelial casts and 1·03 per cent. hyaline casts. In only 1 per cent. of the men were definite signs of disease found, and presumably all the others had non-nephritic albuminuria. The point of greatest interest in this work was the fact that previous albuminuria played little, if any, part in the ætiology of war nephritis. It is highly probable that a large number of these cases belonged to the group of non-nephritic albuminurias, although we have no exact data on which to base any definite conclusions.

The occurrence of albuminuria in alcoholism is one of great interest, and is at present being pursued along these lines. No matter how large the amount of albumin present, the condition is only temporary, and disappears when the alcohol is slowly withdrawn. The question is whether these individuals show a definite renal inadequacy or whether, like the "leaky" kidneys, the albuminuria occurs apart from the presence of any detectable renal lesion. Such cases may, on the other hand, belong to the group of toxic nephritis. Investigations are now in progress to determine this point.

Cases.

Functional Albuminuria.—A large number of urines have been examined from time to time, particularly those from schoolboys who faint in chapel. These urines almost invariably gave a well-marked precipitate, with acetic acid in the cold and all the reactions for euglobulin. They contain albumin as well, and the ratio of albumin to globulin is generally about 2 to 1. The same condition is frequently met with amongst medical students, and at a large hospital medical school I have had an opportunity of examining several urines presenting these characters. In many of these cases the albuminuria has cleared up some years later. These urines frequently showed a few hyaline casts, and calcium oxalate crystals were almost constantly present.

[1] "Albuminuria and War Nephritis among British Troops in France," Medical Research Committee, Special Report, Series No. 43.

Cases of true *orthostatic albuminuria* have also been examined. It not infrequently happens that such cases lose their albuminuria temporarily, and in some instances show great variations in the nature of the proteins present. Thus one case gave all the reactions for globulin, but later, when improvement set in, only small amounts of serum albumin could be detected, and the globulin reactions were not obtained.

The prognosis in all these cases of functional albuminuria is very favourable, and I do not think that it in any way predisposes to organic kidney disease. This view is borne out by the results of MacLean and also by those of Dukes in observations on Rugby schoolboys.

The Cases of "Leaky" Kidneys.—Three cases of marked globulinuria associated with syphilis have been examined. The urines from these cases gave all the characteristic reactions, and the ratio of albumin to globulin was again 1 to 2. Perhaps the most striking example is the case to be described by Dr. Batty Shaw. The urine, when treated with acetic acid in the cold, gave a thick gelatinous precipitate, the urine becoming almost solid. The distilled water test showed the white smoke rings very clearly, and on dialysis there was almost complete precipitation of the protein. This protein, when isolated, gave a well-marked phosphorus reaction, and all the reactions of lecithin-globulin. The remarkable feature of this case was the fact that the urine I examined was of a clear pale yellow colour, and gave no macroscopic evidence of the presence of this interesting protein.[1] The two other cases of "leaky" kidneys were examined with similar results, but in these the globulin reactions were not so well marked. The urine from a patient of Dr. Tyson, which I examined, showed a total protein content of 0·8 gr. per cent., and the ratio of albumin to globulin was 1 to 2. The globulin when isolated contained fatty acids of the palmitic series, but no cholesterol or phosphatides could be detected.

[1] On certain occasions the urine was distinctly opalescent, and suggested the condition was one of bacilluria. Cultures made from catheter specimens, however, proved the urine to be sterile. The opalescence was clearly due to the presence of lecithin globulin; a point of considerable importance in view of my observations on pseudo-chylous ascites already referred to.

An Unusual Form of Turbid Urine due to the Presence of Lecithin-globulin.

By H. Batty Shaw, M.D.

The behaviour of the urine when examined by the ordinary reagents for albuminuria, and the occasional unusual appearance of the urine noted in the following case, were so puzzling, and have received such a ready solution at the hands of Dr. R. L. Mackenzie Wallis that the notes of the case are now reported.

Dr. S., aged 37, came under observation in March, 1913, complaining of albuminuria, from which he had suffered for eighteen months. For the first half of this period he had had marked dropsy of the feet: during the remainder of the period the dropsy was less, largely due to the fact that he had kept his bed. He had contracted syphilis eight or nine years previously, and was treated at once with hydrarg. cum creta for two years continuously. He came to England for further advice concerning the albuminuria above mentioned, and was given a course of mercurial inunction for six weeks, shortly after landing, that was seven months before he consulted me. He had had scarlet fever at the age of 6, and had had albuminuria afterwards up to early youth. Knowing of this early nephritis he had occasionally examined his water as a young man, and always found it free from albumin, and was accepted as a first-class life on three occasions. Before the last of these examinations for insurance, which was carried out shortly before his present illness developed, he, as well as the medical examiner, found that his urine was free from albumin. It is therefore clear from the history that he had had acute nephritis as a child, and had completely recovered from it, and that his urine was normal up to about October, 1911, when dropsy had developed. Dr. S.'s father and mother and two brothers were perfectly healthy, and he himself was a teetotaller and non-smoker.

He was a spare, pale man, 2 st. 5 lb. under the average weight for his height and age. He suffered from palpitation and shortness of breath on effort, showed slight dropsy and was living, and had been living for some time, upon a rigidly slop diet. His blood-pressure was 140 mm., his heart and arteries were normal; the fundus oculi of each

eye was normal, and he was quite free from any toxic symptoms such as headache, sickness, impaired appetite, defective vision, &c.

Examination of the Urine.—The amount of urine passed in twenty-four hours varied from 50 to 60 oz. Its reaction was acid. The colour was pale yellow. *It was not clear, and showed a fine flocculent deposit,* which was not due to organisms, because a similar specimen, obtained by catheter, was negative on culture. The specific gravity varied from 1014 to 1018. A small percentage of sugar was present (0·3 per cent.). On boiling, the urine became solid with what appeared to be albumin: further acidification did not lead to any solution of the deposit. Hyaline, granular and *fatty casts* were reported by different clinical pathologists. The urine on heating and acidifying not only showed a thick deposit, but also a peculiar gelatinous transformation. The urine was examined on several occasions, and always gave this reaction, though the amount of precipitate varied.

The diagnosis of the case, based on the above history and examination, could be none other than that expressed by several observers who had seen him previously, and several others who saw him on the Continent later on. So far as I know nobody had ever doubted that he was suffering from chronic syphilitic parenchymatous nephritis. Not satisfied with the reaction that I had observed in the urine I placed a sample in the hands of Dr. R. L. Mackenzie Wallis and the following is his report:—

"*Urine.*—Clear; pale yellow colour, with a fine flocculent deposit. Reaction—acid.

"*Albumin.*—Heating: coagulum, insoluble in acetic acid; picric acid, precipitate which does not settle well; trichloracetic acid, precipitate in the cold; acetic acid, 10 c.c. of the urine and 1 drop of glacial acetic acid, a thick gelatinous precipitate, the urine becoming almost solid.

"*Distilled Water Test.*—Distinct white rings form.

"*Dialysis.*—Protein almost completely precipitated. No dialysable peptones present.

"The protein precipitate gives a well-marked phosphorus reaction. In all probability the protein present is of the nature of a lipoid-globulin complex. The marked phosphorus reaction would suggest that the lipoid is lecithin."

Dr. S. had taken many opinions as to his condition, not only in his own State, but in England, in Berlin, and in Frankfurt-on-Main. No observer had ever succeeded in demonstrating a positive Wassermann

reaction, either before treatment with mercury and potassium iodide, or after, nor, as I subsequently heard from him, after provocative or therapeutic doses of salvarsan. It is of interest that much of his wandering in search of medical opinion was due to the fact that he hesitated, in view of the condition of his kidneys, to have salvarsan treatment. The unanimous opinion of all the medical men he had consulted in this country was that he should not have salvarsan. At Frankfurt, however, he was persuaded to have it injected. No change was effected in his symptoms in any way, nor was he immediately upset by the administration, a total dose of 2 grm., nor was the behaviour of the urine altered as the result of the administration of salvarsan—that is, when I last heard of or from him on November 24, 1913. He expressed the opinion "I begin to doubt the specific nature of my condition." Such a remark must be noted, though at the same time, as already stated, no observer ever doubted that the condition was due to syphilis, the secondary stage of which was seen and determined by a well-known authority in London not long after the risk of infection had been run.

Dr. Mackenzie Wallis in the paper he has just read shows that in addition to the well-known forms of so-called "functional" or physiological albuminuria, he has defined another in which globulin, and not albumin, is present alone, or is present in a greater amount than the accompanying albumin: in fact the case I have described has been shown by him to be a case of "globulinuria." It is not permissible to describe my case as one of non-nephritic globulinuria, because the clinical picture—the dropsy, the presence of protein in the urine as well as fatty casts, show that it was a case of pure globulinuria occurring in nephritis. I do not think it is clearly established that the renal affection was due to syphilis.

Dr. Mackenzie Wallis tells me that the unique feature about the urine in this case is that there was so much lecithin in the urine that the lecithin-globulin complex could no longer be held in solution, with the result that the urine when voided was occasionally turbid, recalling the appearance of pseudo-chylous or chyliform effusions which have been found in the various serous chambers of the chest and abdomen. Dr. Mackenzie Wallis has shown that these effusions owe their turbidity to the same excessive amount of the lecithin-globulin complex.

Cases of Non-nephritic Albuminuria.

By W. J. Tyson, M.D.

Case I.—Colonel D., aged 70, seen in 1913. History: Urine tested for the first time in 1905, and constantly up to the time of his death in 1914. Albumin has always been present, but the quantity has varied from a small quantity to absolute solidity. Whenever he had a feverish cold the albumin was increased in quantity. A most careful examination was made in Switzerland in 1907, when the report stated that the urine was acid, specific gravity 1007; no sugar present, no casts; a large quantity of albumin present. He never had any of the ordinary signs or symptoms of Bright's disease; no œdema nor dropsy; no increase of tension; no eye troubles.

Case II.—A. P., aged 76, male, consulted me in 1913. History: For seventeen years has had albuminuria, varying from absolute solidity on boiling to $\frac{1}{10}$ Esbach. Tension not high. Liable to giddy attacks and transient aphasia.

Case III.—Mrs. B., aged 70, seen in consultation in 1911. She was insensible at my visit, and was pulseless. Urine absolutely solid with albumin on examination. In two days patient became sensible, albumin disappeared, and she became otherwise normal. There was no further evidence of Bright's disease.

Notes.—As far as my knowledge and experience go I know of no such similar cases as those mentioned above. The enormous amount of albumin present at times, with practically little at other times, with none of the ordinary symptoms or signs of Bright's disease, seems to me to place these cases in a class by themselves. These patients live a long time, and carry on their life duties as usual. It is said that the presence of a large quantity of albumin connotes kidney disease, but these cases do not seem to support this view. My belief is that the passage of albumin, with or without hyaline and finely granular casts, is a comparatively common condition, especially in people past 50 years of age, and of little or no practical importance. That people reaching middle age have constantly been rendered miserable by the finding of albumin there is no doubt.

